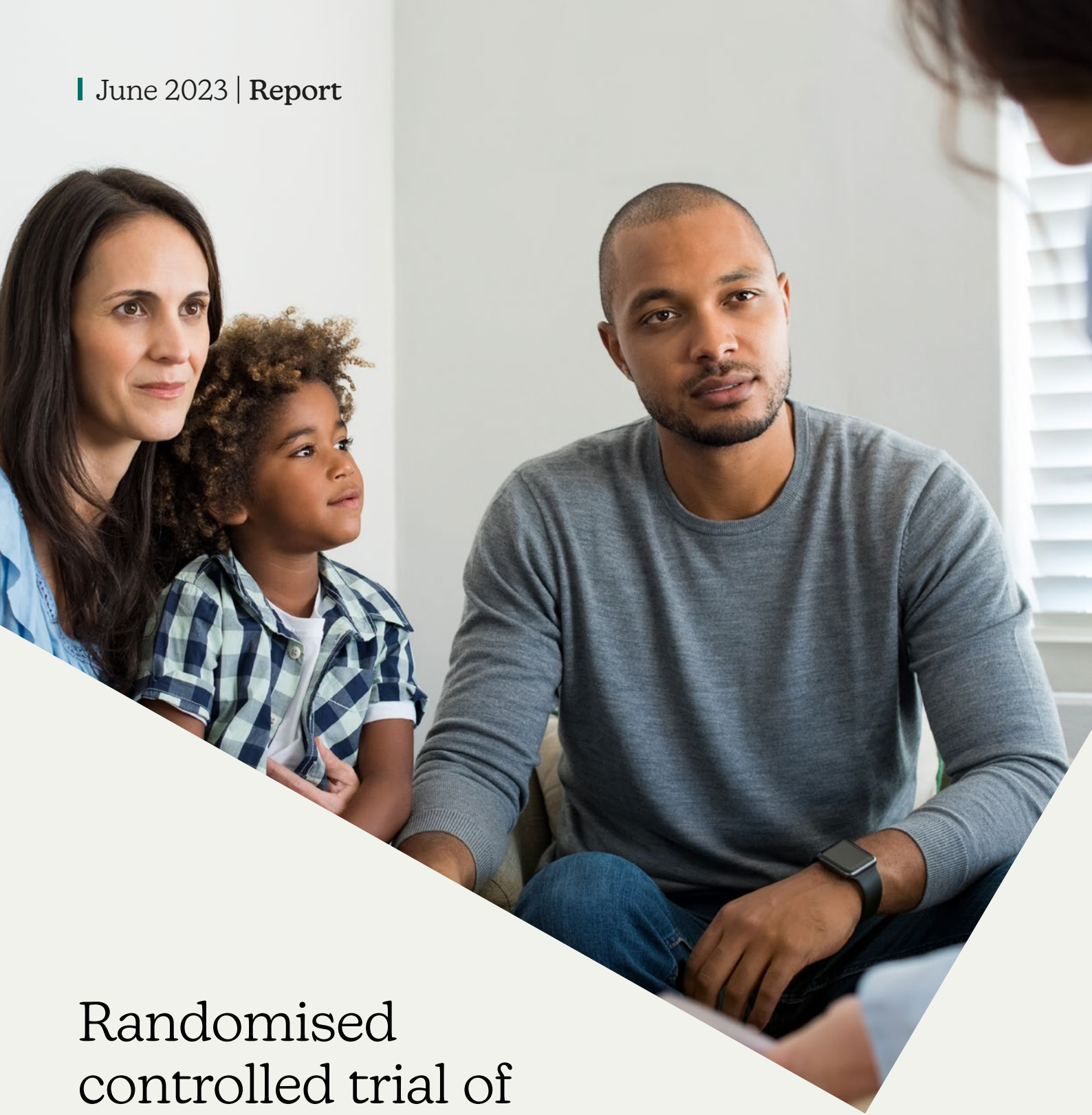


| June 2023 | Report



Randomised  
controlled trial of

# **FAMILY GROUP CONFERENCING AT PRE-PROCEEDINGS STAGE**

**Foundations**

What Works Centre for Children & Families

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# ACKNOWLEDGEMENTS

Thank you to the families who shared their stories with us, and to the staff who completed our surveys. We gratefully acknowledge the advice of our project advisory board members, Professor Ruth Gilbert, Dr Ann Hagell, Andrew Ireland, Professor Peter Marsh, Professor Judith Masson, TK Vincent and HHJ Carol Atkinson. We also acknowledge the advice and contributions of Coram colleagues Allyce Green, Carol Homden, Renuka Jeyarajah-Dent, Hannah Moss, Kevin Yong and others, including former colleagues Emma Borjes, Jo Henderson and Olivia Michelmores. We gratefully acknowledge Professor Richard Dorsett for his advice on and contributions to the data analysis plan. However, this document reflects the views of the authors only.

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## Funding and competing interests

The programme of family group conferences and its evaluation, described in this report, were both grant-funded by What Works for Children's Social Care, and formed part of the Department for Education's Supporting Families: Investing in Practice programme.

## About Foundations - What Works Centre for Children & Families

Foundations is the national What Works Centre for Children & Families. Foundations researches and evaluates the effectiveness of family support services and interventions, and generates the actionable evidence needed to improve them, so more vulnerable children can live safely and happily at home and lead happier, healthier lives.

Foundations was formed through the merger of What Works for Children's Social Care (WWCSC) and the Early Intervention Foundation (EIF) in December 2022.

## About Coram

Coram is the UK's oldest children's charity. Among its services for children, young people and families, Coram does not provide family group conferences. Coram's Impact & Evaluation Team carries out research and evaluation projects in partnership with public sector and third sector organisations, and also works across the Coram group of charities to help teams to evaluate their effectiveness.

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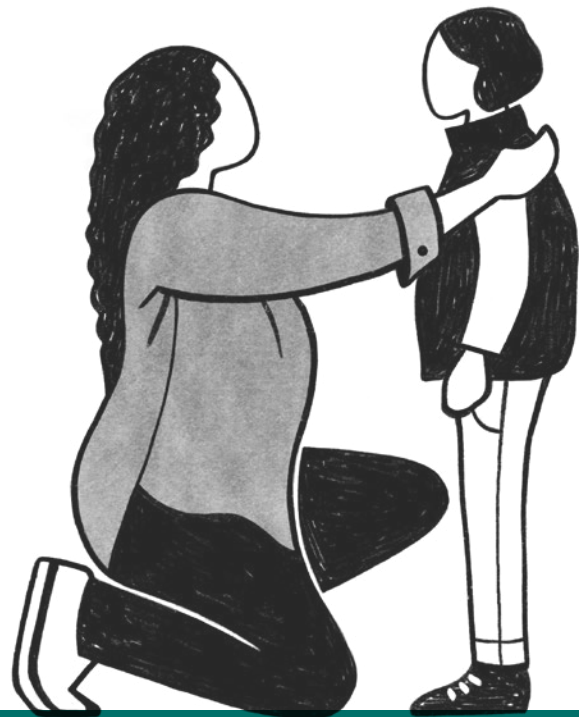
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# EXECUTIVE SUMMARY

This report describes a randomised controlled trial (RCT) carried out on the use of family group conferencing (FGCs) at pre-proceedings stage in England. We assessed the impact on child and parental outcomes as they went into pre-proceedings, comparing families referred for a family group conference with those not referred.

Families were randomly allocated to be offered an FGC (or not) in addition to usual services. The study ran between September 2020 and May 2022 in 21 local authorities in England, and we analysed data on 2548 children in 1471 families. We found that FGCs were cost effective, with children in referred families significantly less likely to go into care. Twelve months after the pre-proceedings letter, children whose families were referred for an FGC were 8.6 percentage points less likely to go into care. Among children in families referred for FGCs, 36.2% were in care; among children in families not referred for FGCs, 44.8% were in care.



# Introduction and background

Family group conferences (FGCs) are a type of decision-making used in children's social care in the UK and internationally, involving meetings led by family members to plan and make decisions for a child who is at risk of harm or abuse. FGCs are hypothesised to improve outcomes through several mechanisms. These include quicker processes, clearer information, more even power-sharing and greater trust between families and professionals.

After a delay due to COVID-19, with staggered starts, 21 local authorities in England introduced a standard model of FGCs for families entering “pre-proceedings”. They randomised families, then referred intervention group families for an FGC. FGC referrals were made for an average of 13 months in the 21-month window from September 2020 to May 2022. Families randomised to the control group received usual services. This was part of the Department for Education's Supporting Families: Investing in Practice programme. It built on work in Southwark and Wiltshire by the charity Daybreak as part of the Children's Social Care Innovation Programme.

A total of 1511 families were randomised, of whom half (739) were assigned to be offered an FGC. In total, 694 children aged 0 to 17 years in 376 families received an FGC, of whom 647 children in 351 families were in the intervention arm. The FGCs took place between October 2020 and June 2022. A substantial minority (29%) of families randomised to the intervention group were not referred to the FGC service by the local authority. Among families who were referred, 67% received an FGC.

This was the first randomised controlled trial (RCT) of FGCs to be carried out in the UK and the largest RCT of FGCs carried out in the world. This evaluation by Coram aimed to estimate the impact of referral for FGCs on child outcomes in the weeks and months (up to 18 months) after entering pre-proceedings, relative to usual services, which may include other kinds of meetings such as family network meetings. For randomised families, we examined whether children became looked-after, how inclusive their birth parents perceived their interactions with their local authority to be, whether court proceedings were issued, time spent in care and whether children's living arrangements remained the same or changed. Where known, most of the FGCs (77%) were delivered in-house by a dedicated FGC team, with 23% of FGCs (in 5 local authorities) delivered by Barnardo's or Daybreak.

Local authorities in the programme may not have been typical; they volunteered to take part on the basis that they were not already offering FGCs at pre-proceedings stage. As none of the 21 local authorities involved in the evaluation were offering FGCs at pre-proceedings stage, it is unlikely that the offer of FGCs would have happened without this programme. Families in the intervention group were offered an FGC, but participation in an FGC was voluntary.

# Research questions

We were interested in finding out the effect of referring families for family group conferencing as they went into pre-proceedings in local authorities in England. We asked five questions about the impact of FGCs. When families are referred to have an FGC at pre-proceedings:

- **Are children more likely to go into care 12 months after the pre-proceedings letter** (primary outcome)?
- **Do parents feel more or less involved in the planning of their children’s care**, as assessed by parents on a four-point scale, around eight weeks after randomisation (perceived inclusiveness)?
- **Are children more or less likely to stay in the same living arrangement** between the date court proceedings are issued (or date of the letter informing families that their local authority will not pursue court proceedings) and a date 6, 12 or 18 months later (sustainment of outcome)?
- **Do children spend more time in care**, looking at the number of days children spend as looked-after, in the 6, 12 or 18 months after the pre-proceedings letter (time spent in care)?
- **Are court proceedings asking a court to take a child into care more or less likely to be issued** by a date 6, 12 or 18 months after the pre-proceedings letter (court diversion)?

Our process evaluation aimed to support the RCT by providing evidence to explain the reasons for the effects of FGCs. Our cost evaluation aimed to establish the cost of the programme and whether FGCs are cost-effective. This involved a request for costs data sent to all local authorities, Daybreak and Barnardo’s.

## Design and sample

This evaluation comprised a non-blinded parallel trial with an intention-to-treat design, with rolling 1:1 randomisation of 1511 families to two arms, plus a process evaluation and cost evaluation.

Families were randomised by local authorities using an independent online platform after the decision was made for families to enter pre-proceedings. Families received pre-proceedings letters approximately one week later.

We analysed data on 2548 children in 1471 families in 21 local authorities, compared with a planned 6000 children in 3300 families in 24 local authorities. Children could be of any age, including children born after the family entered pre-proceedings, and almost all families entering pre-proceedings were eligible for randomisation. The only exclusion criterion was families already randomised in the course of the evaluation.

Most outcomes data was administrative data collected from local authorities in three waves (in March 2021, September 2021 and June 2022), but we also analysed responses to text messages we sent to parents between November 2020 and July 2022, asking them

how included they had felt in planning their child's care. We asked local authorities for mobile phone numbers for all randomised parents, where available, and received 1256. Across 17 local authorities, 180 parents replied to the 746 texts delivered (24%). We also drew on data gathered by Daybreak on FGC characteristics and attendees.

For our process evaluation we surveyed each local authority and 44 FGC coordinators across all local authorities, and interviewed 9 family members and 16 social workers in 4 local authorities.

The 21 local authorities that took part in the programme and evaluation were: Bath and North East Somerset, Birmingham, Bromley, Derbyshire, Knowsley, Lambeth, Lancashire, Leicestershire, Lewisham, Middlesbrough, North East Lincolnshire, Northamptonshire, Nottingham City, Plymouth, Redcar and Cleveland, Rotherham, Salford, Sheffield, Shropshire, Southampton and Sunderland. Three more were originally due to take part. Merton and Blackpool dropped out before the originally planned programme start date, 1 April 2020, and Staffordshire did not start referrals and confirmed it had dropped out in April 2021.

## Findings

### Impact evaluation findings

We found that children whose families were referred for an FGC were significantly less likely to be in care 12 months after entering pre-proceedings. Based on analysis of data for 1227 children (643 in the intervention arm, 584 in the control arm), children whose families were referred for an FGC were 8.6 percentage points less likely to go into care. Among children in families referred for FGCs, 36.2% were in care, whereas of children in families not referred for FGCs, 44.8% were in care: children in the intervention arm were 0.81 times less likely to be looked-after, compared with children in the control arm. Conversely, control group children were 1.24 times more likely than intervention group children to be looked-after. This was statistically significant when adjusted for previous care history ( $p=0.01$ ).

We also found statistically significant differences between trial arms, after adjusting for multiple tests, for three of our secondary outcomes:

- Care status at 6 months: children in the intervention arm were 0.79 times less likely to be looked-after than children in the control arm (33% compared with 42%)
- Court diversion: children in the intervention arm were 0.82 times less likely to have care proceedings issued than control arm children by the end of the reporting period (59% compared with 72%)
- Time spent in care at 6 months: was significantly lower among children whose families had been referred for an FGC than for children whose families had not been referred (87 days compared with 115 days), with an effect size of 0.19 (Glass's delta).

However, there was no significant difference in whether a child's living arrangement (for example, with relatives or in foster care) remained the same or changed in the months after the decision on whether to pursue court proceedings. This was probably due to the sample size for this outcome being small, because the follow-up period started later: from the date court proceedings were issued or stepped down, not the date of the pre-proceedings letter. None of our outcomes were significant at 18 months, probably because samples of children were by then too small to be sufficiently powered for statistically significant findings.

We also wanted to report on parents' perceived level of inclusion in the process, but the response rate to text messages was too low for a reliable breakdown across the intervention and control groups. At around two months into pre-proceedings, we asked "How involved have you been in planning your child(ren)'s care in the last two months?" Though respondents are likely to have been unrepresentative, overall, it was positive to see that 53% of parents, whether or not they were referred for an FGC, replied "completely" or "very" rather than "slightly" or "not at all" to the question.

The FGCs were originally envisaged as almost entirely in-person events, but COVID-19 meant most were delivered virtually (41%) or as hybrid virtual/in-person (19%), rather than in-person (40%). Exploratory analysis showed that hybrid FGCs were better attended than the other two formats. We found some differences in outcomes, which may favour in-person and hybrid formats over virtual FGCs and merit further exploration in future research. Of all 2167 family network member attendees, 47% attended in person and 53% attended by phone or video call. COVID-19 also reduced the number of families in the programme, by delaying the programme start and reducing the number of families entering pre-proceedings.

## Cost evaluation findings

Our analysis suggests that the FGCs at pre-proceedings stage in this programme were cost-effective – that is, they reduced costs for local authorities by lowering the likelihood that children entering pre-proceedings would go into care.

We estimated the total costs to all 21 local authorities and the 2 outsourced providers of taking part in the first year was £2,780,560. Unit costs and the scale of FGC services varied widely across local authorities, but overall we calculated a unit cost of £8911 per FGC delivered or £5242 per child. This unit cost applies to this programme, not the use of FGCs at the pre-proceedings stage in general, for which no benchmark costs are available, to our knowledge. One evaluation (Mason et al., 2017) found that expanding FGCs for more families in Leeds in 2015/16 cost £2418 per FGC. The costs in this study were inflated by including start-up costs. Our RCT design meant FGCs were not offered to all families, reducing FGC services' ability to achieve economies of scale. COVID-19 also disrupted delivery, reducing the number of FGCs delivered and thus increasing cost per FGC. During planning of the programme, a unit cost of £2300 per FGC was assumed. Daybreak told us they thought this would be closer to the actual cost of delivering FGCs at pre-proceedings stage in the absence of the factors listed above.

Despite these factors increasing the FGC unit cost, we still found that FGCs were cost-effective. We estimated the average net cost or benefit per child to a local authority in 2021 prices by referring the family of a child entering pre-proceedings for an FGC. This was a net benefit of £960 (£6202 minus £5242) per child in the first year. This is a benefit-cost ratio of 1.18:1 (£6202 benefit divided by £5242 cost). This evaluation was not designed to collect the cost of usual practice in working with families during pre-proceedings, but it is not zero. This missing information would help with interpreting the cost-benefit of FGCs we have calculated. If the FGC unit cost were lower (costs in this study were inflated by the factors outlined above), the net benefit would increase.

## Process evaluation findings

Our process evaluation showed the difficulty and complexity of problems facing families during pre-proceedings. Despite these problems and histories of previous child removals, we saw examples of families making progress and finding solutions. We also saw the difficulty, complexity and sometimes the intensity of responses to families' problems, with some receiving daily visits, residential placements and a wide range of services involved.

Pre-proceedings support to families was viewed both positively (as supportive or life-changing) and negatively (as judgemental or intrusive). In this context, among our case study families, none felt the FGC had made a major difference to outcomes. FGCs were only one factor in a complex system of problems and service responses. Some FGCs were of poor quality, and descriptions of some other family meetings held for control group families were FGC-like. However, most local authorities told us they thought introducing FGCs at pre-proceedings had made a difference to how they worked with families.

COVID-19 proved the biggest challenge to implementation, with not all virtual and hybrid FGCs viewed by Daybreak as adhering to the original FGC model, some technological issues and concerns about participant safety and risk of coercion. COVID-19 aside, fidelity of the model and adherence to standards of good practice in delivering FGCs were generally high. Staff were trained to the standard set by Daybreak. We learned of only occasional poor practice, such as social workers (rather than independent facilitators) running FGCs, or families not being given private family time as part of the meeting.

## Conclusions

The care pre-proceedings process is a difficult time for any family. This evaluation sought to provide evidence on how to support these families. It drew on the hard work of many hundreds of family members and professionals who took part in the programme. It provides evidence on one of the ways that local authorities can fulfil their statutory obligation to ascertain the wishes and feelings of relevant people. Given our findings on the likelihood of going into care and court diversion, it provides evidence in favour of FGCs as a way to do this cost-effectively, reducing the chances of children going into care. Given our encouraging finding on court diversion, savings are also likely for the justice system, though we did not include these in our cost evaluation.

This is only one study. A recent review of evidence on shared decision-making family meetings did not find a reduction in entry to care internationally (Nurmatov et al., 2020). However, as the first RCT in England looking at FGCs, and the largest RCT in this field, our findings are encouraging, both about the impact of FGCs at the pre-proceedings stage and in showing that it is possible to successfully carry out a large-scale RCT in children's social care. Pre-proceedings is distinct from other stages (such as Child in Need and Early Help), and this evaluation only provides evidence on referring families for FGCs at pre-proceedings, not FGCs in general. We recommend future impact evaluation of FGCs at other stages, especially earlier stages, given the belief that was expressed to us of the potential preventative benefits of FGCs.

Our sample of local authorities covered the geographic spread of England, but the time period we studied was unusual. Like so many evaluations of programmes introduced since March 2020, interpretation of our findings is complicated by COVID-19. We did not evaluate the usual FGC model, and it is unclear how much online FGC delivery will remain in future. In more than half of the local authorities, parents were also being asked to attend other meetings during pre-proceedings. Our counterfactual was usual services offered to families by local authorities during pre-proceedings, but these will have varied in quality and quantity across areas. Compared with these usual services, FGCs are intended to be different, with their different attendees, independent facilitators and venues, refreshments and family-only time. Any of these differences, or other factors such as social workers' belief in the power of FGCs, may help to explain our findings.

FGCs aim to improve decision-making rather than achieve particular outcomes. We tried to gather parents' perceptions of their involvement, but too few local authorities provided us with parents' mobile phone numbers and, where they were available, too few parents replied to our text messages for a reliable analysis of the impact of FGCs on perceived inclusion.

We would expect an improved pre-proceedings process to affect a range of outcomes. So in 2019 we chose looked-after status as our main outcome, because whether a child is in care is important in itself and is an important influence on many other child outcomes. However, none of our analysis allows us to conclude definitively whether children are safer or happier if their families are offered FGCs, or whether they are associated with harms. Further research is needed to understand what, if any, impact they have on children's wellbeing more generally.

FGCs are in widespread use (Wood et al., 2022), but were not previously used at pre-proceedings stage in the 21 local authorities that took part in this programme. As a result, families were not deprived of anything they would otherwise have received. Despite some initial concerns, this large-scale RCT was implemented successfully in children's services settings, where this evaluation design is not commonplace. There were only occasional instances of non-compliance with the protocol (such as social workers failing to refer intervention group families for FGCs and helping to arrange FGCs for control group families). We have set a precedent to build on and demonstrated the feasibility of further impact evaluations of this kind in this sector.

By number of families, this trial was by far the largest of FGCs in the world, based on the findings of the Nurmatov et al. (2020) systematic review. We succeeded in gaining administrative data from all 21 local authorities on almost all randomised families. However, despite the clear definition and importance of looked-after status, the data was often difficult for officials to extract for us, and much of the data we received was patchy and incomplete. These data issues would prove a major limitation in anything other than an RCT. In an RCT, the noise and error are expected to be equal across both trial arms. However, our data cleaning and imputations may have wrongly increased the level of change or continuity in children's lives. The low number of children available for analysis may help explain where our findings are not statistically significant.

This study has found evidence that FGCs had a positive impact: children in families referred for FGCs were less likely to go into care in the months after entering pre-proceedings. While we cannot be sure about the validity of the overall picture in which this difference is seen, and we do not know what happens next in children's lives, this study presents important new evidence on the impact of FGCs at pre-proceedings and their positive role in reducing entry to care.

## Recommendations

### **For future policy and practice, we recommend:**

- Local authorities should continue the use of FGCs at pre-proceedings stage where services already exist, and consider their introduction in other areas
- The Department for Education, local authorities and partners should consider the findings of this study in their response to the Independent Review of Children's Social Care
- Local authorities should ensure good support is available during pre-proceedings from social workers with positive relationships with families
- Local authorities should improve their data on usual services during pre-proceedings and on FGCs, so that further research can increase understanding about how they work, for whom and in what context
- Local authorities should provide high-quality support to FGC services, including good internal communication, referral mechanisms and senior management buy-in
- FGC services should be delivered consistently and to high standards, including using independent facilitators and providing families with private time during the meeting
- FGC services should seek ways to maximise effective working with social workers, because high workloads and turnover were seen as barriers to implementation.

## **We recommend future research on:**

- Where, when, how and how many FGCs are used with families in England
- The impact of FGCs at different stages, especially earlier stages such as for children on Child Protection Plans; and of different types of FGCs: in-person, virtual and hybrid FGCs; and of in-house and externally commissioned FGCs. Any future RCT should mitigate the risk of attrition between randomisation and referral by automatically informing the FGC service each time a family is randomised to the intervention group. It should minimise the data burden by avoiding duplication between the evaluator and delivery partner's monitoring requests. Future evaluations should gather data on the nature and intensity of usual services for families
- The costs of FGCs and of usual support during pre-proceedings, including the relative costs of in-person, virtual and hybrid FGCs; and of in-house and externally commissioned FGCs
- The actual and perceived level of involvement in the process by children and wider family
- Any impacts of FGCs on the wider family – our outcomes looked only at impacts on the immediate family – and other impacts on children, such as wellbeing
- The mediators and moderators of the differences we have found between trial arms, which may include, for example, the level of support from the wider family, and the quality of the relationship with the social worker.



# INTRODUCTION

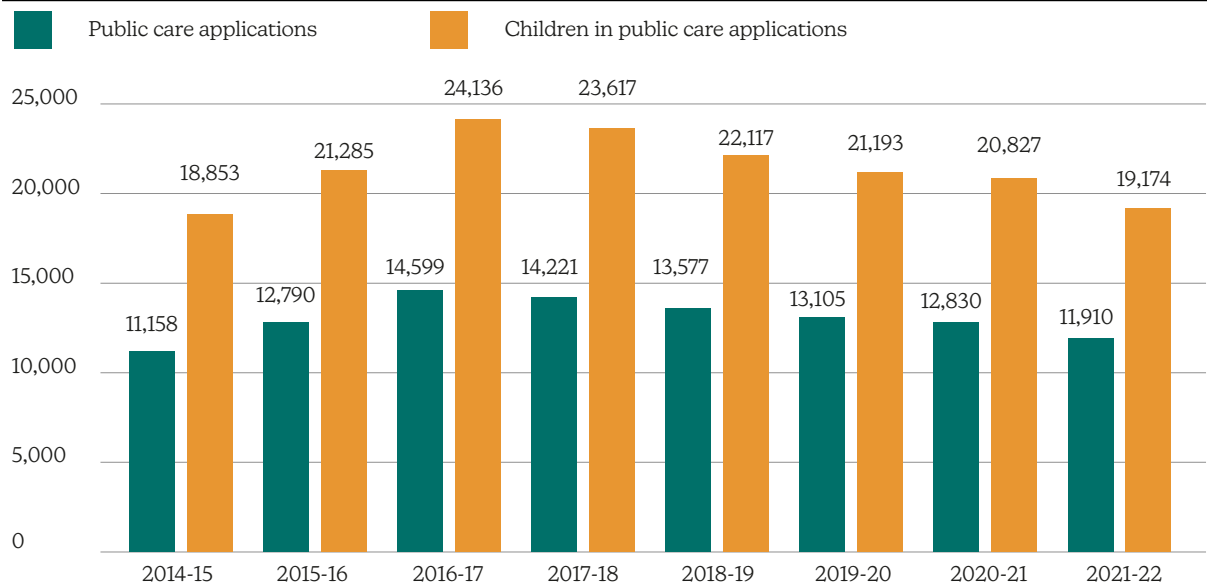
# Background

## About care pre-proceedings

At pre-proceedings stage, a child is already known to the local authority. The parents (usually, but sometimes other family members) have parental responsibility, rather than the local authority. The local authority’s concerns are usually about abuse or neglect. The child may be a Child in Need (section 17 of the Children Act 1989) or subject to a Child Protection Plan (section 47 of the Children Act 1989). Some children who are looked-after under voluntary arrangements (section 20 of the Children Act 1989) may also be the subject of pre-proceedings. In response to new information, or a gradually emerging picture of the high level of risk, a local authority uses pre-proceedings, and then care proceedings, to escalate a child’s status from, usually, a Child Protection Plan to being a looked-after child, and gains a share of parental responsibility. Children who become looked-after in emergency situations bypass the pre-proceedings stage.

No national data is published on how many families enter pre-proceedings, but there has been a recent decline in the number of public law applications received by Cafcass (the [Children and Family Court Advisory and Support Service](#)) when local authorities initiate care proceedings (Figure 1). The 2021/22 number was 21% lower than the peak in 2017/18.

**Figure 1. Public care law applications received by Cafcass, England, 2014/15 to 2021/22**



Source: Coram analysis of published Cafcass (Children and Family Court Advisory and Support Service) data (Cafcass, 2022)

The pre-proceedings stage begins when a local authority's Legal Gateway Panel or Legal Planning Meeting concludes that the local authority should issue a pre-proceedings letter to the parents, or adults with parental responsibility, of a child or children. The letter states that the local authority will bring care proceedings – that is, seek to take the child or children into care by seeking a Care Order from a court,<sup>1</sup> if the parents or others do not take specific actions. Pre-proceedings letters can be issued antenatally, though court proceedings themselves cannot begin until birth. Pre-proceedings letters can be issued at any point until the youngest child in a family reaches the age of 17. Those with parental responsibility are invited to a meeting where, usually accompanied by a legal aid solicitor, the local authority reiterates its concerns. Social workers share information about concerns about the child and work done to support the family and reduce the risk of harm. The extent and nature of this information varies. The pre-proceedings stage can be terminated by a local authority if the parent(s) or others make positive changes, or for other reasons, such as a family agreeing to the children being looked after by alternative carers. The pre-proceedings stage usually ends in one of two ways. Either proceedings are issued – that is, the local authority applies to the court; or the local authority writes to the parent(s) or others to say it no longer considers care proceedings necessary, and “steps down” the case.

The government aims for court proceedings to last no longer than six months from the date of the Care Order application. Published Ministry of Justice family court data for England and Wales shows it took 33 weeks on average to complete care proceedings in 2019, when this evaluation began, increasing to 39 and 45 weeks in 2020 and 2021 respectively, and 49 weeks in the first quarter of 2022 (Ministry of Justice, 2022).<sup>2</sup>

The pre-proceedings stage is sometimes described as “PLO” or Public Law Outline stage. This refers to Ministry of Justice Practice Direction 12A in the Family Procedural Rules first published in 2010 (Ministry of Justice, 2021). Under statutory guidance, local authorities are required, during pre-proceedings, to enable wider family members to contribute to decision-making.

The pre-proceedings checklist includes a record of key discussions with the family, which could include a family plan arising out of an FGC. The record is not required to be filed by local authorities when proceedings are issued as part of the pre-proceedings checklist (Family Procedural Rules, 2017). The record is only required to be disclosed by request.

## About family group conferencing

Family group conferences (FGCs) are meetings led by family members to plan and make decisions for a child who is at risk (Family Rights Group, n.d.a). FGCs can also be used for adults. A recent survey found 79% of local authorities in the UK had an FGC service, with coverage becoming more common from 2016/17 (Wood et al., 2022). FGCs originated in New Zealand. Their development was a response to the disproportionate number of Māori children removed into state institutions.

1 A local authority may apply for other kinds of orders from the court instead or as well, including Supervision Orders.

2 See <https://www.gov.uk/government/statistics/family-court-statistics-quarterly-january-to-march-2022>

Government guidance states that local authorities “should consider referring a family to a family group conference service if they believe there is a possibility the child may not be able to remain with their parents, or in any event before a child becomes looked-after, unless this would be a risk to the child” (Department for Education, 2014). Nearly a third of the local authority respondents to Wood et al. (2022) reported having a written policy that all children/families should be offered an FGC before a child comes into care. However, there is no legal requirement for an FGC. There is neither a right to an FGC nor a responsibility on local authorities to provide FGCs. The Independent Review of Children’s Social Care (MacAlister, 2022) writes positively about family group decision-making and Family Network Plans. The review recommended a new legal entitlement to family group decision-making before a case reaches the PLO stage, “so that a family-led alternative plan for taking care of the child can be considered before a Care Order is presented by the local authority to the court” (MacAlister, 2022: 99).

## How are FGCs used?

No routine national data is published on the use of FGCs in England. In England and Wales, three-quarters of local authorities are reported to run or commission FGCs for children in their area or to be planning to do so (Family Rights Group, n.d.a). Wood et al. (2022: 4)’s UK survey found FGCs were mostly likely to be offered by local authorities at either pre-care proceedings (96%) or when the child is being considered for a Child Protection Plan (96%), and 84% of local authorities offered FGCs for reunification planning.

Harwin et al. (2019) in a case file study found that FGCs were held for 37% of 107 children placed with special guardian families. In another study of care proceedings, Masson et al. (2019) found 79 cases (39% of the sample of care proceedings) were known to have had an FGC or family meeting take place.

FGCs are reported to be used in approximately 30 countries worldwide and 22 countries in Europe<sup>3</sup> (Family Rights Group, n.d.b). There is a legal requirement in countries such as Ireland, the Netherlands and New Zealand for all families in youth care to be offered the opportunity to make their own family group plan, for example through an FGC (Dijkstra et al., 2019).

The FGC meeting itself is short, at around three hours in length. A typical meeting takes place in a neutral location, not the family home or a local authority office (Family Rights Group, n.d.a; Early Intervention Foundation, 2018). The FGC coordinator convenes a meeting with family members, which may or may not include the child or children themselves, to plan and make decisions for a child or children. The coordinator should not be the family’s social worker. They explain the process, share information and seek to motivate people to attend. As well as the immediate family, others may also attend, including extended family, friends, neighbours, advocates, carers, interpreters and professionals. During the conference, the aim is to create a plan to keep the children safe, agreed by all involved. This is usually a single plan for all the children in the family. It may or may not be adopted by social workers as the plan for the child or children’s future care.

3 The Family Rights Group (n.d.a) listed the following countries as using FGCs (mostly in child welfare): Austria, Australia, Canada, Finland, Germany, Holland, Hungary, Ireland, Israel, Italy, Japan, New Zealand, Norway, Poland, Serbia, Slovakia, South Africa, Sri Lanka, South Africa, the USA, England, Wales, Scotland and Northern Ireland.

FGCs can also be called:

- Family group decision-making (FGDM)
- Family unit meeting (FUM)
- Family decision meeting (FDM)
- Team decision-making meeting (TDM)
- Family team meeting (FTM) (Early Intervention Foundation, 2018; Dijkstra et al., 2019).

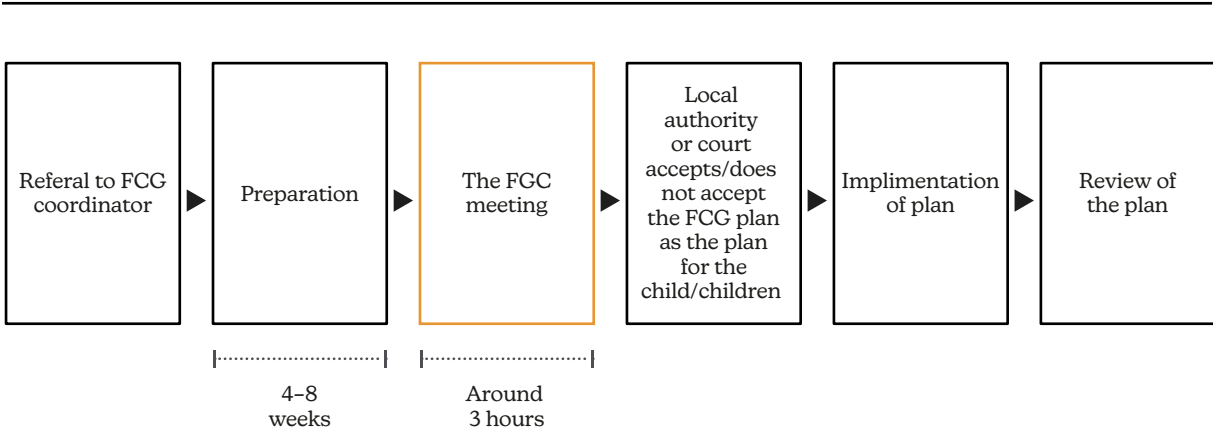
In some cases these are different names for the same model of decision-making, and in some cases they refer to similar but different models. Some may be used to refer to more agency-driven engagement practices (such as TDM or FTM) than FGCs, which are sometimes distinguished from other practice by the involvement of an independent facilitator (Marcynyszyn et al., 2012).

There are typically five stages of an FGC: the referral, preparation, the conference, implementation of the plan and review of the plan (Family Rights Group, n.d.a).

### The FGC model used in this programme

The model of FGC in this programme is similar to that used in other local authorities and offered by other providers (Figure 2). The FGCs were hypothesised to improve outcomes via several mechanisms. As set out in the logic model, these include more even power-sharing, clearer information, quicker processes and greater trust (Appendix 1).

Figure 2. Stages and timings of FGCs



Daybreak describes the model of intervention in this programme as involving:

- The mandatory offer of an FGC at the point of pre-proceedings letter (no social worker discretion to refer or not refer a family to the FGC service)
- A 20-day referral target once the family is identified at Legal Gateway Panel
- A 15-day conference target once the referral is received by the FGC service
- Four core questions to discuss in the FGC meeting (see below)
- The family is offered the opportunity to review and fine tune their plan.

At the start of the programme, the local authorities' delivery partner, Daybreak, set out to:

- Provide all materials, practical support and consultation to local authorities throughout set-up and implementation of the FGC model offered in the pre-proceedings letter
- Provide training for the operational management team, children and families teams and FGC coordinators (both existing, and to help with recruitment and training of additional staff)
- Facilitate regional and/or national learning and practice-sharing forums for participating local authorities
- Provide ongoing monitoring reports and audits as feedback to participating sites, and collaborate with evaluators to identify FGC outcomes
- Present and promote the use of this Daybreak FGC model to improve positive outcomes for children and families as well as making savings for local authority budgets.

Daybreak delivered initial training in each local authority, mostly completed before March 2020, covering FGCs and programme processes, target timescales and their monthly spreadsheet to track referrals and delivery. The training was open to everyone within the local authority who was going to be involved, including decision-makers and social workers. In addition to the initial sessions, Daybreak provided an ongoing training offer for new staff. This offer came to an end in January 2022.

Daybreak also made a range of tools and templates available to local authorities, including an FGC guide, electronic documents and a flow diagram setting out who was responsible for each task throughout the process. The monitoring tools, such as the monthly spreadsheet, regular check-ins with local authorities and a six-month audit, were intended to help Daybreak identify and address potential challenges.

The FGC model in this programme encouraged the use of four core questions for discussion in each FGC meeting:

1. (Compulsory) What support can the family network provide to help ensure that the child/ren can live in a sustainable, safe and well-cared-for environment with parent/s?
2. (Compulsory) If assessments show that it is not possible for child/ren to live with (parent/s), who, within the wider family network, is willing to put themselves forward to provide suitable, safe and sustainable care for the child/ren? What support can others in the family network offer to those putting themselves forward to care for the child/ren?
3. (Optional) If the family cannot offer suitable care for the child/ren, what contact would the family network like with the child/ren if it is possible?
4. (Optional) If siblings are to move from their family home and cannot live together, how can the family network ensure that the siblings remain in contact with each other if this is possible?

The questions could be re-written to be personalised to each family but it was stressed that they must not lose their clarity and meaning.

In Daybreak's model, FGC meetings start with a first session of information-sharing with all those present (family and professionals), and then private family time to discuss what has been said and agree on a family plan if possible.

The charity Family Rights Group has an accreditation scheme for FGC services. Local authorities taking part in the programme were encouraged to seek accreditation, if they did not already have this (e.g. due to already delivering FGCs at stages other than pre-proceedings). The scheme's seven standards and beliefs, which can be used as a quality benchmark, are:

1. The FGC coordinator is independent
2. The FGC should respect the family's consent to proceed
3. The FGC should be family-led and include "private time" for the family to make a plan in response to concerns
4. The central focus should be the child or adult who is the subject of the FGC, and they should be offered support in their involvement, including an advocate
5. The FGC service should ensure that the family has all necessary resources, including adequate preparation, relevant information and a safe and appropriate environment to make its plan
6. The FGC should respect the family's privacy and right to confidentiality
7. The FGC should be sensitive to the family's culture, taking account of ethnicity, language and religion.

What Works for Children's Social Care told us in August 2022 that 5 local authorities out of the 21 in the programme had gained accreditation, and 3 were planning this, though over half had not responded to the query.

## **The evidence base for FGCs**

The programme evaluated in this report originated in the Department for Education's Children's Social Care Innovation Programme (2014/20). Through this, the Thomas Coram Research Institute (Munro et al., 2017) evaluated work in the London Borough of Southwark and Wiltshire County Council by the charity Daybreak. The evaluation found promising findings on outcomes for FGCs used at pre-proceedings stage. However, it did not have a robust comparison group. It found that, 3 to 12 months after FGCs, around three-quarters of children were living with a parent (n=83, 60%) or a relative (n= 22, 16%). For children where no FGC took place, 61% lived with parents (n=22, 50%) or relatives (n=5, 11%). During the study timeframe of 2015/16, proceedings were initiated in 29% of FGC cases and 50% of non-FGC cases. Lawrence et al. (2020) followed up the 2015/16 cohort in 2019 and found the pattern to be maintained, with 71% of children who had an FGC living with family, compared with 43% who had no FGC.

Other Innovation Programme round one projects included the use of FGCs. One of these was the Leeds Family Valued programme, which oversaw the expansion of FGCs to more families, including those affected by domestic violence and with a new offer for child protection. The programme evaluation (Mason et al., 2017) found statistically significant reductions in: number of looked-after children; rate of looked-after children per 10,000 population; number of Child Protection Plans; and number of Children in Need. It also found that families who participated in an FGC felt more involved in the process and that their values had been respected (100% of families interviewed, n=54). Nearly all

also felt their FGC had helped address their problems and felt the services offered were appropriate to their needs (99% and 91% respectively).

The Leeds Family Valued and North East Lincolnshire's Creating Strong Communities programme evaluation also reported reductions in court proceedings in relation to FGCs (Sebba et al., 2017).

## **Previous randomised controlled trials of FGCs**

There are few examples of RCTs of children's social care programmes or interventions in England, and none cover FGCs (Baginsky et al., 2017). Nurmatov et al. (2020) carried out a systematic review of studies comparing family group meetings to control group services. They found studies of provision in the USA, Netherlands and Sweden, as well as Munro et al. (2017) and Mason et al. (2017) covering England. The largest previous FGC RCT cited was Perry et al. (2013) in the USA, with 677 families involved. Nurmatov et al. (2020) concluded that the evidence base was of poor quality, with few robust comparison groups. They found no RCTs on shared family decision-making meetings that identified a reduction of entry or re-entry to care, referrals or re-referrals for maltreatment, or increased satisfaction, empowerment or reunification with families, compared with control services. Thus, they concluded that, overall, evidence of effectiveness was weak.

As such we began the RCT from a position of equipoise, holding no prior view on whether FGCs at pre-proceedings stage had an impact on child outcomes.

## **Past and future provision of FGCs in the local authorities taking part in the programme**

Local authorities volunteered to join the programme. We reviewed descriptions of usual practice from their June 2019 programme application forms. In the trial protocol we assigned these into categories:

- No FGCs: no FGC service is offered at all
- New: FGC service is newly established
- Sporadic: FGCs sporadic/uneven/not routinely offered
- Embedded: FGC service is well-established (but not at pre-proceedings stage).

We then categorised again based on their responses to the baseline survey and more recent data at the end of the programme (see Appendix 2 for a breakdown by local authority). The number of local authorities in the embedded category increased from 8 in 2019 to 18 most recently (Table 1).

Table 1. Use of FGCs by local authorities in the programme

Category	Pre-programme category	Category at start of programme	Category at end of programme
No FGCs	0	0	2
New	9	9	0
Sporadic	4	2	1
Embedded	8	10	18
<b>Total local authorities</b>	<b>21</b>	<b>21</b>	<b>21</b>

Note: pre-programme category is based on programme application forms. Category at start of programme is based on responses to baseline survey, October 2020 to July 2021. Category at end of programme is based on responses to follow-up survey (September to December 2021), data provided to us by the Department for Education (December 2021) and Daybreak data (March 2022).

Data provided to us by the Department for Education (on 19 of the 21 local authorities in the programme) shows that the majority offered FGCs before the programme (13 out of 19, 68%). This is broadly similar to our categorisation based on June 2019 information – FGCs were “embedded” in 8 authorities, “sporadic” in 4 and “new” in 9 (Table 1). We categorised the six that Daybreak found were not offering FGCs before the trial as “new”. However, we also categorised three local authorities as “new” that Daybreak reported were offering FGCs before the trial.

Department for Education data (on all 21 local authorities from December 2021) and Daybreak data (on 19 from March 2022) indicates that, where plans were known, all will continue to offer FGCs after the programme other than Derbyshire and Leicestershire. Some planned FGCs across children’s social care, others at Public Law Outline stage only.

In our surveys of local authorities, 13 mentioned meeting with the family as part of usual practice during pre-proceedings, while 8 did not. We know from our interviews that these meetings included family network meetings in at least some cases.

Our baseline survey asked local authorities what they wished to achieve by taking part in the programme. All local authorities referred to at least 1 of the following: enhancing the service offered at pre-proceedings; evidencing the impact of FGCs to inform practice; or improving outcomes (see Appendix 3 for more details).



# METHODS

# Programme & evaluation timeline

The key dates for the programme, and of our fieldwork, are shown in Table 2. Full details on the method are available in the trial protocol (Taylor et al., 2020).

**Table 2. Programme and evaluation timeline**

Date	Event
June 2019	Local authorities apply to join the programme, on the basis that they are not currently delivering FGCs at pre-proceedings stage.
November 2019	Daybreak begins visiting local authorities.
January 2020	Research ethics approval for the RCT.
February 2020	Daybreak begins delivering training to local authority FGC services.
March 2020	Trial protocol published.
April 2020	Original planned start date, postponed due to COVID-19.
July 2020	Lifting of some COVID-19 restrictions.
September 2020	Programme starts: the first 7 local authorities begin FGC referrals.
October 2020	The first FGCs take place. The largest group of local authorities (10) begins FGC referrals. First baseline surveys completed by local authorities.
November 2020	A further 2 local authorities begin FGC referrals. First text messages sent to parents. Further COVID-19 restrictions come into force.
January 2021	A further 1 local authority begins FGC referrals.
March 2021	First of 3 administrative data requests sent to local authorities. Focus group with Daybreak staff. Start of baseline interviews with social workers. Lifting of some COVID-19 restrictions.
April 2021	Final baseline surveys completed by local authorities. The final local authority begins FGC referrals.
May 2021	Final baseline interviews with social workers.
June 2021	Interviews with family members.
July 2021	Interviews with family members. Lifting of most COVID-19 restrictions.
September 2021	Second of 3 administrative data requests sent to local authorities. Follow-up survey sent to local authorities. Survey sent to FGC coordinators. The first local authorities (2) cease to randomise families.
October 2021	7 local authorities randomise their final families.
November 2021	The largest group of local authorities (9) randomises their final families.
December 2021	Reintroduction of some COVID-19 restrictions.
January 2022	1 local authority randomises its final families. End of Daybreak training offer to local authorities.

<b>February 2022</b>	Start of follow-up interviews with social workers. Lifting of COVID-19 restrictions.
<b>March 2022</b>	1 local authority randomises its final families. Final follow-up interviews with social workers.
<b>May 2022</b>	The final local authority still taking part randomises its final families.
<b>June 2022</b>	Third and final data request sent to local authorities.
<b>July 2022</b>	Final text messages sent to parents.

The trial is registered with the Open Science Framework ([osf.io/jrxn8](https://osf.io/jrxn8)). Following a recommendation for approval from Coram’s Research Ethics and Governance Advisory Group, Coram trustees approved the application for ethical approval of the evaluation plan on 22 January 2020.

## Impact evaluation questions and methods

The primary research question relates to **care status**: what effect does referring families for FGC at pre-proceedings stage, relative to usual services, have on whether children in 21 local authorities in England are in care (looked-after), 12 months after the pre-proceedings letter?

Our four secondary research questions are: does referring families for FGC at pre-proceedings stage relative to usual services when used in 21 local authorities in England...

- **Perceived inclusiveness**: ... change the mean perceived inclusiveness of how the local authority worked with the parent(s) in planning their child or children’s care, as assessed by parents on a four-point scale, around eight weeks after randomisation?
- **Sustainment of outcome**: ... change the likelihood that a child’s living arrangement remains the same or changes, between the date of a court judgment (or date of the letter informing families that their local authority will not pursue court proceedings) and a date 6, 12 or 18 months later?
- **Time spent in care**: ... change the number of days children spend as looked-after (versus time spent not looked-after) in the 6, 12 or 18 months after the pre-proceedings letter?
- **Court diversion**: ... change the likelihood that court proceedings go ahead (percent of children for whom they go ahead) by a date 6, 12 or 18 months after the pre-proceedings letter?

# Trial design

This trial was a parallel non-blinded randomised controlled trial (RCT) with 2 arms: 1:1 randomisation to usual services or usual services plus referral for an FGC, and an intention-to-treat analysis (Taylor et al., 2020).

## Randomisation

Families were randomised on a rolling basis with 1:1 assignment of each family to either the intervention or the control group within each local authority. We ensured that randomisation allocation would be more difficult to guess in advance, with blocks of different sizes, so that the treatment and control groups were equally sized in each local authority as well as overall. This is known as a permuted block design. As is typical in social policy trials, all parties were unblinded to allocation, meaning all parties knew which families were in the intervention or control group.

The randomisation platform was independent of Coram. Local authorities were asked to randomise families immediately after the Legal Gateway Panel by accessing a University of Aberdeen-hosted website. The website generated randomisation outcomes, the sequence being concealed from website users, and no re-randomisation of the same family ID number was possible. At this point, the decision that a family should enter pre-proceedings was known only to the local authority. The family became aware of the local authority's decision when the pre-proceedings letter was delivered (by hand or received in the post), which may be a few days or a week later. There may be one letter or two, in the case of two-parent families, but for simplicity we refer to the letter as singular. As we asked local authorities to send our information sheet in the same envelope, families should have simultaneously become aware of this evaluation.

## Intervention

The intervention evaluated was referral by local authorities of families to FGCs, at entry into pre-proceedings. Local authority processes vary, within the boundaries of legal requirements. However, usual services during pre-proceedings entail a relatively intensive period of involvement with a family by social workers and other professionals. The amount and nature of involvement will vary according to the family's circumstances and needs. For families randomised to the treatment group, referral for a relatively intensive period of involvement with an FGC coordinator was added, though the FGC meeting itself is a short, one-off intervention of around three hours in length. A typical meeting takes place in a neutral location (not the family home or local authority) (Family Rights Group, n.d.a; Early Intervention Foundation, 2018).

The model of intervention was made up of a combination of work by the charity Daybreak and work by the 21 local authorities. Daybreak, a provider of FGCs, provided support to the local authorities in the sample to ensure effective delivery and implementation. See the 'Background' section for more detail.

## Setting

Local authorities originally planned to begin referrals for FGCs for treatment group families from April 2020, but the start of the programme was delayed to September 2020 due to the COVID-19 pandemic, and the programme length shortened from 18 months to 14 months. Local authorities were able to randomise families into the study up until the end of the reporting period on 31 May 2022.

This trial took place during the COVID-19 pandemic. This was a period of rapid change and learning for local authorities in terms of delivering FGC via hybrid and remote methods. Although these delivery formats may continue in future, FGCs were originally intended to be in-person events: the service that this trial evaluated is not likely to be fully representative of routine practice.

## Data collection

Our primary outcome (and three of the four secondary outcomes) was collected from local authorities directly. On three occasions, we requested that data leads complete a pre-populated data return in a standard template, relating to the time period just finished, with the child as the unit of analysis. Due to family-level randomisation we did not know how many children were in each family in advance, and asked local authorities to add one row for each child.

Our secondary outcome on “perceived inclusiveness” was collected directly from parents by text message. At around eight weeks post-randomisation, texts were sent to parents asking them how involved they had felt with the planning of their child’s care. As part of the randomisation process on the study website, local authority staff were asked to provide up to three mobile phone numbers for each recipient of a pre-proceedings letter. We sent up to two texts – a first and reminder text. We sent the following first text message:

**“Hi, we’re Coram. You may remember our information sheet on our study about how local councils work with families. How involved have you been in planning your child(ren)’s care in the last two months? Please reply: 1: not at all 2: slightly 3: very 4: completely. We will send you a reminder in a week unless you reply STOP. Thank you.”**

A week later, we sent the following reminder to those who had not responded:

**“Hi, we’re Coram. Last week we asked you how involved you have been in planning your child(ren)’s care in the last two months. Please reply: 1: not at all 2: slightly 3: very 4: completely. As a thank you, you will be entered into our monthly prize draw to win a £25 Amazon voucher. Thank you.”**

The first text was sent 8.3 weeks after randomisation, on average. The maximum length of time between randomisation and our first text was 71 days (10.1 weeks); the minimum was 53 days (7.6 weeks).

# Outcomes

Our main analyses of all the outcomes except the perceived inclusiveness outcome are at 12 months, but to provide extra information we have also reported outcomes at 6 and 18 months:

**Care status:** point-in-time legal status taken from local authority records at 12 months after date on pre-proceedings letter. Dichotomised into: “in care” (looked-after child) or “not in care” (all other statuses)

1. **Perceived inclusiveness:** assessment by parents, two months into pre-proceedings, of their perceived level of inclusion in planning their child or children’s care, on a four-point scale. In two-parent families where both respond, we calculated the mean score, so there were seven possible answers per child
2. **Sustainment of outcome:** whether the child’s living arrangement, on 2 dates separated by 6, 12 or 18 months (3 separate variables), was the same or was different, taken from local authority records. The first date was the date of the court judgement or, in the case of families who do not go to court, the date of the letter informing families that their local authority will not pursue court proceedings. The second date was the date 6, 12 or 18 months after this
3. **Time spent in care:** number of days between date on pre-proceedings letter and a date 6, 12 or 18 months later (3 separate variables) spent “in care” (as a looked-after child), taken from local authority records
4. **Court diversion:** whether court proceedings are issued or stepped down, by the end of the reporting period (31 May 2022),<sup>4</sup> taken from local authority records.

The choice of 12 months balances two factors: waiting long enough for outcomes to have happened, and the declining sample size over time.

## Eligibility

We instructed local authorities to include all families who began pre-proceedings in the trial, including:

- Families where one or more members did not speak English as a first language (FGC coordinators should arrange for interpreters)
- Families where a restraining order or other circumstances meant that one or more family members could not be present in the same meeting (FGC coordinators can arrange separate meetings)
- Families with any number of children of any age, from unborn to the youngest child being 17 (Mason, 2017)<sup>5</sup>
- Families with one or more members based abroad (FGC coordinators can arrange video conferencing)

4 In the protocol this outcome was defined as “whether court proceedings are issued or not, by a date 6, 12, or 18 months after the pre-proceedings letter”. However, this was condensed to a single variable to maximise the sample size and avoid interpretation difficulties, given the possibility that FGCs may affect the timing of court proceedings.

5 There were no children in the data set where the youngest child in the family was aged over 17. No families or children were therefore excluded on the basis of age.

- Families with any type of problem or circumstances leading to the local authority having decided they should enter pre-proceedings (which may include neglect, physical abuse, emotional abuse, domestic violence, substance misuse, a combination of these or other issues)
- Families whose local authorities were seeking any kind or combination of court order(s), such as care, supervision or interim orders, and including children becoming looked-after with parental agreement (section 20)
- Families where the children were already living with relatives, friends or neighbours in a formal or informal kinship care arrangement
- Families where the children had any legal status other than that of looked-after child (we expected this would usually be Child Protection Plan but could be Child in Need or other)
- Families whose children had been previously looked-after, but the parents (or other family members) had regained care (so this may not be the first time they have started pre-proceedings)
- Families who had raised a complaint against the local authority
- Families who had previously taken part in an FGC
- Families where the parent(s) of the child or children were themselves aged under 18
- Families who were being entered into the trial by their local authority after the local authority had finished delivering the number of FGC referrals it agreed as part of the Supporting Families: Investing in Practice programme (i.e. unfunded cases that the local authority may decide to pay for itself)
- Families where new information came to light after they entered pre-proceedings, meaning the local authority took immediate action to escalate and pre-proceedings were bypassed or halted.

We asked local authorities to exclude the following families from randomisation:

- Families of children who were already looked-after
- Urgent or emergency cases where the local authority took immediate action, bypassing the pre-proceedings stage. As these families do not enter pre-proceedings, they were not eligible
- Families who had already entered pre-proceedings in the course of this evaluation. We told local authorities to only randomise families once.

## Sample size

We expected data to be available for analysis on 3300 families representing around 6000 children, assuming 1.79 children per family. In our power calculation (see Taylor et al., 2020) we assumed an intraclass correlation (ICC) of 0.9 per family because of the high likelihood that all siblings in a family have the same legal care status. Our code gave a minimum detectable effect size (MDES) of 0.09, but this was calculated before three local authorities dropped out of the trial. The MDES for the actual analysable sample was 0.210 (Table 3). The MDES is the smallest change or difference an impact evaluation estimates it will be able to detect.

Table 3. Sample size/minimum detectable effect size calculations

	Protocol estimate for randomised sample	Actual randomised sample	Protocol estimate for analysed sample	Actual analysed sample	
MDES (proportion of a standard deviation)	0.09	0.141	0.095	0.206	
Baseline/endline correlations - Child	N/A	N/A	N/A	N/A	
Intracluster correlations (ICCs) - Family	0.9	Unknown	0.9	0.90	
Alpha	0.05	0.05	0.05	0.05	
Power	0.8	0.8	0.8	0.8	
1-sided or 2-sided?	2-sided	2-sided	2-sided	2-sided	
Level of intervention clustering	Family	Family	Family	Family	
Average cluster	1.79	Unknown	1.79	1.77	
Expected final sample size (children)	Intervention	3300	Unknown	3000	643
	Control	3300	Unknown	3000	584
	Total	6600	Unknown	6000	1227
Expected final sample size (families)	Intervention	1850	757	1650	352
	Control	1850	754	1650	342
	Family	3700	1511	3300	694*

\* Analysable primary outcome (care status).

## Participating local authorities

Daybreak and What Works for Children’s Social Care selected 24 local authorities from 36 applicants with a view to maximising representativeness. During the planning phase, Merton and Blackpool dropped out of the programme, leaving 22. Over the course of the study Staffordshire dropped out, leaving a total sample of 21 local authorities.

Table 4. Characteristics of participating local authorities

Local authority	Type	Most recent children’s services Ofsted rating as of 2019	Mid-year population estimate, under 18s, 2018 (ONS)
Bath and North East Somerset	Unitary	Good	35,946
Birmingham	Metropolitan district	Requires improvement	288,388

<b>Bromley</b>	London borough	Good	75,055
<b>Derbyshire</b>	County	Good	153,272
<b>Knowsley</b>	Metropolitan district	Requires improvement	33,477
<b>Lambeth</b>	London borough	Requires improvement	249,727
<b>Lancashire</b>	County	Requires improvement	62,629
<b>Leicestershire</b>	County	Requires improvement	140,307
<b>Lewisham</b>	London borough	Requires improvement	68,458
<b>Middlesbrough</b>	Unitary	Requires improvement	32,513
<b>North East Lincolnshire</b>	Unitary	Good	34,503
<b>Northamptonshire</b>	County	Inadequate	170,235
<b>Nottingham City</b>	Unitary	Requires improvement	68,651
<b>Plymouth</b>	Unitary	Requires improvement	52,552
<b>Redcar and Cleveland</b>	Unitary	Requires improvement	27,626
<b>Rotherham</b>	Metropolitan district	Good	57,196
<b>Salford</b>	Metropolitan district	Good	56,566
<b>Sheffield</b>	Metropolitan district	Good	117,497
<b>Shropshire</b>	Unitary	Good	59,839
<b>Southampton</b>	Unitary	Requires improvement	50,832
<b>Sunderland</b>	Metropolitan district	Inadequate	54,563

Just over half of local authorities (n=11) recruited into the study had an Ofsted rating of “requires improvement”). Eight local authorities had a “good” rating and two had a rating of “inadequate” (Table 4). The most common type of local authority in this study was unitary (n=8, 38%), with 6 metropolitan districts, 4 counties and 3 London boroughs.

The local authorities contained a good mix of regions (covering all regions except the east of England) and of types, and covered a wide range of sizes. The smallest local authority was Redcar and Cleveland with 27,626 under-18s and the largest was Birmingham with 288,388 under-18s.

## Participating families

Overall, 1511 families were randomised into the study. At the point of randomising we did not have information on the number of children per family. The first randomisation was in September 2020, with the last in May 2022 (Table 5). As a mean average, local authorities randomised families into the study for 13 months (58 weeks), but this ranged from 12 to 19 months (50 to 82 weeks). The most common month for local authorities starting the programme was October 2020; the most common month for ending the programme was November 2021.

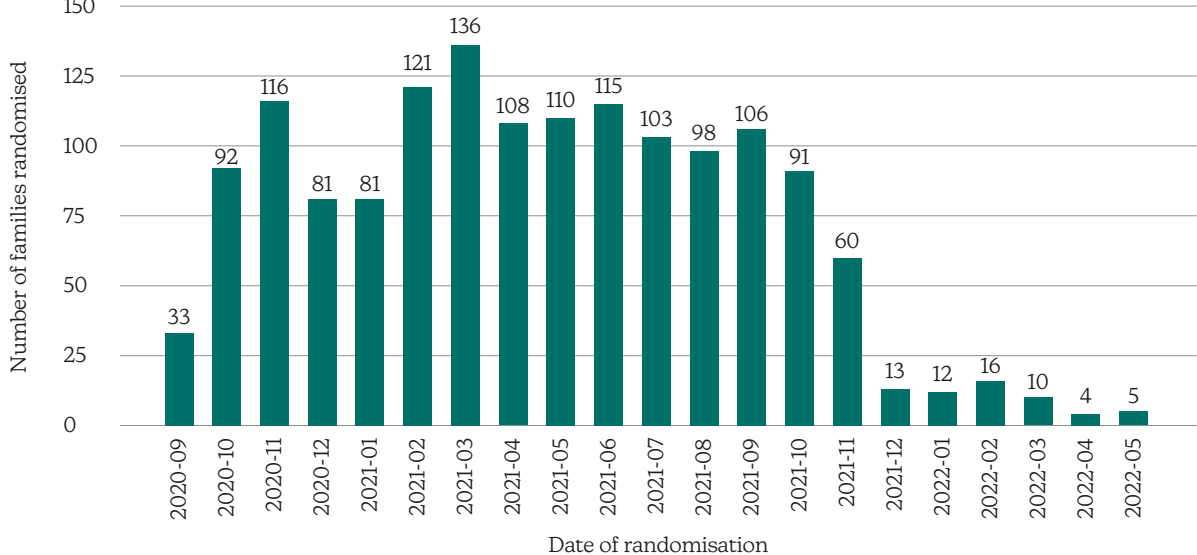
**Table 5. Randomisations by local authority**

Local authority	N families randomised	% of overall randomisations	First	Last	Weeks
Bath and North East Somerset	23	1.5%	07/10/2020	20/10/2021	54
Birmingham	118	7.8%	23/09/2020	26/10/2021	57
Bromley	46	3.0%	17/11/2020	23/11/2021	53
Derbyshire	70	4.6%	14/10/2020	23/11/2021	58
Knowsley	48	3.2%	01/10/2020	25/11/2021	60
Lambeth	48	3.2%	30/04/2021	25/05/2022	56
Lancashire	127	8.4%	03/09/2020	31/03/2022	82
Leicestershire	84	5.6%	13/10/2020	28/09/2021	50
Lewisham	79	5.2%	01/10/2020	14/02/2022	72
Middlesbrough	54	3.6%	06/10/2020	26/10/2021	55
North East Lincolnshire	60	4.0%	16/09/2020	20/10/2021	57
Northamptonshire	106	7.0%	06/10/2020	30/11/2021	60
Nottingham	48	3.2%	28/10/2020	17/11/2021	55
Plymouth	21	1.4%	10/11/2020	16/11/2021	53
Redcar & Cleveland	64	4.2%	21/09/2020	26/10/2021	57
Rotherham	135	8.9%	28/10/2020	29/11/2021	57
Salford	51	3.4%	23/09/2020	20/10/2021	56
Sheffield	61	4.0%	07/10/2020	01/10/2021	51
Shropshire	99	6.6%	09/09/2020	04/11/2021	60
Southampton	51	3.4%	06/01/2021	02/02/2022	56
Sunderland	118	7.8%	09/09/2020	28/10/2021	59
<b>Total</b>	<b>1511</b>	<b>100%</b>	<b>03/09/2020</b>	<b>25/05/2022</b>	<b>90</b>

Local authorities returned data for 1500 randomised families, 99% of the total families randomised into the study. Of these 1500, we excluded 57 children from 29 families from the data analysis: 3 families opted themselves out of the study; 13 families were ineligible for randomisation (see below); 8 family IDs could not be traced back to randomisation IDs; 3 families contained children who passed away over the course of the programme;

and contact was lost with 2 families. There was also one child who was excluded from data analysis because they had been adopted into another family several years prior. The total eligible sample therefore constituted 1471 families, containing 2548 children. Most randomisations took place between October 2020 and December 2021 (Figure 3).

**Figure 3. Histogram of the date of randomisation**



## Children excluded from analysis

Thirteen families (with 22 children) were excluded because they were randomised into the study despite not meeting the eligibility criteria, on the grounds that the children:

- were urgent or emergency cases where the local authority took immediate action, bypassing the pre-proceedings stage. As these families did not enter pre-proceedings, they were not eligible. More details on emergency cases are given in Appendix 4
- were in families who had already entered pre-proceedings in the course of this evaluation. Families should only be randomised once.

Local authorities did not provide explanations for all randomisations in error so we are unable to provide a more detailed breakdown.

We excluded a further 16 families (containing 35 children) from the analysis for the following reasons:

- 8 families containing 23 children could not be identified by the local authority (i.e. the ID code entered into the study website to randomise the family could not be traced in records)
- 3 families containing 5 children were excluded because the families opted themselves out of the study
- 3 families containing 4 children who passed away over the course of the study
- 2 families containing 2 children with whom local authorities lost contact over the course of the study.

One child was excluded because they were adopted by another family several years before this programme, but their birth siblings were not excluded.

A further 55 children from 28 families were excluded from the outcomes analysis owing to very poor-quality data. The total eligible sample was 1471 families (2548 children) and the potentially analysable sample was 1443 families (2493 children). Primary outcome data (care status at 12 months post-pre-proceedings letter) was available for 694 (1227 children) of these families.

Figure 4. Consort diagram showing total participant flow through study

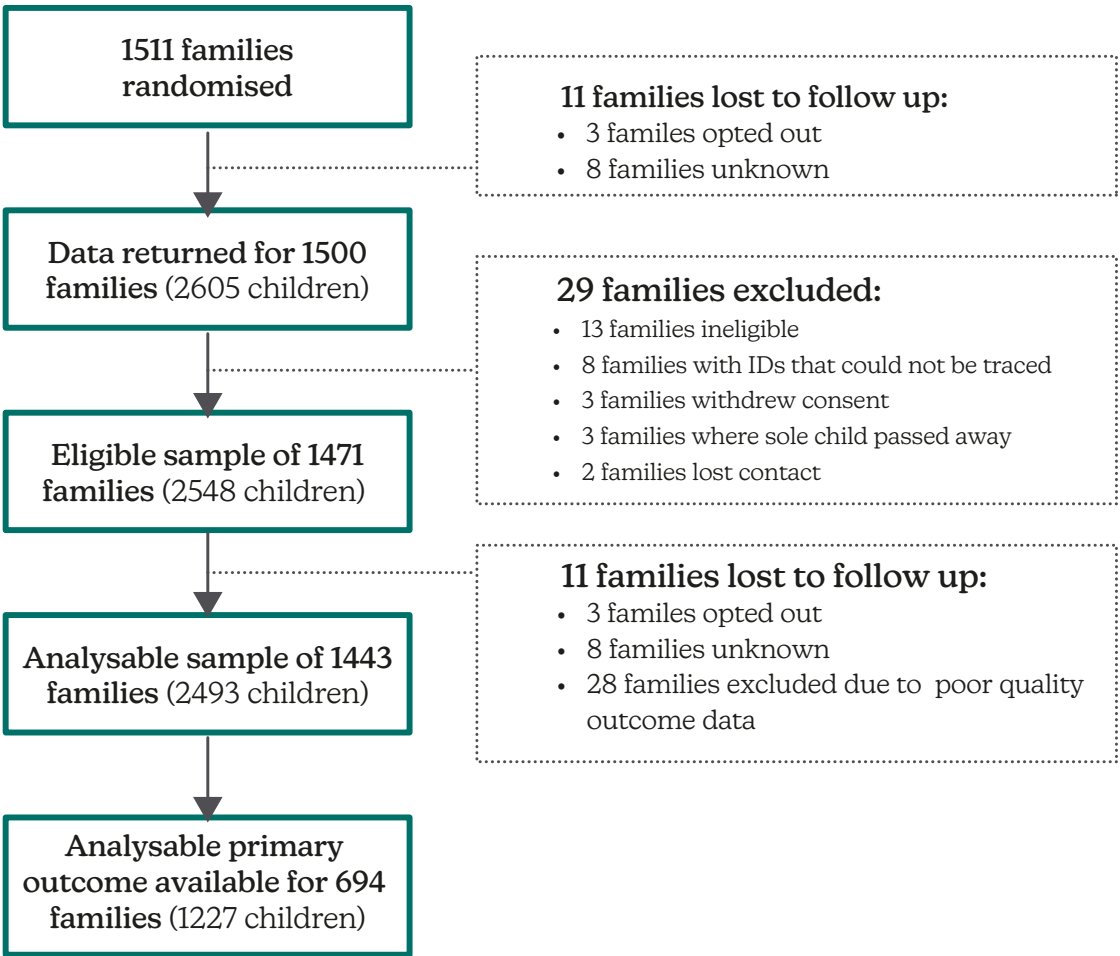
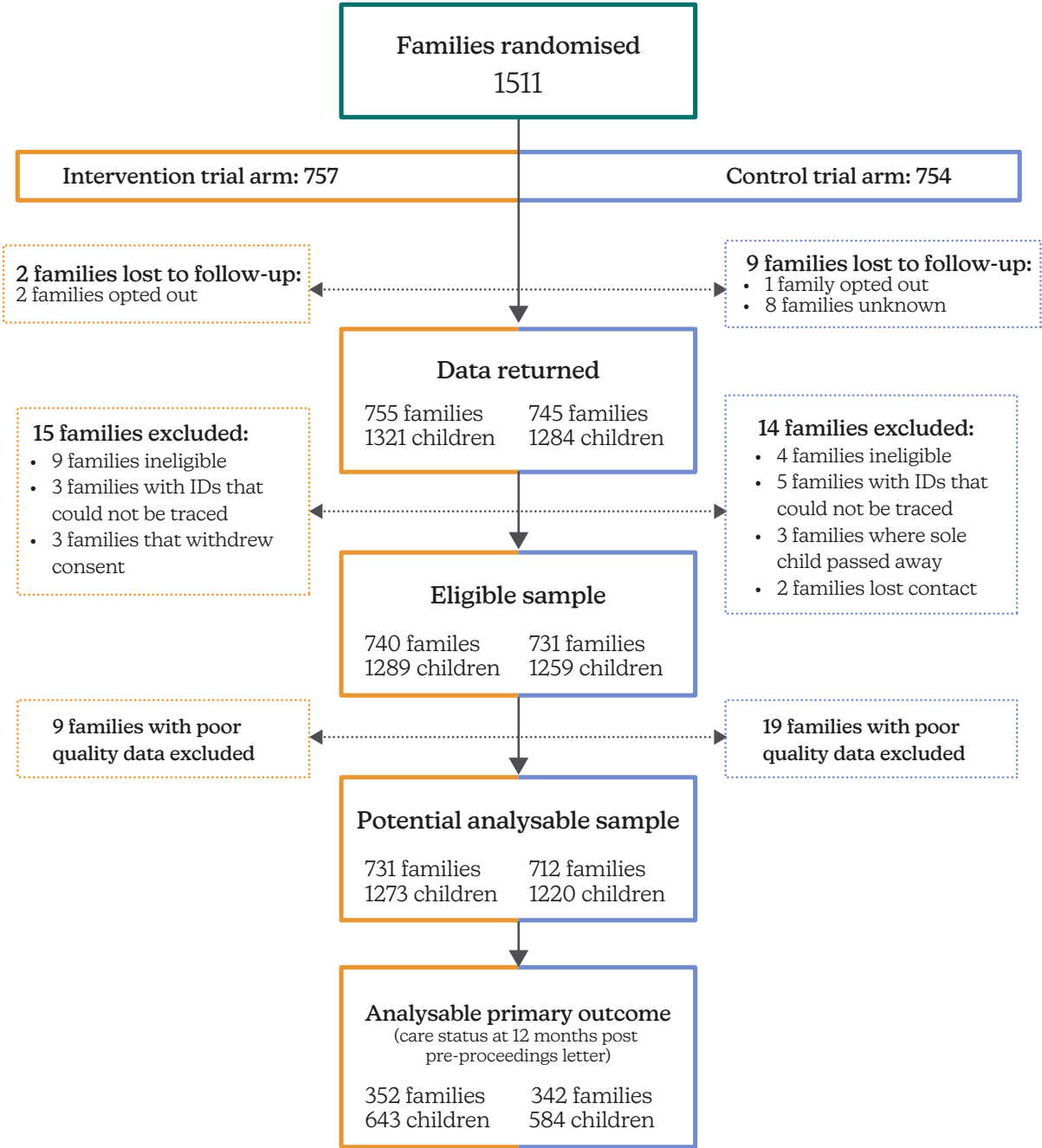


Figure 5. Consort diagram showing participant flow through study by trial arm



# Quantitative data cleaning

Excel spreadsheets were used to collect outcome data from local authorities. This data was imported into R Markdown statistical software (R Core Team, 2018; RStudio Team, 2020), where it was cleaned and merged. The list of R packages used is provided in Appendix 5. Where data did not meet the validation criteria (see Appendix 6), data contacts at the local authorities were contacted and if necessary asked to correct or update data.

Following this, if implausible or incomplete values were still present in the data, they were cleaned following the assumptions summarised below (see Appendix 6 for the complete list):

- We deleted duplicate cases (children)
- Where data was missing for a child that was likely to be consistent between siblings (e.g. date of pre-proceedings letter, FGC details, living arrangement, legal status), we assumed this was the same as for their sibling(s)
- Where the date on the pre-proceedings letter was missing or outside a plausible range, we used the randomisation date
- Where an FGC occurrence was missing or unknown, or the local authority reported that there was not an FGC but a date of FGC delivery was provided, we assumed that an FGC took place
- Where the date that care proceedings were issued was out of a plausible range, we excluded stage of care proceedings and the date from our analysis
- Where there was both a date for care proceedings issued and a date for a step-down letter, we used only the date when care proceedings were issued
- Where the start dates for the first living arrangement and legal status episodes were missing, these were made to be the date of randomisation. If there were previous episodes, this was made to be the day after the end date of the previous episode
- Where there was no end date for living arrangement or legal status episode, this was imputed as 31 May 2022 unless there was a subsequent episode. If there was a subsequent episode, this was made to be the date before the start date of the next episode.

We merged our spreadsheet with data gathered from local authorities by Daybreak, which provided further information about the FGCs that were delivered. Daybreak collected data monthly from local authorities using an Excel spreadsheet template to capture referrals and FGC delivery, in order to identify and address potential challenges. However, owing to some discrepancies in family ID codes, it was not possible to match all cases (one local authority's data was particularly difficult to match owing to all cases using different ID codes). Daybreak's data was generally used where there was a discrepancy with data provided in data returns. However, where local authorities reported that there was an FGC and this was either missing or reported as no FGC by Daybreak, we assumed that there was an FGC. After querying with local authorities, some FGCs still had implausible dates (n=5). However, these families were still assumed to have had FGCs.

We became aware of some limitations and weaknesses of the data throughout the trial. For example, some local authorities were compelled to estimate some data because it was not directly collected or stored – at least three local authorities noted that they do not have access to step-down letter dates, for example, and so estimated them from dates on the correspondence and files available to them.

## Missing data

Some baseline characteristics were more complete than others. For example, gender was reported for approximately 95% of the total eligible sample of children, but for care history this was only 77%. For some variables, such as the number of children per family, we cannot know whether data is missing (where local authorities have not reported data for siblings), other than by comparing these numbers with our predictions. A full summary of baseline characteristics including “unknown” cases is provided in Appendix 7.

With regard to data on the delivery of FGCs, after cleaning our data and merging this with Daybreak’s monthly monitoring data, some information was still incomplete. For example, for the 376 FGCs delivered to the eligible sample, we do not know the date for 67 (18%) of these, the delivery format for 16 FGCs (4%) or the number of attendees for 12 FGCs (3%). We have a detailed breakdown of who attended (e.g. father attended, child attended etc.) for 308 FGCs (82%).

Before imputation of missing values, there were considerable rates of missingness. For example, a key variable is date of pre-proceedings letter, as is the date from which the follow-up period for three of our key outcomes is calculated. In the overall sample (before exclusions) this was missing for approximately 22% of children. Missing data for our outcome variables varied across outcomes and local authorities. For example, for court diversion although local authorities reported that 689 children had been stepped down, step-down dates were only provided for 421 children (61%). Similarly, local authorities reported that care proceedings had been issued for 952 children, but dates were reported for only 715 children (75%). Some local authorities told us that step-down dates were not routinely recorded.

In terms of living arrangements and legal status, for 135 children (5%) in the eligible sample no living arrangements were reported and for 89 children (3%) no legal status was reported. However, in terms of cases where there are details of at least one living arrangement, missing data is more difficult to determine. For example, local authorities may have left the end date for a legal status episode blank because that legal status was still in place at the end of the reporting period (31 May 2022) or because the end date is missing and they have not added a subsequent episode.

# Quantitative data analysis

The data analysis was carried out in R Markdown (RStudio Team, 2020). Following the data analysis plan (see Taylor et al., 2020), we calculated descriptive statistics for the characteristics of the full sample and of those analysed. We calculated descriptive statistics for the intervention and control groups on each variable to check the key characteristics of respondents were balanced across intervention and control groups. We re-ran balance checks for the analysable sample for each outcome at 12 months.

We created our outcomes from local authority records as follows:

- For **care status**, using local authority records of legal episodes between randomisation and 31 May 2022, we used the legal status at 12 months post-pre-proceedings letter. If the legal status was “looked-after child” they were categorised as a looked-after child and if the legal status was “Child in Need”, “Child Protection Plan”, “none of these” or “not applicable”, they were categorised as a non-looked-after child. Cases were excluded where legal status at 12 months post-pre-proceedings letter was missing
- For **sustainment of outcome**, we looked at the child’s living arrangement at the date on which care proceedings were issued or the step-down letter was sent. We then looked at the living arrangement 12 months after this date. If the living arrangement was the same at the first date and at 12 months later, this was counted as a sustainment, and if the living arrangement was different this was counted as a change. This outcome does not therefore take into account any changes in living arrangement between these two time points. We have used a survival analysis to compensate for this
- For **time spent in care**, we calculated the total number of days spent as a looked-after child according to legal status between the pre-proceedings letter and 12 months post-pre-proceedings letter. We added up the days spent as a looked-after child across multiple legal status episodes that occurred during this period for each child
- For **court diversion**, we used the date that care proceedings were issued or the date of the step-down letter. If the date that care proceedings were issued was at or before 12 months post-pre-proceedings letter, this was categorised as “care proceedings were issued”. If there was a date after 12 months post-pre-proceedings letter, this was categorised as “care proceedings yet to be issued”. Similarly for the step-down letter date, if this was at or before 12 months post-pre-proceedings letter, this was categorised as “stepped down”. If the date was after 12 months post-pre-proceedings letter, this was categorised as “not yet stepped down”. The analysis plan in the protocol was to analyse this outcome at 6, 12 and 18 months, but this approach meant that court diversion was effectively analysed at 1 time point, including all proceedings/step-downs up until 31 May 2022. We chose to do this to maximise the sample size for this test. We also analysed this data using survival analysis to account for the time from the pre-proceedings letter.

Our **perceived inclusiveness** outcome was collected directly from parents. When two parents responded from the same family (in eight families), we calculated the mean and used this as a singular score for the family. If one phone number replied with a substantive answer more than once, we calculated the mean and used this as a singular response.

As per What Works for Children’s Social Care statistical guidance for RCTs (What Works for Children’s Social Care, 2021), our impact estimation employed an intention-to-treat (ITT) approach. We used regression analyses for each of our outcomes:

- For our primary analysis we used a logistic regression model to determine the likelihood of being looked-after or not looked-after at 12 months after the pre-proceedings letter being issued for both trial arms
- For our secondary outcomes of likelihood of court proceedings being issued and living arrangements being sustained, we also used logistic regression models
- For our secondary outcomes of days spent in care and perceived inclusiveness, we used a linear regression model.

For all models we included fixed effects for local authority<sup>6</sup> and reported clustered standard errors to account for the nesting of children within families. It is important to exercise caution when drawing comparisons between the effects for local authorities, because many factors not adjusted for in the analysis may influence the findings.

Odds ratios and risk ratios were used to express differences between trial arms for logistic regression models (care status, court diversion, sustainment of outcome) and Glass’s delta effect sizes were reported for linear regression models (days spent in care, perceived inclusiveness). An odds ratio is a measure of the odds of an event occurring for one group compared with the odds of it occurring for another. This was calculated as the exponentiated coefficients from the logistic regression.

A risk ratio is a measure of the relative risk of an event occurring for one group compared with another. We used the marginal standardisation approach outlined by Naimi and Whitcomb (2020) and their accompanying code to calculate risk ratios for the trial arm – outcome association adjusted for other predictor variables (e.g. gender, age etc.). We used bootstrapping to calculate bias-corrected and accelerated (BCa) 95% confidence intervals.

We have reported the regression models with and without predictor variables. Backwise deletion was used to remove variables that did not significantly predict the outcome, apart from variables that were unbalanced across trial arms, which were included in all regression models. The following variables were included in the model as predictors:

- Dummy variable for gender of child (female, male, unknown)<sup>7</sup>
- Dummy variables for ethnic group of child (White, Non-White)<sup>8</sup>
- Dummy variables for age of child at end of reporting period (0–3 years; 4–7 years; 8–11 years; 12–19 years; unknown)<sup>9</sup>
- Number of children in family (1, 2, 3, 4, 5+<sup>10</sup>)
- Dummy variables for child’s legal status on entry into pre-proceedings stage (Child in Need, Child Protection Plan, neither, unknown<sup>11</sup>)

6 For sustainment of outcome the sample size was too small to estimate fixed effects for local authorities.

7 Neither “ and “unknown” gender categories were merged due to the small number of cases.

8 Ethnic group categories were merged due to the small number of cases. Unknown categories were merged with non-White owing to the small number of cases.

9 Ages 12 to 19 were merged from 12 to 15, 16 and 17, and 18+ categories due to the small number of cases.

10 Five to nine children was grouped as five-plus children, owing to the small number of cases.

11 Owing to the approach to data cleaning and emergency escalations, 25% of children were looked-after at the date of the pre-proceedings letter. For this dummy variable, these children were categorised as “unknown” legal status.

- Dummy variables for time into implementation of FGCs in local authority of the date on the pre-proceedings letter (early: 1–4 months; mid: 5–9 months; established: 10 months+)
- Dummy variable for whether the child’s mother has previously had a child(ren) removed from their care (yes, no, unknown).<sup>12</sup>

We also conducted survival analyses to explore whether changes in outcomes occurred sooner or later for the intervention and control trial arms for a change in living arrangements and the decision to issue or step-down care proceedings. We reported Kaplan–Meier survival curves, survival times for the 25th, 50th (median) and 75th percentiles and standard errors for the intervention and control groups. Log-rank tests were carried out to detect statistically significant differences between the control and intervention group survival curves. We also reported an accelerated failure time model (AFT) or a multivariate Cox proportional hazard model depending on model fit, with all covariates initially included, and then removed if non-significant (backward elimination).

We reported descriptive statistics regarding the fidelity of the intervention for both trial arms. We have also reported descriptive statistics for key features of FGC delivery, such as the type of commissioning, training of facilitators, number of attendees and format of delivery. Given the substantial level of non-compliance, we have reported a per protocol and a complier average causal effect (CACE) analysis for outcomes at 12 months in order to estimate the impact of the FGC on compliers. For the purposes of the CACE analysis and the cost analysis, where FGC delivery was unknown, this was treated as no FGC delivered.

For the cost–benefit analysis, we used the percentage point difference in the proportion of children who were looked-after at 12 months (primary outcome) between trial arms. In order to calculate this difference adjusted for unbalanced predictors (care history) and fixed effects (local authority) we took the mean of the difference of the predicted probabilities across the full sample using our logistic regression. This was to avoid compositional differences between the trial arms contaminating the interpretation of the effect as a percentage point difference.

<sup>12</sup> This was not originally specified as a predictor variable in the protocol (Taylor et al., 2020) but was included as this baseline variable was unbalanced across trial arms. Deprivation indicator was included as a predictor in the protocol, but local authorities did not provide sufficient postcode information to match with area codes.

# Process evaluation questions and methods

Our process evaluation aimed to support the trial by providing evidence on the reason for the effectiveness or ineffectiveness of FGCs. It aimed to understand how FGCs were delivered across local authorities, what usual services and FGCs looked like and the experiences of those delivering and receiving services.

Our process evaluation questions were:

1. Was the project implemented as planned?
2. What were the barriers and enablers to successful implementation?
3. Were FGCs carried out as planned and to Daybreak standards? Why or why not?
4. Were any adaptations made to the FGC model that was planned? If so, what, why and where?
5. What, if any, changes were made to usual services?
6. How much did staff and families think that what they experienced during pre-proceedings (usual services or usual services plus referral for FGC) impacted their outcomes?

As planned, we used a number of qualitative and quantitative methods to answer these questions:

1. Interviews with families involved in pre-proceedings and case-holding social workers
2. A focus group with key Daybreak project staff
3. A baseline survey of all 21 local authorities
4. A follow-up survey of all 21 local authorities
5. A survey of 44 FGC practitioners and team managers from all 21 local authorities.

We also reviewed information provided by the Department for Education in December 2021 from a survey about the continuation of FGC delivery and by Daybreak in March 2022 on which local authorities would continue with FGC after the programme.

## Interview sample and recruitment

We used a case study approach for our interviews to help illustrate the different experiences of families receiving an FGC or usual services during pre-proceedings. We also wanted to understand what a pre-proceedings usual services offer looked like. The case studies aimed to help us understand the quantitative findings from the RCT and answer the process evaluation questions.

We selected eight families (or “cases”) from four local authorities. Four families (one per local authority) received an FGC and four families (one per local authority) did not. The local authorities were Bath and North East Somerset (BANES), Leicestershire, Lewisham and Rotherham. Local authorities were selected to provide diversity in region, FGC current practice in 2019, Ofsted rating and size (in terms of number of resident children) (Appendix 8).

For each case we aimed to interview at least one primary carer, a young person in the family, if they were available and willing, and the case-holding social worker. Interviewees were recruited through our main FGC contact in each of the four local authorities. As a thank you, primary carers received a £30 voucher for taking part and young people, who took part in shorter interviews, received a £15 voucher. Sampling limitations are discussed later.

## Interview method and analysis approach

Interviews were a mix of face-to-face, video and phone calls. Although only face-to-face interviews were planned (Taylor et al., 2020), during the COVID-19 pandemic video and phone calls were safer and more convenient for many participants. It has been argued that online interview methods can replicate and possibly improve on traditional methods (Braun, Clarke & Gray, 2017).

Interviews were semi-structured and ranged from 18 minutes to 55 minutes in length. We planned to interview the primary carers and young people at 1 time point (April to July 2021) and the case-holding social worker at 2 time points – an initial interview in April to May 2021 and then a follow-up interview 1 year later.

All interviews were recorded, transcribed and then analysed in two different ways. Data from the interviews with primary carers, young people and social workers was synthesised by case to understand support received, experiences of support and the outcome of pre-proceedings including the longer-term perspective from the social worker one year on. The second approach was analysing all interview data together using thematic analysis with the assistance of NVivo software. This was done mainly deductively to focus on answering the process evaluation questions. Transcripts were read and coded with the process evaluation questions as a guide. These codes were refined and developed into more focused higher-level analytical themes to help answer the process evaluation questions.

Using the unique family IDs, we also linked the families to the data collected from local authorities and Daybreak. We aimed to link our findings from interviews with post-FGC feedback forms; however, none were available.

## Interview participant characteristics

We spoke with 25 people via 21 interviews across the 4 local authorities to inform our case studies (see Appendix 9). We spoke with parents, young people and social workers. We interviewed 7 parents, 2 young people aged over 12 and over and 16 social workers, who all had 8 families in their caseloads.<sup>13</sup> When multiple social workers had been managing the family's case, we spoke with more than one social worker per family.

<sup>13</sup> One joint interview with two parents, two joint interviews with parent and child, one group interview with three social workers.

# Focus group method

In March 2022, a Coram researcher facilitated a focus group with two key project staff members from Daybreak. The virtual focus group was semi-structured and lasted 1 hour 45 minutes. It aimed to understand more about project set-up, delivery and enablers and barriers. The discussion was recorded, transcribed and analysed against the process evaluation questions.

# Survey samples and method

## Local authority surveys

We sent a link to a baseline online survey to our main contacts at local authorities approximately 1 month after their first family was randomised, which was between October 2020 and April 2021. We then sent the link to a follow-up survey to all local authorities in September 2021.

We received a 100% response rate (n=21) to both surveys, which were hosted on the platform SmartSurvey. Most baseline surveys were completed in November to December 2020, and follow-up surveys were completed in September to December 2021.<sup>56</sup>

Responses to our baseline survey were most commonly completed by managers, followed by heads of service and FGC coordinators and facilitators. We also received responses from an assistant director, a social worker, a project development officer, a team leader and a project support officer.

## FGC coordinator survey

We received 44 responses to the survey between September 2021 and December 2021, across all 21 local authorities. The mean number of responses per local authority was two and the range was one to five. Responses were received from 30 FGC coordinators, 5 facilitators, 3 practitioners, 4 team managers, 1 independent coordinator and 1 senior coordinator. We do not know how representative of all FGC coordinators who took part in the programme our respondents were.

Twenty-six respondents reported being on permanent contracts with the local authority, eight were on fixed-term contracts, eight were contractors for FGC services, one was on a secondment and one worked for Daybreak.

We asked respondents how long they had worked as an FGC coordinator, not just for their current local authority. The mean average (n=42) was 3 years and 11 months or, excluding 2 outliers with 20 or more years of experience, 2 years 10 months (34 months, n=40); the mode was 18 months (n=5). The minimum length of experience was 3 months. Most of this time had been spent coordinating/facilitating FGCs at pre-proceedings stage. The mean average was 3 years 1 month (37 months, n=42), or 2 years and 4 months (28 months) excluding outliers. Just under half (46%) of respondents had co-ordinated/facilitated FGCs at stages other than pre-proceedings stage.

With the exception of one respondent who had been in post for three months, all had received one-day training from Daybreak on FGC at pre-proceedings at the start of the programme.



# FINDINGS

# Impact evaluation findings

Our primary outcome, care status (a child having a legal status as looked-after or not looked-after) at 12 months after the pre-proceedings letter, was statistically significant ( $p=0.01$ ). Children in the intervention arm were 0.81 times less likely to be looked-after (36.2%), compared with children in the control arm (44.8%). This is a difference of 8.6 percentage points and can also be expressed as: children in the control arm were 1.24 times more likely to be looked-after.

Our secondary outcomes (except for perceived inclusiveness, which we exclude due to a low text message response rate) were subject to multiple tests corrections (9 tests) (Table 6). A summary of our impact analysis findings is summarised below:

- **Care status** was also statistically significant at 6 months, with children in the intervention arm 0.79 times less likely to be looked-after at 6 months after the pre-proceedings letter than children in the control arm
- **Sustainment of outcome** (whether living arrangement was the same or different at the time care proceedings were issued or stepped down and a date 6, 12 or 18 months later) was not significant at any time point. This is probably due to sample size, because for most children not enough time passed between their care proceedings being issued or stepped down and the follow-up date within the reporting period
- **Time spent in care** up to 6 months after the pre-proceedings letter was also statistically significant. Children in the intervention arm spent approximately ten fewer days as looked-after children compared with children in the control arm. The effect size was small at 0.19 (Glass's delta). Time spent in care was not significantly different between trial arms at 12 months post-pre-proceedings letter
- **Court diversion** was statistically significant, with children in the intervention arm 0.82 times less likely to have care proceedings issued (rather than stepped down) than children in the control arm by the end of the reporting period
- No effects were significant at 18 months, probably because samples were by then too small to be sufficiently powered.

The chances of obtaining a positive result by chance increase with the number of statistical tests carried out. It is good practice to correct for this. We used Hochberg's correction to look at our secondary outcome analyses at 12 months (Table 6 below). We calculated p-values against which we compared the observed p-values to determine statistical significance. Statistical significance is found if the observed p-value is smaller than the Hochberg-adjusted p-value.

It is interesting that time spent in care is not statistically significant (both before and after multiple tests corrections) at 12 months, but our primary outcome of care status at 12 months after the pre-proceedings letter is significant ( $p=0.01$ ). We suspect this is because time spent in care requires a more detailed account of the dates at which a child's legal status changed during the programme, whereas legal status at 12 months requires a legal status to be provided for a date 12 months from the pre-proceedings letter.

Table 6. Hochberg-adjusted p-values for secondary outcomes

	Observed p-value	Hochberg-adjusted p-value	Whether significant against Hochberg-adjusted p-value
Court diversion	0.000654	0.006	Significant
Care status at 6 months	0.00278	0.011	Significant
Time in care at 6 months	0.008	0.017	Significant
Time in care at 12 months	0.06	0.022	Non-significant
Care status at 18 months	0.12	0.028	Non-significant
Sustainment of outcome at 6 months	0.12	0.033	Non-significant
Sustainment of outcome at 18 months	0.36	0.039	Non-significant
Time in care at 18 months	0.56	0.044	Non-significant
Sustainment of outcome at 12 months	0.97	0.050	Non-significant

The difference between groups in care status remained at 18 months post-pre-proceedings letter (Table 7 next page), but was not statistically significant. This is probably due to the sample size falling to 312 children by this point, rather than a falling away over time of the effect we detected at 6 and 12 months. There is a possibility that there is no effect of FGCs at 18 months, but we consider this unlikely given the power calculations (Appendix 10) and the direction of the coefficient.

We used adjusted risk ratios for binary outcomes (care status, court diversion and sustainment of outcome) and Glass’s delta effect sizes for continuous outcomes (time spent in care). We have summarised our risk ratios and effect sizes in Tables 7 and 8 below.

Table 7. Summary of risk ratios for binary outcomes

Outcome and time period	Trial arm	N			Adjusted risk ratio (95% CI)
Care status		Looked-after (unadjusted cumulative incidence %)	Not looked-after	Total	
6 months	Intervention	361 (32.3%)	755	1116	0.79 (0.78–0.80)
	Control	465 (42.5%)	628	1093	
12 months	Intervention	223 (34.7%)	420	643	0.81 (0.79–0.83)
	Control	272 (46.6%)	312	584	
18 months	Intervention	55 (36.2)	97	152	0.76*
	Control	76 (47.5%)	84	160	
Court diversion		Proceedings issued (unadjusted cumulative incidence %)	Stepped down	Total	Adjusted risk ratio (95% CI)
End of reporting period	Intervention	367 (58.6%)	259	626	0.82 (0.81–0.84)
	Control	451 (72.2%)	174	625	
Sustainment of outcome		Change (unadjusted cumulative incidence %)	No change	Total	Adjusted risk ratio (95% CI)
6 months	Intervention	45 (11.1%)	359	404	0.69 (0.64–0.73)
	Control	72 (16.7%)	359	431	
12 months	Intervention	33 (18.0%)	150	183	1.05 (0.92–1.22)
	Control	43 (19.7%)	175	218	
18 months	Intervention	2 (28.6%)	5	7	2.29*
	Control	2 (12.5%)	14	16	

\*These risk ratios were not adjusted for local authority and care history, and confidence intervals were not calculated, owing to small sample sizes.

Table 8. Summary effect sizes for continuous outcomes

Outcome	Time period (months)	Glass's delta effect size	Lower 95% CI	Upper 95% CI
Time spent in care	6	0.19	0.11	0.28
	12	0.20	0.09	0.31
	18	0.17	-0.04	0.37
Perceived inclusiveness*	2 months from randomisation	-0.06	-0.36	0.23

\*This outcome was underpowered due to a low text response rate. Regression results are reported in Appendix 11.

## Participant characteristics

The eligible sample consisted of 2548 eligible children and young people in 1471 families. Of these, 740 families (50.3%) were allocated to the intervention group and 731 to the control group (49.7%). The intervention group comprised 1289 children (50.6% of the eligible sample) and the control group comprised 1259 children (49.4%). The number of children per family ranged from 1 to 8, with a mean of 1.7 children per family (SD=1.2).<sup>14</sup> We do not have information on whether families contained one or two parents.

On 31 May 2022, the children and young people were aged a mean average of 6.8 years (SD=5.4), ranging from 0 years up to 19. At the time of the final data return, 31 May 2022, 8 children in the sample (0.3%) were not yet born. In total 53% of the sample of children were female and 78% were White British. Around two-thirds of children (69%) came from households where their mother had not previously had a child/children removed from their care. The majority of children (90%) did not move outside of the local authority during the trial period. Full baseline characteristics of the sample are reported in Appendix 7 and the number of families by randomisation outcome per local authority is provided in Table 9.

Table 9. Number of families by local authority by randomisation outcome

Local authority	N eligible families (%)	Intervention (%)	Control (%)
Bath and North East Somerset	22 (1.5%)	12 (1.6%)	10 (1.4%)
Bromley	46 (3.1%)	23 (3.1%)	23 (3.2%)
Birmingham	116 (7.9%)	59 (8.0%)	57 (7.8%)
Derbyshire	67 (4.6%)	35 (4.7%)	32 (4.4%)
Knowsley	43 (2.9%)	22 (3.0%)	21 (2.9%)
Lambeth	43 (2.9%)	22 (3.0%)	21 (2.9%)
Lancashire	123 (8.4%)	63 (8.5%)	60 (8.2%)
Leicestershire	81 (5.5%)	45 (6.1%)	36 (4.9%)
Lewisham	77 (5.2%)	39 (5.3%)	38 (5.2%)

<sup>14</sup> This may be an underestimation. Local authorities were asked to provide information on all siblings in the household. We cannot be sure whether details for all siblings were provided.

Middlesbrough	54 (3.7%)	27 (3.7%)	27 (3.7%)
North East Lincolnshire	59 (4.0%)	29 (3.9%)	30 (4.1%)
Northamptonshire	105 (7.1%)	52 (7.0%)	53 (7.3%)
Nottingham	48 (3.3%)	24 (3.2%)	24 (3.3%)
Plymouth	21 (1.4%)	10 (1.4%)	11 (1.5%)
Redcar & Cleveland	63 (4.3%)	32 (4.3%)	31 (4.2%)
Rotherham	131 (8.9%)	64 (8.7%)	67 (9.2%)
Salford	49 (3.3%)	25 (3.4%)	24 (3.3%)
Sheffield	61 (4.2%)	31 (4.2%)	30 (4.1%)
Shropshire	95 (6.5%)	45 (6.1%)	50 (6.8%)
Southampton	50 (3.4%)	24 (3.2%)	26 (3.6%)
Sunderland	117 (8.0%)	57 (7.7%)	60 (8.2%)
<b>Total</b>	<b>1471 (100%)</b>	<b>740 (100%)</b>	<b>731 (100%)</b>

## Balance checks

We conducted balance checks to compare intervention and control group children in terms of the following key baseline characteristics:

- Age (mean)
- Sex
- Ethnicity (White British compared with any other ethnic group)
- Number of children per family (mean)
- Mover: whether the child moved outside of the local authority during the trial period
- Care history: whether mother had previously had a child or children removed
- Legal status at time of pre-proceedings letter (Child Protection Plan, Child in Need, neither).

We planned to check trial arms for levels of deprivation derived from postcode district. However, the postcode information provided was insufficiently detailed to allow this.

For continuous variables (age, number of children in the family), we tested balance using two sample t-tests with unequal variances. Balance in proportions (e.g. sex, ethnicity) was tested using a chi-square test for all known cases. All but two balance checks did not detect any statistically significant differences between intervention and control groups. There was a significant difference ( $p < 0.05$ ) in terms of whether a child's mother had previously had a child taken into care ( $p = 0.004$ ); a higher percentage of children's mothers in the control trial arm had previously had children removed compared with children in the intervention arm. This variable was therefore included as a predictor in the logit models in the main analysis. There was also a statistically significant difference in legal status at pre-proceedings letter ( $p = 0.02$ ).

Table 10. Baseline characteristics of overall sample by intervention group and balance checks

Variable	Intervention	N	Control	N	2-sided test (p-value)
<b>Total</b>	<b>50.6%</b>	<b>1289</b>	<b>49.4%</b>	<b>1259</b>	
Age (mean)	6.8	1251	6.7	1233	0.56
Sex (female)	46.0%	560 (1218)	48.0%	576 (1199)	0.33
Ethnicity (White British)	81.4%	1004 (1233)	80.9%	976 (1206)	0.79
Number of children per family <sup>1</sup> (mean)	1.7	740 families	1.7	733 families	0.74
Mover <sup>2</sup>	10.0%	115 (1145)	10.1%	111 (1104)	1.00
Care history <sup>3</sup>	27.9%	278 (998)	34.0%	327 (962)	0.004
Legal status at pre-proceedings letter date (Child in Need)	8.0%	97 (922)	10.9%	125 (844)	0.02
Legal status at pre-proceedings letter date (Child Protection Plan)	60.2%	734 (922)	53.2%	643 (844)	
Legal status at pre-proceedings letter date (neither)	31.8%	91 (922)	36.0%	76 (844)	

1. At family level. All other balance checks were analysed at child level.

2. Whether the child moved outside of the local authority during the trial period.

3. Whether the mother had previously had a child or children removed.

We re-ran the balance check excluding families where there was inconsistency in the response between siblings (a potential indicator of low data quality because for the majority of cases we would expect the mother's previous experience of children being removed to be the same for siblings). However, after excluding these cases, care history remained unbalanced between trial arms (p=0.004).

## Independence of observations across key outcomes

Participants were randomised at the family level given the whole-family nature of FGCs. However, cases were analysed at the child level. For this reason we have reported the intra-class cluster correlations for each child outcome (Table 11) to understand the independence of observations within families. These ICCs are in line with the assumptions used in the sample size calculations (Taylor et al., 2020; ICC 0.9), where we assumed that ICCs at family level were very high because of the likelihood that all siblings in a family have the same legal care status.

**Table 11. Intra-class correlation coefficients for individuals within families for key outcomes**

Outcome at 12 months	N cases	N families	Intra-class correlation coefficient (ICC)
Care status	1227	694	0.90
Court diversion (single time point)	1251	754	1.00
Sustainment of outcome	401	245	0.73
Time spent as looked-after child	1433	810	0.88

## Primary outcome

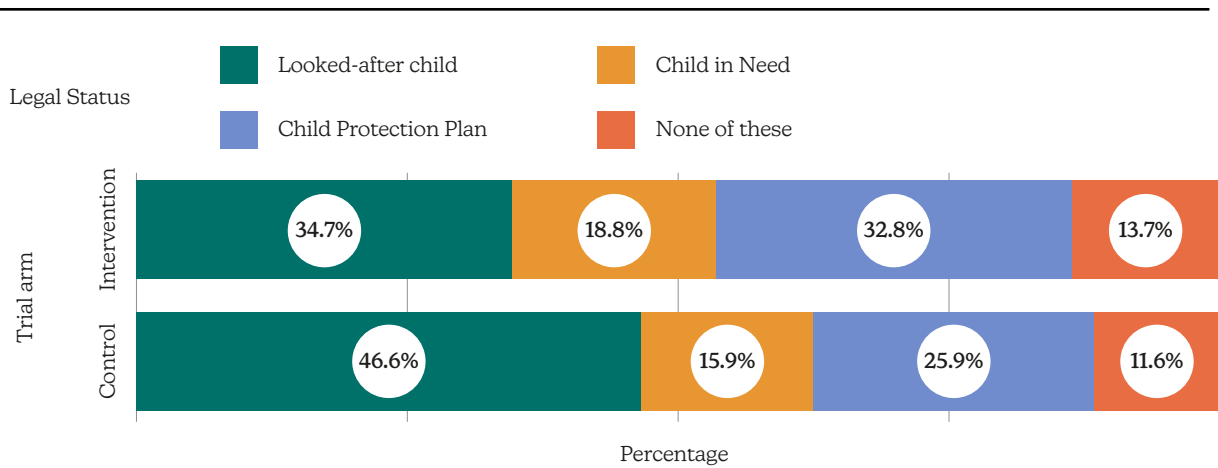
### Descriptive statistics

Children were included in the analysis if the pre-proceedings letter was issued at least 12 months before the end of the reporting period (31 May 2022; N=1433). N=1060 children were excluded from analysis because the end of the reporting period was less than 12 months after the pre-proceedings letter was issued. Of the 1433 children with letters in the acceptable date range, a legal status at 12 months after the date of the pre-proceedings letter was available for 1227 children. This subgroup of 1227 children is 49% of the total eligible sample of analysable children randomised into the study (2493) and 48% of the eligible children (2548) in the sample. This subgroup of 1227 children were members of 694 families.

As carried out and reported for the overall eligible sample, we re-ran the balance check to see whether baseline characteristics were balanced across trial arms for this subgroup for whom we have primary outcome data. All variables were balanced across trial arms except for previous care history. That is, children's mothers in the control condition were significantly more likely to have had a child or children previously removed from their care compared with children's mothers in the intervention condition.

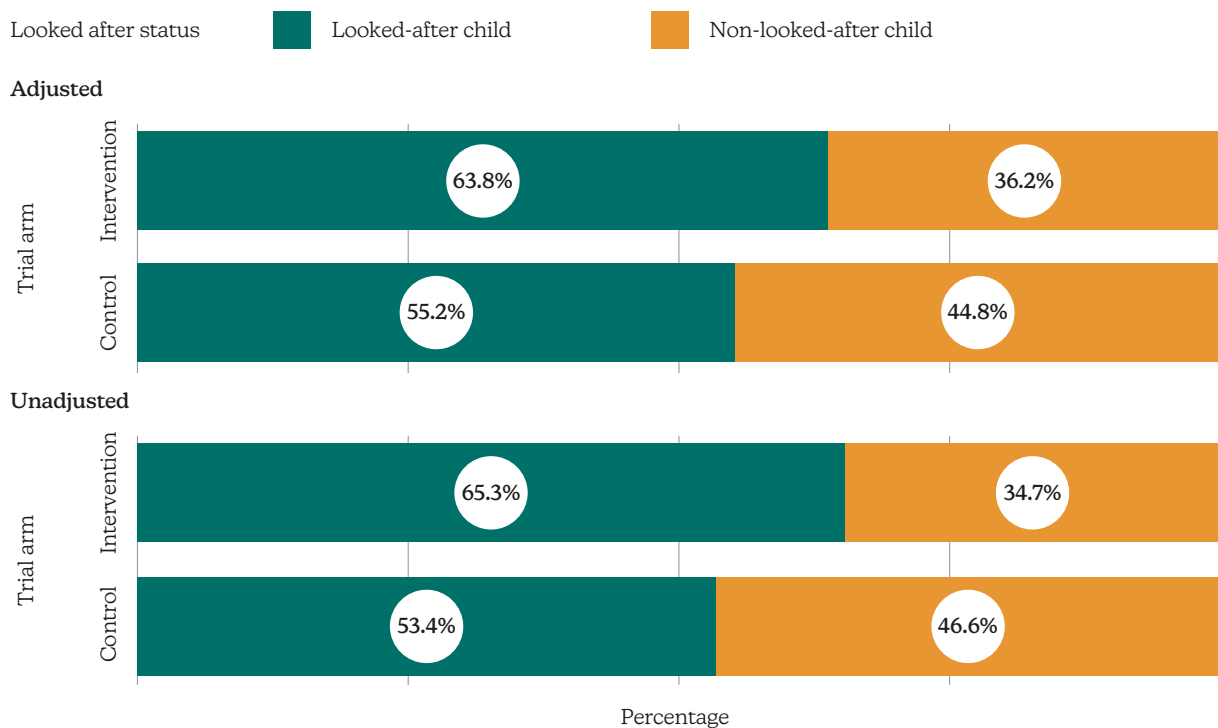
Overall, without adjustment for baseline imbalance, 40% of children (495) in this subsample of 1227 were looked-after at 12 months post-pre-proceedings letter, while 30% (362) were on a Child Protection Plan, 17% (214) were Children in Need and 13% (156) were not in any of these categories. This is broken down by trial arm in Figure 6.

**Figure 6. Child legal status at 12 months post-pre-proceedings letter by trial arm (unadjusted percentages)<sup>15</sup>**



At 12 months post-pre-proceedings letter, without adjustment for baseline imbalance, 40% of children were looked-after – 47% of children in the control trial arm and 35% of children in the intervention trial arm (Figure 7). Our headline analysis adjusts for baseline imbalances: at 12 months post-pre-proceedings letter 45% of children in the control trial arm were looked-after, and 36% of children in the intervention trial arm.

**Figure 7. Looked-after and non-looked-after children at 12 months post-pre-proceedings letter by trial arm (adjusted and unadjusted percentages)**



<sup>15</sup> We do not present adjusted proportions for all legal status categories, because these were not included in the regression analysis.

## Main findings

A logistic regression was fitted to the data with fixed effects for local authority to predict care status at 12 months post-pre-proceedings letter by trial arm. It is important to exercise caution when drawing comparisons between the effects for local authorities, because many factors not adjusted for in the analysis may influence differences. Care history was included as a predictor variable because this was significantly unbalanced between trial arms. Clustered standard errors were used to account for nesting within families (Table 12). According to this headline model, trial arm was a significant predictor of care status at 12 months ( $p=0.03$ ). The odds ratio was 0.68 (95% CI 0.46–0.95), meaning the chances of children in the intervention group being looked-after at 12 months were 0.68 times lower than if their family was randomised to the control group.

Our headline finding, the adjusted risk ratio from this model, was 0.81 (95% CI 0.79–0.83), meaning children in the intervention trial arm were 0.81 times less likely to have a looked-after child legal status 12 months post-pre-proceedings letter, compared with children in the control trial arm. The adjusted difference in the proportion of children who were looked-after at 12 months was 8.59 percentage points lower in the intervention arm compared with the control arm (44.8% compared with 36.2%). These adjusted numbers take account of imbalance between trial arms and constitute our main impact estimate. Risk ratios can be expressed in two ways. So we can also report that, comparing children in the control trial arm with children in the intervention arm, control group children were 1.24 times more likely to be looked-after.

Table 12. Basic logistic regression analysis of care status scores

Variable	Coefficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	-1.49	-3.10	0.12	0.82	0.07
Trial arm (intervention)	-0.42	-0.79	-0.05	0.19	0.03
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	0.26	-1.87	2.39	1.09	0.81
Birmingham	1.06	-0.64	2.76	0.87	0.22
Derbyshire	0.72	-1.03	2.46	0.89	0.42
Knowsley	-0.47	-2.31	1.37	0.94	0.62
Lambeth	0.25	-1.68	2.18	0.99	0.80
Lancashire	1.71	-0.12	3.53	0.93	0.07
Leicestershire	1.19	-0.63	3.01	0.93	0.20
Lewisham	0.66	-1.15	2.47	0.92	0.48
Middlesbrough	1.19	-0.57	2.95	0.90	0.18
North East Lincolnshire	2.40	0.60	4.20	0.92	0.01

<b>Northamptonshire</b>	1.22	-0.63	3.06	0.94	0.20
<b>Nottingham</b>	-0.39	-2.58	1.80	1.12	0.73
<b>Plymouth</b>	-0.17	-2.19	1.84	1.03	0.87
<b>Redcar and Cleveland</b>	-0.16	-2.20	1.88	1.04	0.88
<b>Rotherham</b>	0.18	-1.50	1.87	0.86	0.83
<b>Salford</b>	1.18	-0.67	3.04	0.95	0.21
<b>Sheffield</b>	1.02	-0.74	2.78	0.90	0.26
<b>Shropshire</b>	1.85	0.10	3.60	0.89	0.04
<b>Southampton</b>	1.76	-0.33	3.85	1.07	0.10
<b>Sunderland</b>	0.54	-1.22	2.30	0.90	0.55
<b>Care history (unknown)</b>	0.29	-0.38	0.96	0.34	0.40
<b>Care history (yes)</b>	1.43	0.95	1.91	0.24	<0.01
<b>Observations</b>	<b>1227</b>				

## Sensitivity analyses

We also carried out another logistic regression with fixed effects for local authority and other predictors, such as demographic characteristics and care history, to predict care status at 12 months post-pre-proceedings letter by trial arm. According to this model, trial arm was a significant predictor of care status at 12 months ( $p=0.01$ ). The odds ratio was 0.49 (95% CI 0.29–0.84), meaning the chances of children in the intervention group being looked-after at 12 months were 0.49 times lower than the chances for children in the intervention group. Clustered standard errors were used to account for nesting within families (Appendix 13). The adjusted risk ratio was 0.83 (95% CI 0.80–0.86).

We used backwise deletion to remove predictor variables from the previous model that did not significantly predict the outcome (care status at 12 months). According to this model, trial arm was a significant predictor of care status at 12 months ( $p=0.02$ ) (Appendix 13). The odds ratio was 0.52 (95% CI 0.30–0.89), meaning the chances of children in the intervention group being looked-after at 12 months were 0.52 times lower than the chances for children in the control group. Clustered standard errors were used to account for nesting within families. After adjusting for legal status at the time of pre-proceedings letter, the effect of trial arm remained significant. There was a significant effect for prior legal status, whereby children who were not Children in Need or on a Child Protection Plan were less likely to be a looked-after child at 12 months post-pre-proceedings letter. Adjusting for legal status decreases the odds ratio for trial arm slightly; in the basic regression there were 48% lower odds of children in the intervention arm being looked-after compared with the odds for the control arm, but for the adjusted regression this was 32% lower. The adjusted risk ratio was 0.86 (95% CI 0.84–0.88).

Given that care history was consistently imbalanced across trial arms and significantly predicted the outcome, we ran an exploratory analysis including care history as an interacting predictor with trial arm. These interactions were not statistically significant ( $p > 0.05$ ).

We also analysed care status at 6 months and 18 months after the pre-proceedings letter was issued. Owing to the time at which these outcomes were taken, the 18-month sample was considerably smaller (312 children) because only children for whom the pre-proceedings letter was issued by 30 November 2022 (18 months before the end of the reporting period) had outcomes available for analysis. As the direction was the same and the scale of the effect was similar at 6, 12 and 18 months (Table 7), it is likely that the smaller sample size accounts for the non-significant finding at 18 months, rather than a reduction in effectiveness over time. Trial arm was a significant predictor of care status at 6 months when adjusted for multiple tests ( $p < 0.01$ ,  $n = 2209$ ), with children in the intervention trial arm significantly less likely to be a looked-after child at 6 months (33.1%) post-pre-proceedings letter compared with children in the control arm (41.8%). Trial arm was not significant at 18 months ( $p = 0.12$ ), probably owing to the small sample size. Full regression analyses for care status at 6 and 18 months adjusted for local authority and previous care history are reported in Appendix 14.

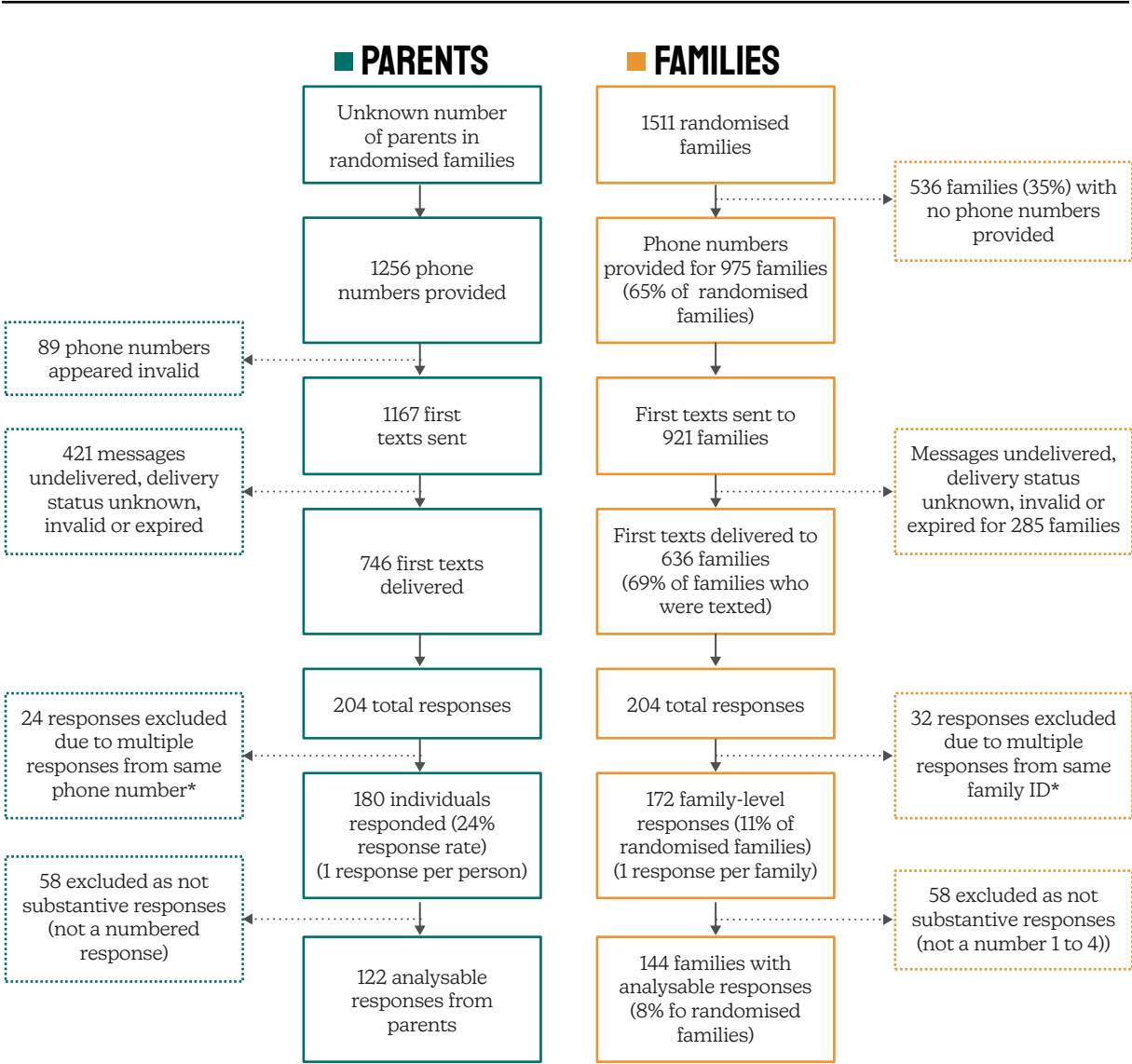
# Secondary outcomes

## Perceived inclusiveness

### Data collection

Data collected directly from parents will be outlined here, in contrast to the other outcomes that were collected from local authority records. There were 1511 randomised families, but the actual number of parents is unknown. Local authorities provided 1 or more phone numbers for 975 families (65%), making up a total of 1256 phone numbers (including 15 landline phone numbers). Of these, 50% were in the intervention group (624) and 50% in the control group (632). No phone numbers were provided for 536 families (35% of families randomised). A through-flow of participants providing analysable text responses is provided in Figure 8.

Figure 8. CONSORT diagram for text messages



\*Mean score was calculated when there was more than one substantial response from an individual or family.

Local authorities provided 1 phone number for just under half of families randomised (46%), 2 phone numbers for 17% of families and 3 phone numbers for 1% of families (Appendix 15).

In total we sent 1869 text messages to 1167 different mobile phone numbers, with 1419 messages delivered in total (including first and reminder texts). The rest of the messages were undelivered (n=336), invalid (n=25), the delivery status was unknown (n=82) or the message expired (n=7).

First text messages were delivered to 746 different phone numbers and the overall response rate was 24%. The number of delivered texts began to fall after December 2021 and the number of replies began to drop from November 2021 in line with falling randomisation numbers towards the end of the programme. In total we received 204 replies from 180 different numbers. Of these, around 58% were prompted by our first text and around 42% were prompted by our reminder text a week later. As an incentive, we offered a monthly prize draw for a £25 voucher in our reminder texts. We sent vouchers to a total of 17 phone numbers. See Appendix 15 for the number of text messages sent and received by month and in total.

As planned, parents answered our question around 2 months after randomisation. On average, responses came in 61 days after randomisation (just under 9 weeks). We received responses 53 to 88 days after randomisation (7.6 to 12.6 weeks). Parents responded just under 2 days after we texted them on average. The minimum response time was 0 days and maximum response time was 74 days after we texted them.

There was variation in the completeness of phone number data provided by each local authority, impacting the number of texts sent to families in each authority. The response rate varied by local authority from 0% in Southampton (although only 5 texts in total were delivered to families in this local authority) to 48% in Knowsley. Detailed response rates by local authority are provided in Appendix 15.

We received 129 substantive messages, meaning the text contained a number between 1 and 4 (Table 13). Among those providing a substantive answer, 54% said they had felt very or completely included in planning their child(ren)’s care in the last 2 months. Texts were delivered to 367 different numbers from the “refer for FGC” group (49% of text messages delivered overall) and to 379 different numbers from the “do not refer” group (51%). We did not gather equivalent data from wider family members – a possible avenue for future research.

**Table 13. Responses to the text message question “How involved have you been in planning your child(ren)’s care in the last two months?” by June 2022**

Response	Number of responses*	Percent of responses**	
		All responses	Valid responses
<b>1: not at all</b>	34	17%	26%
<b>1.5***</b>	1	0%	1%
<b>2: slightly</b>	24	12%	19%
<b>3: very</b>	14	7%	11%

Response	Number of responses*	Percent of responses**	
		All responses	Valid responses
4: completely	56	27%	43%
Subtotal	129	63%	100%
Opt-out (“STOP”)	56	27%	
Other	19	9%	
Total	204	100%	

\* Responses, not phone numbers; includes some replies where the number sent more than 1 response.

\*\* Percentages may not sum to 100 due to rounding.

\*\*\* One parent responded outside the 1-4 answer categories we provided.

In some of the “other” written responses parents expressed their views about their experience of social services, the care of their children and how involved they felt, or expressed confusion about receiving text messages about planning their children’s care.

## Characteristics of responders

Families for whom phone numbers were available are likely to have differed from other families in unknown ways. We were able to match the texts sent to the main data set of eligible participants for 889 families (or 1580 children), to explore differences in the characteristics between families who responded to text messages and those who did not. Children whose parent(s) replied to the text about perceived inclusiveness were comparable to children whose parent(s) did not reply across all baseline characteristics except for previous care history (Appendix 7). Children whose parents responded to the text message were more likely to have a mother who had previously had a child or children removed from her care ( $p < 0.01$ ).

## Analysable subsample

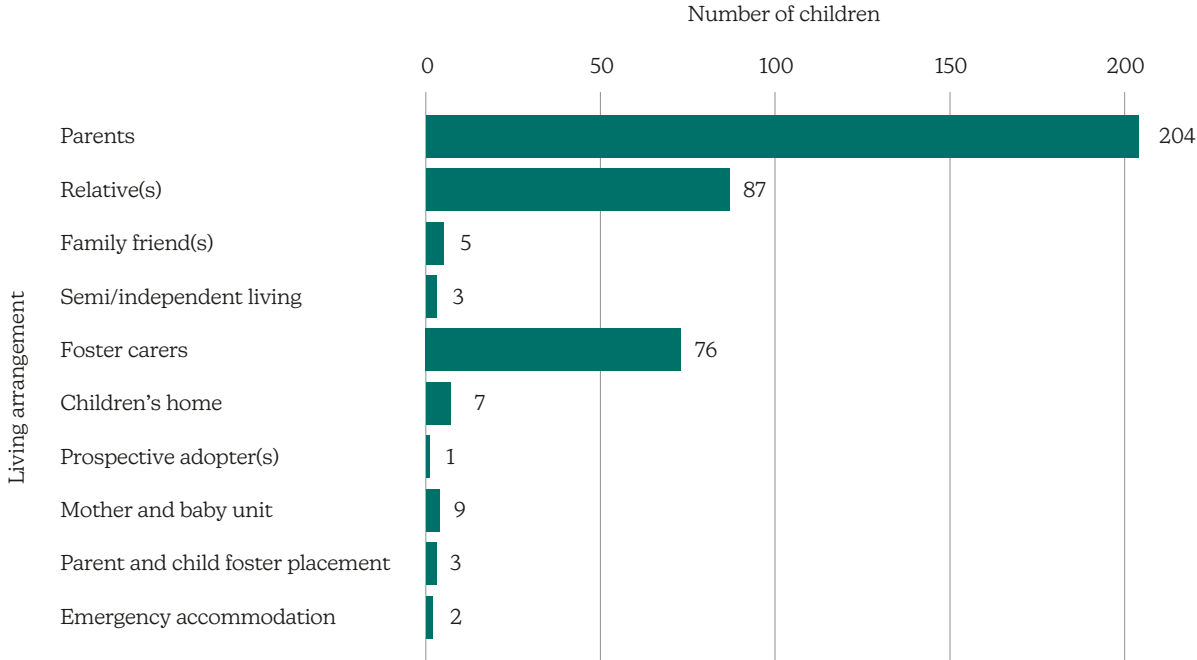
We analysed one text message response per family (mean responses calculated; see data analysis section above). We only included text responses for families whose family ID codes could be traced back to our randomisation spreadsheet, meaning that we excluded 2 families who could not be traced and who were eligible for randomisation, resulting in an analysable sample of 111. This comprises 7.5% of the total eligible families randomised into the study. These families comprise 188 children, which is 7.3% of the total eligible sample. In our protocol we specified that we would not include perceived inclusiveness as an outcome if the overall response rate was less than 50%. This is therefore not part of our main impact analysis, but we report our findings in Appendix 11 for transparency.

## Sustainment of outcome

We had information about the living arrangement of 1173 children at the date of either the step-down letter or when care proceedings were issued. This reduced to  $n=401$  children by 12 months after this date, in 245 families. This fall is not surprising given that fewer months of outcomes data were available for children in families entering pre-proceedings in the later months of the programme. Participants in this sample were from 20 of the 21 local authorities. Balance checks showed that trial arms were equivalent across all baseline characteristics except for care history (Appendix 7).

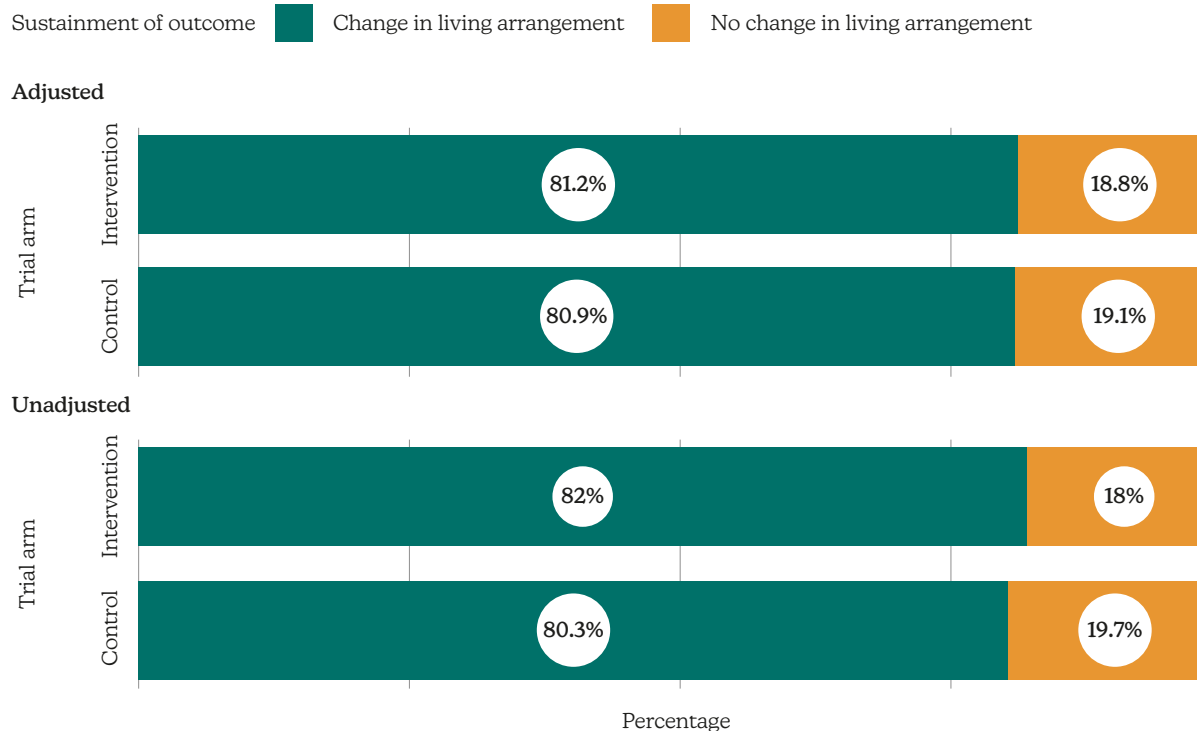
Just over half of these children (61%) were living with their birth parent(s)/adoptive parent(s) at the point of care proceedings either being issued or stepped down (Figure 9). Next most often, 17% were living with relative(s) and another 12% were living with foster carer(s).

**Figure 9. Living arrangement 12 months after proceedings issued or case stepped down**



The majority of children in both the intervention (82%) and control trial arm (80%) did not experience a change in living arrangement between this time and a date 12 months later (Figure 10).

**Figure 10. Living arrangement 12 months after proceedings issued or case stepped down by trial arm (adjusted and unadjusted percentages)**



A logistic regression was fitted to the data. According to this model, trial arm was not a significant predictor of sustainment of living arrangements at 12 months ( $p=0.97$ ) (Table 14). The odds ratio was 0.98 (95% CI 0.45–2.15), meaning the chances of children experiencing a change in living arrangement at 12 months did not differ by trial arm. The adjusted risk ratio was 1.05 (95% CI 0.92–1.22). Clustered standard errors were used to account for nesting within families (Table 14). The adjusted difference between trial arms was -0.21%, with 18.8% of children in the intervention arm experiencing a change in living arrangements by 12 months compared with 19.1% in the control arm.

**Table 14. Basic regression analysis for sustainment of outcome**

Variable	Coefficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	-1.00	-3.15	1.15	1.10	0.36
Trial arm (intervention)	-0.02	-0.80	0.77	0.40	0.97
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	-0.61	-4.02	2.80	1.74	0.73
Birmingham	-17.86	-20.11	-15.60	1.15	<0.01
Derbyshire	-0.48	-2.71	1.75	1.14	0.67
Knowsley	-18.02	-20.34	-15.70	1.19	<0.01
Lancashire	-0.01	-2.31	2.29	1.17	0.99
Leicestershire	0.18	-2.67	3.03	1.45	0.90
Lewisham	-1.89	-4.82	1.04	1.50	0.21
Middlesbrough	-18.06	-20.33	-15.79	1.16	<0.01
North East Lincolnshire	1.09	-1.27	3.46	1.21	0.37
Northamptonshire	-18.10	-20.65	-15.56	1.30	<0.01
Nottingham	-17.71	-19.94	-15.49	1.13	<0.01
Plymouth	-17.91	-20.35	-15.46	1.25	<0.01
Redcar and Cleveland	-0.50	-3.06	2.07	1.31	0.70
Rotherham	-1.89	-4.89	1.12	1.53	0.22
Salford	0.20	-2.54	2.94	1.40	0.88
Sheffield	-0.61	-2.97	1.74	1.20	0.61
Shropshire	-1.82	-4.22	0.58	1.23	0.14
Southampton	-17.56	-19.87	-15.25	1.18	<0.01
Sunderland	-1.42	-3.80	0.96	1.21	0.24
Care history (unknown)	0.54	-0.83	1.91	0.70	0.44
Care history (yes)	0.88	-0.01	1.77	0.45	0.05
Observations	401				

We also fitted a logistic regression with all predictor variables. According to this model, trial arm was not a significant predictor of sustainment of living arrangements at 12 months ( $p=0.56$ ) (Appendix 13). The odds ratio was 0.78 (95% CI 0.34–1.81), meaning the chances of children having changed living arrangement by 12 months did not differ according to whether they were randomised to the control or intervention group. The risk ratio was 1.02 (95% CI 0.84–1.25). Clustered standard errors were used to account for nesting within families.

We used backwise deletion to remove predictor variables from the previous model that did not significantly predict the outcome (change in living arrangements at 12 months). Although care history was not a significant predictor in the model above ( $p>0.05$ ), it was retained as it was unbalanced across trial arms. The odds ratio was 0.90 (95% CI 0.40–2.03) and trial arm was not a significant predictor of sustainment of outcome ( $p=0.80$ ). The adjusted risk ratio was 1.02 (95% CI 0.86–1.22). After adjusting for significant predictor variables, the effect for trial arm remained non-significant and the confidence interval for the odds ratio still crossed 1 – i.e. no difference between trial arms.

We also analysed sustainment of outcome at 6 months and 18 months after the pre-proceedings letter was issued. This outcome has smaller sample sizes at each time point compared with our samples for care status and time in care, which measured the time period for observation from the pre-proceedings letter, whereas sustainment of outcome is measured from the date at which care proceedings were either issued or stepped down. On average, care proceedings were issued 17 weeks (SD=13.7) or stepped down 22 weeks (SD=12.1) after the pre-proceedings letter was sent. The sample sizes for 6, 12 and 18 months for sustainment of outcome were 835, 401 and 23 children respectively.

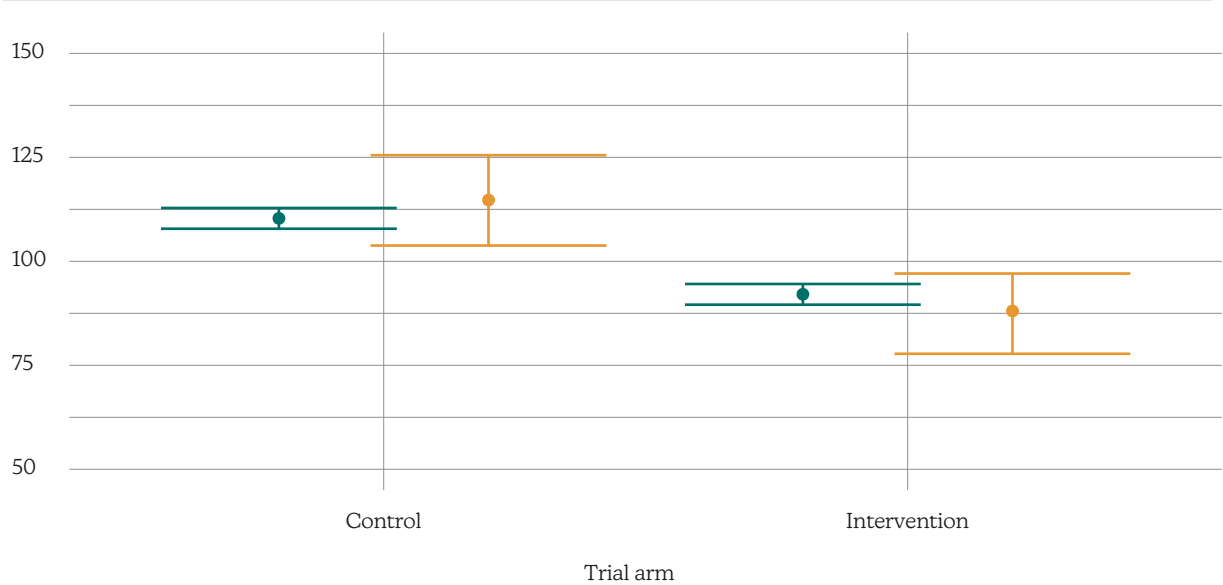
Trial arm did not significantly predict sustainment of outcome at either 6 ( $p=0.12$ ) or 18 months ( $p=0.36$ ). Full regression analyses for sustainment of outcome at 6 and 18 months adjusted for local authority and previous care history are reported in Appendix 14. The sample size, particularly at 12 and 18 months, probably meant that these analyses were not sufficiently powered to detect an effect. For example, the confidence interval for the 18-month regression analysis crossed 0 and had a large clustered standard error (1.74).

## Time spent in care

We had sufficient data for analysis of time spent in care between the pre-proceedings letter and a date 12 months later for 1433 children, comprising 56% of the total eligible sample. Balance checks showed that trial arms were equivalent across all baseline characteristics except for care history (Appendix 7).

Over half of the children did not spend any time in care between their pre-proceedings letter and a date 12 months later ( $n=886$ , 61.8%); these children were included in the regression and counted as 0. Overall the mean average number of days spent in care was 100 days (SD=140.5). The mean number of days spent in care was higher for children in the control trial arm (115 days, SD=141.1) than in the intervention trial arm (87 days, SD=135.8). The lower bound for the control group is higher than the mean (dot) of the intervention group, meaning that, at this level of significance, we can say with confidence that there is a difference.

Figure 11. Forest plot showing mean days in care by trial arm with 95% bootstrapped confidence interval (1000 reps)



A linear regression was fitted to the data with fixed effects for local authority to predict days spent in care by trial arm. According to this model, there was not a statistically significant difference in time spent in care for children randomised to the intervention arm compared with the control arm ( $p=0.06$ ) (Table 15). Clustered standard errors were used to account for nesting within families. The Glass’s delta effect size was 0.20 (95% CI 0.09–0.31).

Table 15. Basic regression analysis of time spent as a looked-after child by trial arm

	Coefficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	35.48	-24.24	95.20	30.44	0.24
Trial arm (intervention)	-18.98	-39.04	1.08	10.23	0.06
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	24.64	-65.19	114.47	45.79	0.59
Birmingham	41.48	-24.90	107.86	33.84	0.22
Derbyshire	45.28	-25.15	115.70	35.90	0.21
Knowsley	-1.08	-66.48	64.32	33.34	0.97
Lambeth	36.24	-63.14	135.61	50.66	0.47
Lancashire	69.74	-2.27	141.75	36.71	0.06
Leicestershire	53.43	-21.21	128.08	38.05	0.16
Lewisham	42.48	-32.81	117.78	38.38	0.27
Middlesbrough	97.90	17.88	177.92	40.79	0.02
North East Lincolnshire	112.96	39.81	186.12	37.29	<0.01

<b>Northamptonshire</b>	114.89	28.37	201.40	44.10	0.01
<b>Nottingham</b>	-5.11	-73.59	63.37	34.91	0.88
<b>Plymouth</b>	11.91	-65.69	89.50	39.56	0.76
<b>Redcar and Cleveland</b>	18.77	-55.51	93.06	37.87	0.62
<b>Rotherham</b>	36.31	-29.69	102.30	33.64	0.28
<b>Salford</b>	61.77	-13.80	137.35	38.53	0.11
<b>Sheffield</b>	71.48	-2.19	145.16	37.56	0.06
<b>Shropshire</b>	100.59	27.99	173.19	37.01	0.01
<b>Southampton</b>	121.35	17.27	225.42	53.06	0.02
<b>Sunderland</b>	15.64	-48.10	79.37	32.49	0.63
<b>Care history (unknown)</b>	9.19	-24.43	42.80	17.14	0.59
<b>Care history (yes)</b>	76.34	52.08	100.61	12.37	<0.01
<b>Observations</b>	1433				

A second linear regression was fitted including all predictor variables. According to this model, there was not a statistically significant difference in time spent in care for children randomised to the intervention arm compared with the control arm ( $p=0.20$ ) (Appendix 13). Clustered standard errors were used to account for nesting within families.

We also fitted a linear regression with just significant predictor variables. According to this model, there was not a statistically significant difference in time spent in care for children randomised to the intervention arm compared with the control arm ( $p=0.24$ ). Adjusting for age, legal status and care history resulted in a less pronounced effect (smaller regression coefficient and larger  $p$ -value) for trial arm compared with our basic model that adjusted for care history. Given the smaller confidence interval, it may be that adjusting for legal status and age gives us a more precise estimate of the effect of trial arm and that this effect is smaller (8 days compared with 19).

It is interesting that time spent in care is not statistically significant (both before and after multiple tests corrections) at 12 months, but our primary outcome of care status at 12 months after the pre-proceedings letter is significant ( $p=0.01$ ). Although children in the control trial arm spent on average more time in care (115 days) than children in the intervention trial arm (87 days), when adjusted for local authority and previous care history this difference was not significantly different ( $p=0.06$ ). We suspect that our findings for time spent in care in the 12 months following the pre-proceedings letter and care status at 12 months post-proceedings letter differ owing to the nature of the data capture for these two variables. Time spent in care is calculated by totalling episodes of legal statuses reported by local authorities. Care status was calculated from the legal status reported for a child at a date 12 months after their pre-proceedings letter. Time spent in care therefore requires a more detailed account of the dates when a child's legal status changed during the programme, whereas legal status at 12 months requires a legal status to be provided for a date 12 months after the pre-proceedings letter. We therefore

believe that our primary outcome of care status is more reliable, because it is less burdensome for local authorities to provide enough data to calculate it accurately.

We also analysed time spent in care at 6 and 18 months after the pre-proceedings letter was issued. The 6-month analysis increased the sample size considerably to 2398 children, because we were able to include children in the analysis who had a pre-proceedings letter issued by 30 November 2021 (6 months before the end of the reporting period). Trial arm was a significant predictor of time spent in care at 6 months ( $p=0.01$ ) when adjusted for multiple tests, with children spending on average 10 fewer days in care in the intervention arm. The Glass’s delta effect size was 0.19 (0.11–0.28). Unfortunately, the sample size at 18 months was small at 401 children. Trial arm did not significantly predict time spent in care ( $p=0.56$ ) with the confidence interval for the regression coefficient and Glass’s delta crossing 0. Full regression analyses for time spent in care at 6 and 18 months adjusted for local authority and previous care history are reported in Appendix 14.

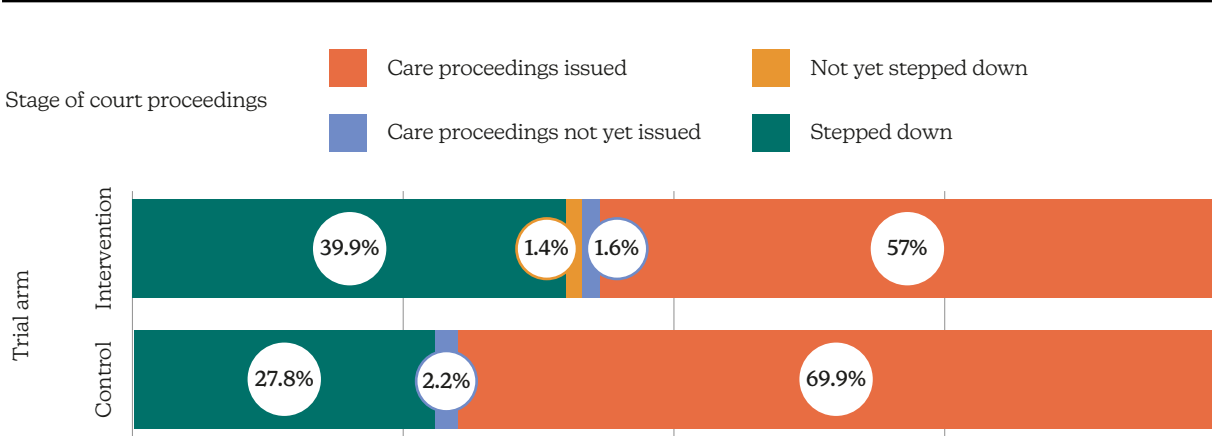
### Court diversion

We had sufficient data for analysis of court diversion for 1251 children (in 754 families), comprising 49% of the total eligible sample. Balance checks showed that trial arms were equivalent across all baseline characteristics except for care history.

For the majority of children in this subsample care proceedings had been issued by 12 months post-pre-proceedings letter ( $n=794$ , 63%) and for  $n=24$  (2%) of children, care proceedings would go on to be issued but had not been by this date. For a third of children ( $n=424$ , 34%) the case was stepped down and for  $n=9$  children (1%) the case would be stepped down but had not been by 12 months post-pre-proceedings (Figure 12).

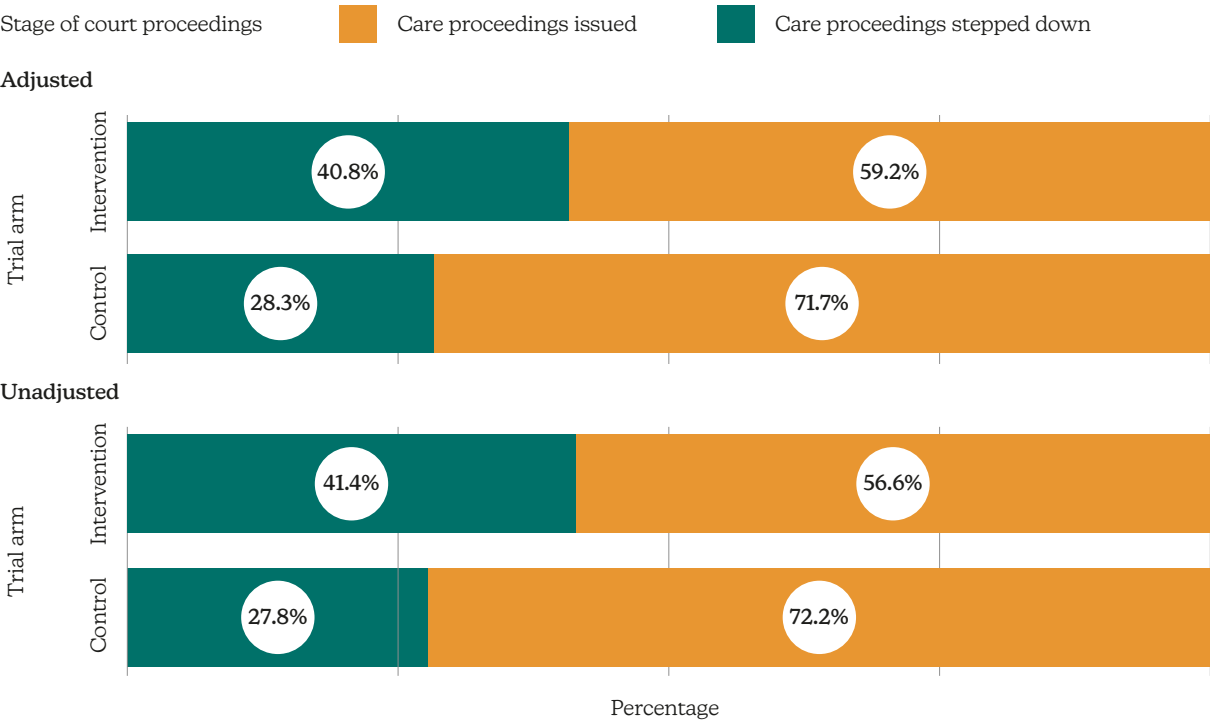
The date court proceedings were issued ranged from October 2020 to May 2022 (end of the reporting period). The mean average time from the pre-proceedings letter being issued to court proceedings being issued was 17 weeks ( $SD=13.7$ ), ranging from 0 to 72 weeks. Step-down letter dates ranged from November 2020 to May 2022. The mean time from pre-proceedings letter to step-down letter was 22 weeks ( $SD=12.1$ ), ranging from 0 to 72 weeks. On average the length of time from court proceedings being issued to the first court ruling was 6 weeks ( $SD=12.5$ ), ranging from 0 to 64 weeks.

**Figure 12. Percentage of children in each stage of care proceedings at 12 months post-pre-proceedings letter by trial arm (unadjusted percentages)**



Court proceedings issued/not yet issued were grouped, and stepped down/not yet stepped down were grouped, to allow for court diversion to be analysed as a binary outcome. A logistic regression was fitted to the data with fixed effects for local authority to predict court diversion at 12 months post-pre-proceedings letter by trial arm and unbalanced predictor variables (care history and legal status at pre-proceedings letter).

**Figure 13. Percentage of children for whom proceedings were either issued (or not yet issued) or stepped down (or not yet stepped down) 12 months post-pre-proceedings letter by trial arm (adjusted and unadjusted percentages)**



According to this model, trial arm was a significant predictor of court diversion at 12 months ( $p < 0.01$ ). The odds ratio was 0.51 (95% CI 0.34–0.75), meaning the odds of care proceedings being issued were 0.51 times lower for children in the intervention arm compared with the odds for the control arm. Clustered standard errors were used to account for nesting within families (Table 16). The adjusted risk ratio was 0.82 (95% CI 0.81–0.84), meaning children in the intervention trial arm were 0.82 times less likely to have proceedings issued compared with children in the control trial arm. The adjusted difference in the proportion of children with proceedings issued or stepped down was 12.5%, with court proceedings issued for 59.2% of children in the intervention arm compared with 71.7% of children in the control arm.

Table 16. Basic regression analysis for court diversion

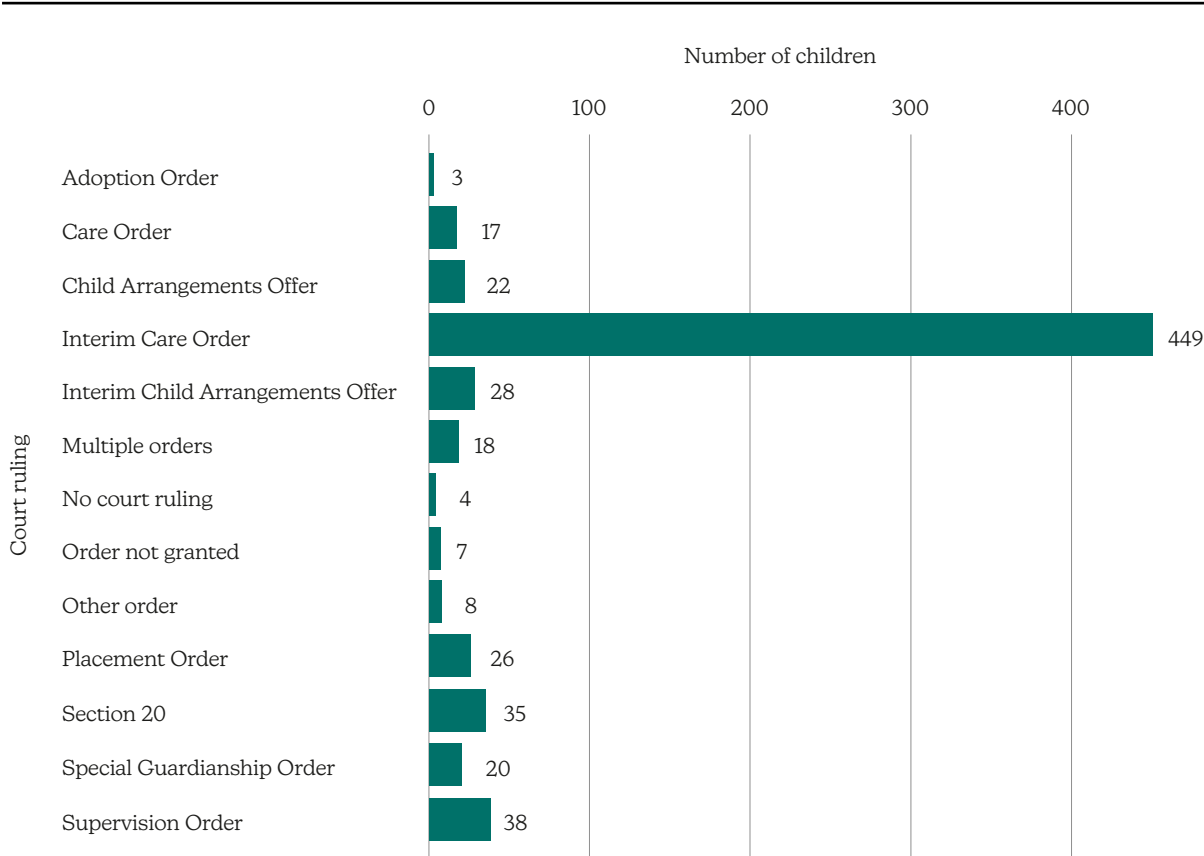
	Coefficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	2.07	0.70	3.44	0.70	<0.01
Trial arm (intervention)	-0.68	-1.07	-0.29	0.20	<0.01
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Birmingham	0.51	-1.57	2.59	1.06	0.63
Bromley	-1.54	-3.06	-0.03	0.77	0.05
Derbyshire	-2.10	-3.59	-0.61	0.76	0.01
Knowsley	15.78	14.30	17.26	0.76	<0.01
Lambeth	-0.81	-3.83	2.21	1.54	0.60
Lancashire	-0.25	-1.81	1.31	0.80	0.75
Leicestershire	-1.74	-3.23	-0.24	0.76	0.02
Lewisham	-0.71	-2.46	1.04	0.89	0.43
Middlesbrough	-0.87	-3.54	1.81	1.36	0.53
North East Lincolnshire	-1.25	-2.76	0.27	0.77	0.11
Northamptonshire	-2.86	-4.67	-1.05	0.92	<0.01
Nottingham	-1.31	-3.01	0.40	0.87	0.13
Plymouth	15.56	14.00	17.11	0.79	<0.01
Redcar and Cleveland	-0.08	-2.05	1.88	1.00	0.94
Rotherham	0.08	-1.99	2.16	1.06	0.94
Salford	-0.68	-2.78	1.41	1.07	0.52
Sheffield	-1.35	-2.90	0.19	0.79	0.09
Shropshire	-1.50	-2.96	-0.03	0.75	0.05
Southampton	-2.29	-3.97	-0.61	0.86	0.01
Sunderland	-2.34	-3.78	-0.89	0.74	<0.01
Care history (unknown)	0.21	-0.50	0.93	0.36	0.56
Care history (yes)	1.18	0.64	1.73	0.28	<0.01
Observations	1251				

We also fitted a logistic regression model with all predictor variables. According to this model, trial arm was a significant predictor of court diversion at 12 months ( $p < 0.01$ ). The odds ratio was 0.47 (95% CI 0.30–0.73), meaning the chances of children in the intervention group having care proceedings were 0.47 times lower than the chances for children in the control arm (Appendix 13). The adjusted risk ratio was 0.83 (95% CI 0.81–0.84).

We then fitted a logistic regression model with the significant and unbalanced predictor variables. According to this model, trial arm was a significant predictor of court diversion at 12 months ( $p < 0.01$ ). The odds ratio was 0.51 (95% CI 0.33–0.78), meaning the chances of children in the intervention group having care proceedings were 0.51 times lower than the chances for children in the control group (Appendix 13). The adjusted risk ratio was 0.85 (95% CI 0.84–0.86). Trial arm remained significant after adjusting for other predictors. Gender, legal status at pre-proceedings and implementation time predicted the outcome in addition to care history, which was already included in the basic model. For example, care proceedings were more likely to be issued (as opposed to stepped down) for children on a Child Protection Plan than for children on a Child in Need plan. However, the inclusion of these predictors did not improve the precision of the odds ratio for trial arm, which remained at 0.51.

For children for whom care proceedings were issued by the end of the reporting period (31 May 2022), 675 (83%) had details of a court ruling (Figure 14). Over half of these children were issued with an Interim Care Order.

**Figure 14. Bar chart of court rulings by 31 May 2022 for children for whom care proceedings were issued (n=675)**



### Survival analysis

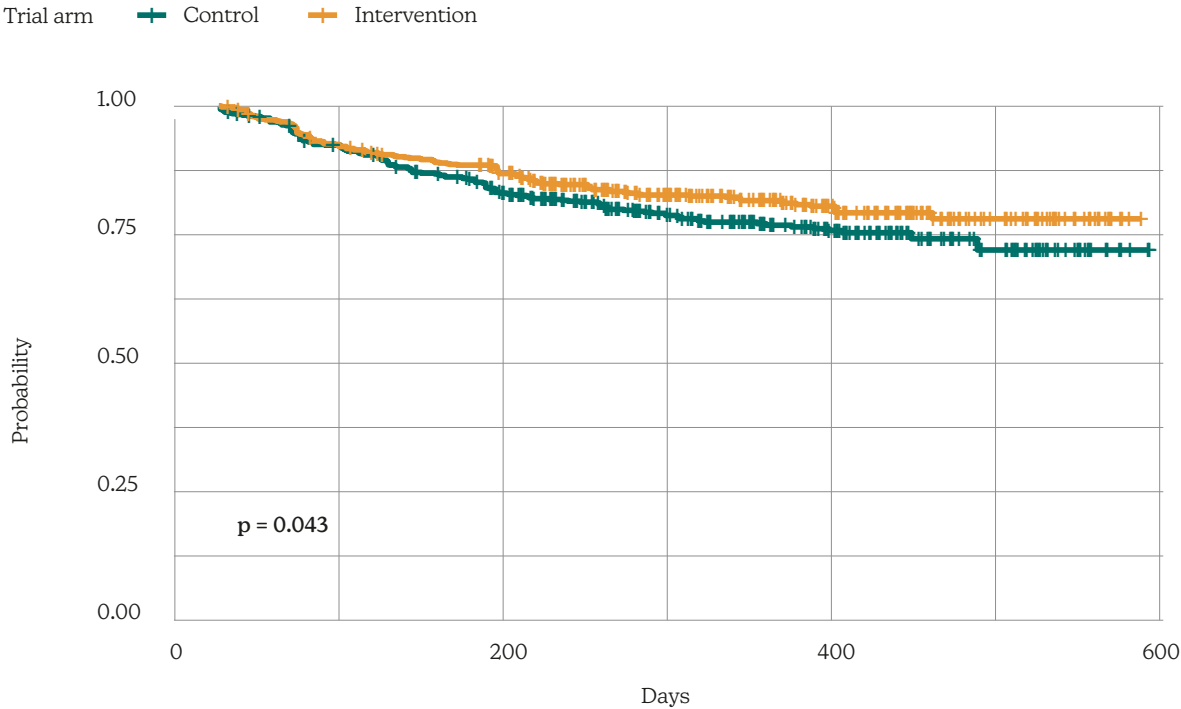
To investigate whether changes in placement happened sooner in one trial arm than in the other, we carried out survival analyses for two outcomes: the decision whether to issue care proceedings (or send a step-down letter); and a child’s change in living arrangements.

We explored the fit of the data on living arrangements (we used the first date of a change in living arrangement after the pre-proceedings letter was sent; n=1,452) using the Kaplan-Meier method (Table 17). The log-rank test was statistically significant (p=0.04) meaning that there was a significant difference between trial arms, whereby children in the control arm were more likely to experience a change in living arrangements by the end of the reporting period (31 May 2022; Figure 15).

**Table 17. Descriptive statistics for Kaplan-Meier survival curve for change in living arrangement by trial arm**

Trial arm	N	Days from pre-proceedings letter to first move or end of reporting period			
		Standard error	25th percentile	Median	75th percentile
Intervention	759	5.6	222	341	452
Control	693	5.9	215	320	418
Both	1452				

**Figure 15. Kaplan-Meier survival curve for change in living arrangement by trial arm**



We also used a multivariate Cox proportional hazard model in order to include covariates.<sup>16</sup> After backwards elimination of non-significant covariates, the resulting model included care history and legal status at entry into pre-proceedings. The p-values for all 3 overall tests (likelihood, Wald and log-rank score) were 0.001 or less, indicating

<sup>16</sup> We have reported the Cox proportional hazards model because it demonstrated better model fit (AIC = 4100.29) compared with the accelerated time failure (AFT) model (AIC = 4860.12).

that the model was significant. The p-value for trial arm was 0.08 with a hazard ratio of 0.81 (95% CI 0.65–1.02) (Table 18). This means that the likelihood of experiencing a change in living arrangement was lower by 19% for children in the intervention trial arm, but not significantly so. The model shows that after adjusting for care history and legal status, the difference between trial arms was not statistically significant.

**Table 18. Multivariate Cox proportionate hazard model of change in living arrangement**

Trial arm	Regression coefficient	Statistical significance	Hazard ratio (95% CI)
Trial arm (intervention)	-0.20	0.08	0.81 (0.65–1.02)
Legal status at pre-proceedings letter date (Child Protection Plan)	0.40	0.06	1.49 (0.98–2.26)
Legal status at pre-proceedings letter date (neither)	-2.52	0.01	0.08 (0.01–0.59)
Legal status at pre-proceedings letter date (unknown)	0.04	0.88	1.04 (0.63–1.70)
Care history (unknown)	-0.83	<0.01	0.44 (0.29–0.65)
Care history (yes)	0.61	<0.01	1.85 (1.42–2.39)

We also looked at survival curves for the progression of decision-making and planning processes (we used the decision to issue proceedings or inform parents that the local authority would not be doing this). Using the Kaplan–Meier method, the log-rank test was not statistically significant (p=0.12), meaning that there was not a significant difference between trial arms (Figure 16).

**Figure 16. Kaplan–Meier survival curve for care proceedings issued or stepped down by trial arm**

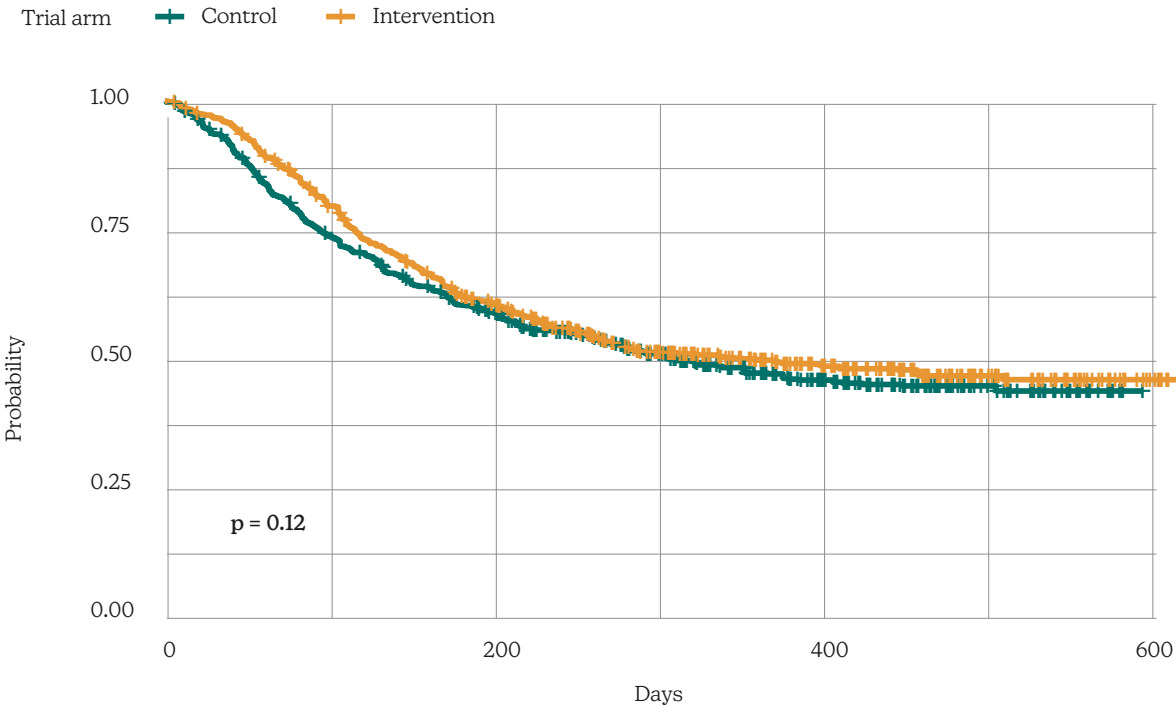


Table 19. Descriptive statistics for Kaplan-Meier survival curve for care proceedings being entered or stepped down

Trial arm	N	Days from pre-proceedings letter to first move or end of reporting period			
		Standard error	25th percentile	Median	75th percentile
Intervention	1273	4.61	110.00	234.00	385.96
Control	1214	4.64	90.00	224.96	355.96
Both	2487				

We also used an accelerated failure time model in order to include covariates.<sup>17</sup> After backwards elimination of non-significant covariates, the resulting model included ethnicity, age, number of children per family, legal status at entry into pre-proceedings, time into implementation of FGCs in local authority of the date on the pre-proceedings letter and care history. The p-value for trial arm was 0.08 and therefore not a significant predictor of time to care proceedings being issued/stepped down (Table 20).

Table 20. Accelerated failure time model of care proceedings being issued or stepped down

Trial arm	Regression coefficient	Statistical significance
Intercept	6.17	<0.01
Trial arm (intervention)	0.11	0.08
Log (scale)	0.09	<0.01
Ethnicity (White)	-0.54	<0.01
Age (4 to 7)	0.24	0.01
Age (8 to 11)	0.21	0.02
Age (12 to 19)	0.36	<0.01
Age (unknown)	0.57	0.07
Number of children (2)	0.10	0.25
Number of children (3)	-0.04	0.68
Number of children (4)	0.04	0.71
Number of children (5+)	0.47	<0.01
Legal status at pre-proceedings letter date (Child Protection Plan)	0.31	<0.01
Legal status at pre-proceedings letter date (neither)	0.10	0.50
Legal status at pre-proceedings letter date (unknown)	0.22	0.06
Time into implementation (5 to 9 months)	-0.37	<0.01
Time into implementation (10+ months)	0.05	0.54
Care history (unknown)	0.33	<0.01
Care history (yes)	-0.17	0.02

<sup>17</sup> We have reported the accelerated failure time (AFT) model because it demonstrated better model fit (AIC = 17,684.49) compared with the Cox proportional hazards model (AIC = 18,246.11).

## FGC delivery – compliance analysis

Local authorities were asked to provide information in the administrative data returns regarding FGC delivery at pre-proceedings for all families randomised into the trial. Daybreak also collected information from local authorities each month concerning more detailed information about FGCs to enable them to support local authorities with delivery. This information was merged to enable analysis of FGC delivery and compliance across both trial arms.

Of the 1471 families about whom we collected data, 376 families received an FGC at pre-proceedings (26%). The majority (n=351, 93%) of these families were in the intervention arm, with n=25 families (7%) randomised to the control arm. This means 47% of families in the treatment group received an FGC (351/740) and 3% of families in the control group (25/736; Table 21). Delivery compliance varied widely across local authorities: for reasons described below, they delivered FGCs to between 24% and 72% of families randomised to the intervention arm. Nine local authorities delivered at least 1 FGC to a control group family: up to 29% of families allocated to the control trial arm.

**Table 21. FGC delivery compliance by trial arm and local authority**

Local authority	Families randomised to intervention arm	Of which were referred to the FGC service	Of which received an FGC	Families randomised to the control arm	Of which received an FGC
Bath & North East Somerset	12	12 (100%)	7 (58%)	10	0 (0%)
Birmingham	59	29 (49%)	24 (41%)	23	0 (0%)
Bromley	23	9 (39%)	7 (30%)	57	0 (0%)
Derbyshire	35	32 (91%)	20 (57%)	33	0 (0%)
Knowsley	22	19 (86%)	15 (68%)	21	6 (29%)
Lambeth	22	11 (50%)	6 (27%)	22	3 (14%)
Lancashire	63	34 (54%)	32 (51%)	62	6 (10%)
Leicestershire	45	22 (49%)	11 (24%)	36	1 (3%)
Lewisham	39	29 (74%)	28 (72%)	38	0 (0%)
Middlesbrough	27	11 (41%)	9 (33%)	27	0 (0%)
North East Lincolnshire	29	24 (83%)	13 (45%)	30	0 (0%)
Northamptonshire	52	49 (94%)	36 (69%)	53	0 (0%)
Nottingham	24	13 (54%)	9 (38%)	24	0 (0%)
Plymouth	10	4 (40%)	3 (30%)	11	2 (18%)
Redcar & Cleveland	32	30 (94%)	17 (53%)	32	1 (3%)
Rotherham	64	60 (94%)	31 (48%)	67	2 (3%)
Salford	25	23 (92%)	9 (36%)	24	0 (0%)
Sheffield	31	25 (81%)	13 (42%)	30	0 (0%)
Shropshire	45	38 (84%)	26 (58%)	50	3 (6%)

Local authority	Families randomised to intervention arm	Of which were referred to the FGC service	Of which received an FGC	Families randomised to the control arm	Of which received an FGC
Southampton	24	10 (42%)*	10 (42%)	26	0 (0%)
Sunderland	57	40 (70%)	25 (44%)	60	1 (2%)
Subtotal, external services** (5)	183	119 (65%)	92 (50%)	210	1 (0.5%)
Subtotal, internal services (16)	557	405 (73%)	259 (46%)	526	24 (5%)
Total, all (21)	740	524 (71%)	351 (47%)	736	25 (3%)

\*This was n=8 FGCs according to our merged (Coram and Daybreak) data set, but as n=10 FGCs were reported to have taken place, we estimated as 10 referrals.

\*\* Local authorities that delivered FGCs through externally commissioned services were Bromley, Leicestershire, Lewisham, Northamptonshire and Southampton.

The proportion of families who were referred to an FGC service following randomisation to the intervention trial arm varied widely across local authorities, from 39% to 94% of families. Internal FGC services were more likely to receive referrals from their colleagues elsewhere in the local authority than external services were to receive referrals (73% compared with 65%). Overall, 71% of families randomised to the intervention trial arm were referred to the FGC service. This means that 29% of families did not receive an FGC referral and could therefore not receive an FGC. Of the 522 families referred, around two-thirds (67%) went on to receive an FGC. A similar proportion of families received an FGC in local authorities where the service was externally commissioned (50%), compared with authorities where the service was provided internally (46%). However, the conversion rate for referrals (the proportion of families who were referred for an FGC and went on to receive one) was higher for externally commissioned services (77%) compared with internally provided FGCs (64%).

### Receipt of FGCs by control group families

For almost half of the 25 families in the control trial arm who received an FGC, the reason for this is unknown or unclear (48%). For 9 families (36%), local authorities explained that a referral was made despite randomisation to the control condition. Specific reasons for this included that the social worker chose to have an FGC, that the FGC was delivered in error and that the FGC was deemed by the local authority a necessary tool to help the child and family. In 4 cases (16%) the family requested an FGC.

### Non-receipt of FGCs by intervention group families

For the majority of the 53% of families who were randomised to the intervention trial arm but did not receive an FGC (389/740), this was because the family declined or refused the offer of an FGC (n=226, 58%). Local authorities explained specific reasons for this, including parent(s) not consenting, a lack of engagement from the parent(s), parents feeling that they did not need an FGC and parent(s) not wanting to involve their wider family network in an FGC.

Other reasons varied. For 55 families (14%), the reason for non-delivery was unknown or unclear. For 39 families (10%) an FGC was not held due to a change in the child(ren)'s care circumstances, such as the case escalating to proceedings or the child(ren) becoming looked-after. For 27 families (7%) an FGC was reportedly deemed unviable by the local authority, for reasons that included social workers being unable to identify a family network and the identified network not wanting to take part in an FGC. For 8 families (2%) a referral for FGC was made but no further information was provided. For 6 families (2%), the FGC was scheduled to take place but had not by the end of the reporting period. Four families (1%) did not receive an FGC because they moved.

Finally, for 24 families (6%) the local authority reported that no referral was received, with reasons including that the social worker did not engage or failed to send through a referral. Our analysis of data gathered from local authorities by Daybreak suggests this is an underestimation, given that we found FGC services did not receive referrals for 29% (n=218) of families in the intervention trial arm. We can reasonably assume that for the 55 families (14%) where the local authority did not report a reason, the reason was that a referral was not made. It may also be that other reasons reported by local authorities (e.g. emergency case escalation, FGC unviable) concern families who also did not receive a referral to the FGC service.

### Exploratory analysis of randomisation compliance and primary outcome

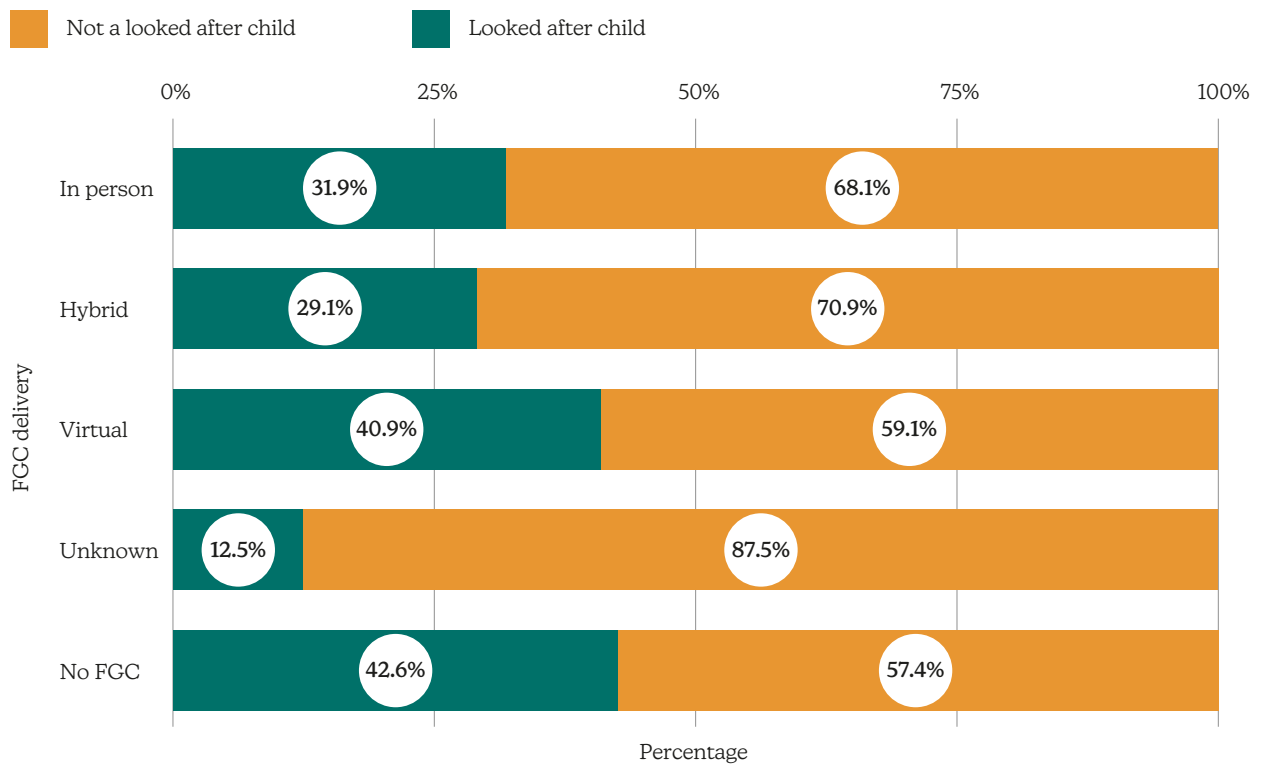
To explore the role of compliance in the analysis of the primary outcome, we ran a per protocol analysis and excluded all cases that were not compliant with randomisation (e.g. excluded intervention trial arm cases that did not receive an FGC and control trial arm cases that received an FGC). The results of this regression analysis are provided in Appendix 16. The trial arm (and, in this analysis, receipt of an FGC) was not a significant predictor of care status at 12 months post-pre-proceedings letter ( $p=0.22$ ), with a risk ratio of 0.76.

We also looked at outcomes for children who received the FGC in different formats: hybrid, in-person or virtual. These plots provide some evidence that children in families that receive in-person or hybrid FGCs, compared with children in families that receive virtual FGCs:

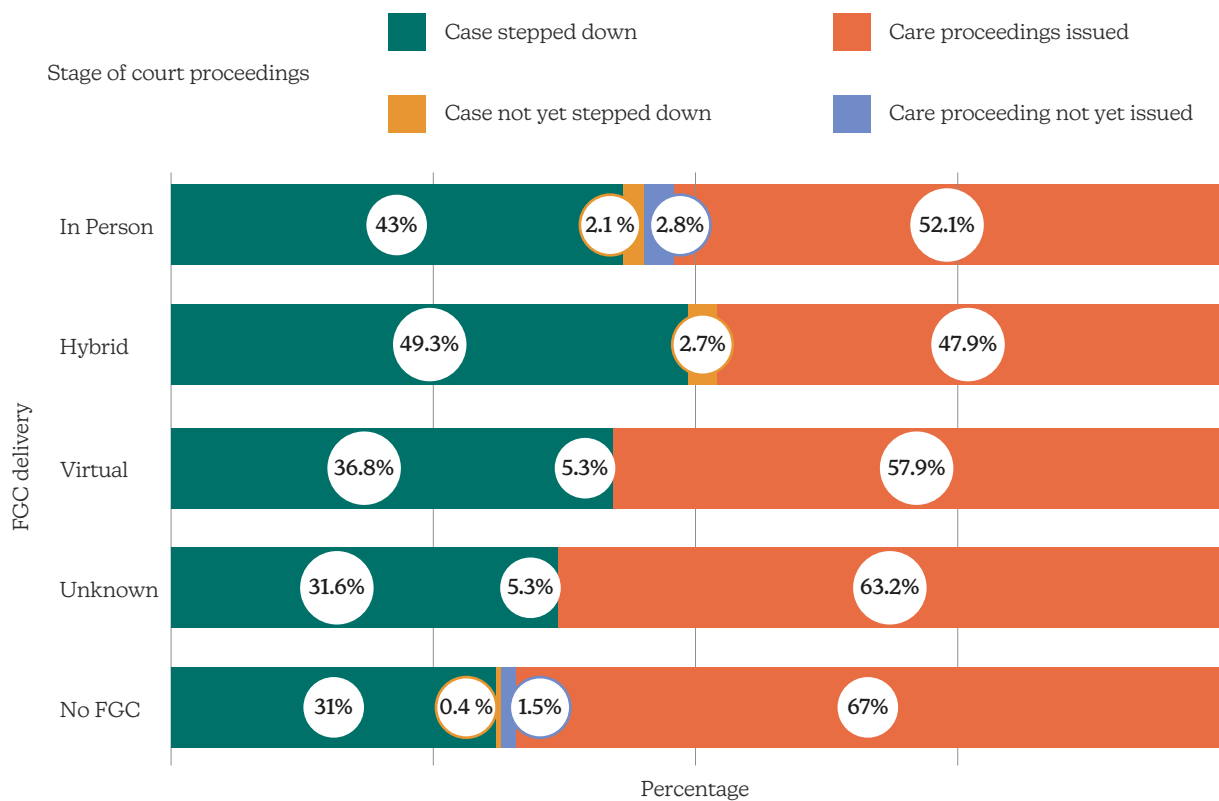
- Are less likely to be looked-after at 12 months post-pre-proceedings letter (Figure 17)
- Are more likely to have the case stepped down and less likely to have care proceedings issued (Figure 18)
- Are more likely to have experienced a change in their living arrangements by 12 months (Figure 19)
- Are likely to have spent less time in care (Figure 20).

It is important to note that this analysis is exploratory and often relies on small numbers; further research is needed. These plots describe patterns in the data but do not test for significant or causal differences between delivery formats. There may be related and potentially explanatory factors associated with delivery format that influence outcomes, such as virtual FGCs occurring for children with family networks outside the UK.

**Figure 17. Stacked bar chart showing the percentages of child legal status by FGC delivery format (n=1231) (unadjusted percentages)<sup>18</sup>**

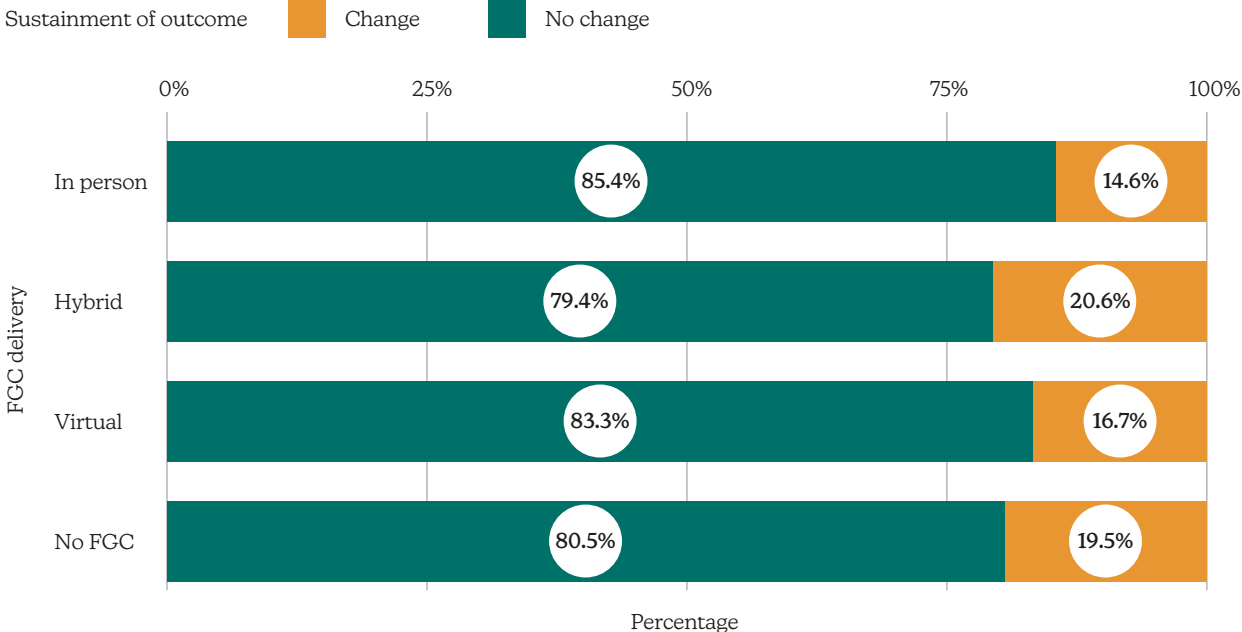


**Figure 18. Stacked bar chart showing the proportions of court proceedings at 12 months by FGC delivery format (n=1251) (unadjusted percentages)**

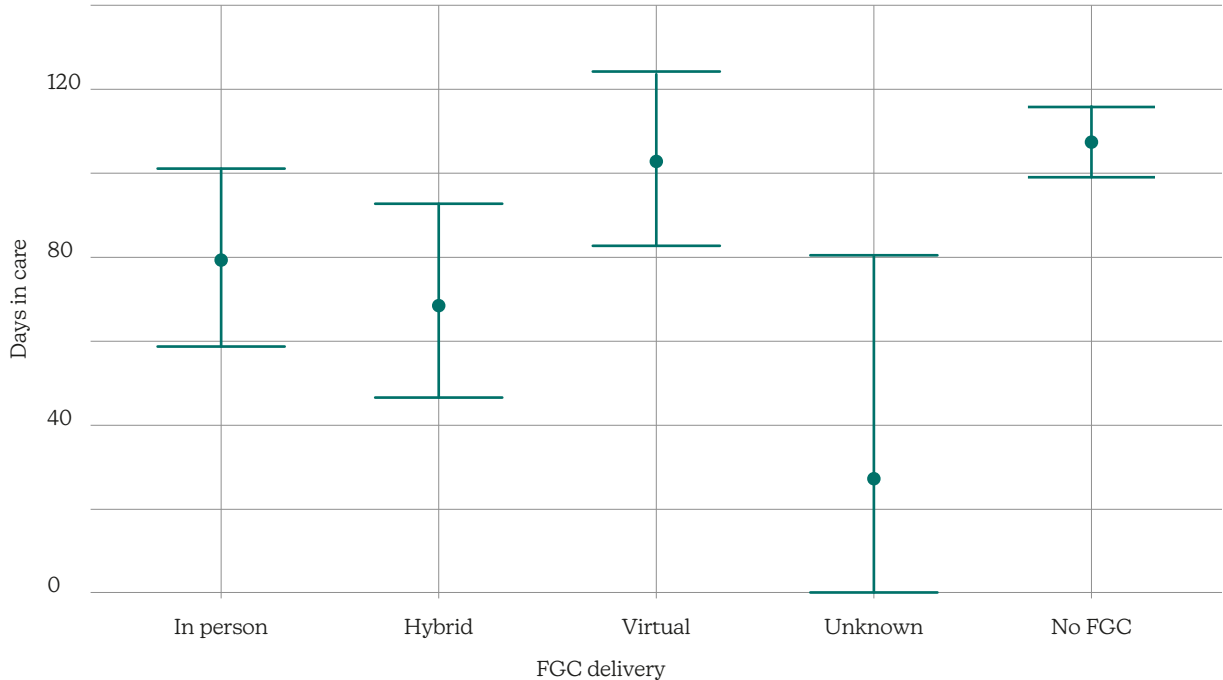


<sup>18</sup> We do not present adjusted proportions for the graphs in this section regarding FGC delivery, because these were not included in regression analyses

**Figure 19. Stacked bar chart showing the proportions of sustainment of outcome at 12 months by FGC delivery format (n=1511) (unadjusted percentages)**



**Figure 20. Forest plot showing mean days in care by FGC format (n=1433)**



**Complier average causal effects (CACE)**

We computed the complier average causal effects (CACE) using instrumental variable regression by two-stage least squares to provide an estimate of the causal effect for FGCs. The full output for these regression analyses is reported in Appendix 17.

For our primary outcome, the probability of becoming looked-after was estimated to be 16 percentage points lower for compliers ( $p < 0.01$ ). To compare the CACE with the ITT effect, we calculated the adjusted predicted probabilities from the ITT logistic regression for trial arms. This showed that the probability of becoming looked-after was nine percentage points lower for children in the intervention arm. As expected, the average complier effects were larger than the ITT effects estimated in the main analysis.

The likelihood of care proceedings being issued (compared with stepped down) was 28 percentage points lower for treatment compliers ( $p < 0.01$ ). For the ITT analysis, the likelihood of care proceedings being issued was 13 percentage points lower for children in the intervention trial arm.

For time spent in care, like the ITT effect for trial arm ( $p = 0.06$ ), the CACE was not significant ( $p = 0.07$ ). However, the coefficient is larger whereby children who receive an FGC spend on average 36 fewer days in care compared with children who do not. In the ITT analysis, children in the intervention arm spent on average 19 fewer days in care compared with the control arm.

Sustainment of living arrangements was not statistically significant for either the ITT ( $p = 0.97$ ) or CACE analysis ( $p = 0.96$ ). The CACE analysis estimated that the probability of a change in living arrangements was one percentage point higher for treatment compliers. For the ITT analysis, the adjusted predicted probabilities for the trial arms were near equivalent; the difference was just 0.2%.

**Table 22. CACE analysis for key outcomes at 12 months post-pre-proceedings letter\***

Outcome	CACE coefficient	CACE	Of which received an FGC
Care status	-0.16	<0.01	1227
Sustainment of outcome	0.01	0.96	401
Time spent in care	-36.45	0.07	1433
Court diversion	-0.28	<0.01	1251

\* Court diversion was analysed at a single time point because all proceedings issued or stepped down by the end of the reporting period.

### Cost analysis findings

The cost of usual support for families during care proceedings is unknown, and so despite the estimates that follow we do not know whether the costs of this programme represented an increase or decrease in expenditure. We asked local authorities to refer intervention group families for FGCs in addition to usual services, so an increase is more likely, but we do not know how much more than usual practice it costs to deliver FGCs or whether alternatives like family network meetings are more or less expensive. Pre-proceedings was described to us as an intense period of working with families, so costs will certainly not be zero and may be substantial.

As planned, we requested cost data from local authorities as part of our online follow-up survey in September 2021. We asked for their actual costs of implementing FGC at pre-proceedings stage in the first year of the programme, broken down by staff costs, overheads and other costs, and by start-up and ongoing costs. Costs reported included for IT, mileage, venues, interpreters, office space, room bookings, use of advocacy and refreshments.

### Data quality and completeness

Where data was incomplete, we assumed those providing data were typical and estimated totals. Eighteen local authorities provided data on the costs they incurred (Appendix 18); 3 did not (Leicestershire, Lewisham and Southampton). Local authorities that did not provide costs data were not representative because they made up three of the five that externally commissioned the FGC service at pre-proceedings stage. Daybreak provided costs data for the three local authorities they were commissioned to run the service for: Bromley, Lewisham and Southampton.<sup>19</sup> Barnardo's provided cost data for Leicestershire and Northamptonshire.<sup>20</sup>

The completeness and quality of data provided varied. There was no standardised template available for us to draw on, and assumptions, inclusions and exclusions may have varied. Some local authorities did not provide a breakdown of the costs they reported. The majority did not specify whether costs were start-up costs or ongoing costs. In most cases, costs were assumed to be ongoing costs when this was not specified, other than training costs, which we treated as staff start-up costs. Local authority funding to take part in the programme was not ring-fenced and, given COVID-19, in-house FGC teams may have been deployed to other work. We are not confident that we received the costs of FGC team staff time spent specifically on FGCs. Dates varied but most of the months covered by the data fell in the financial year 2021/22.<sup>21</sup>

### In-house FGC service start-up costs

We received start-up costs from 9 local authorities with an in-house FGC service, totalling 23% of total costs for the 16 that provided cost data on in-house FGC services. Middlesbrough reported the lowest start-up costs of £533, which they referred to as "staff start-up costs". They did not report overhead or other start-up costs. They also reported the lowest ongoing costs. Maximum start-up costs were £321,550 and the mean average was £46,323. Some local authorities had existing FGC services, albeit not at pre-proceedings stage, but the data quality issues discussed above may also explain some of this variation.

Training costs included staff time to attend the introductory training delivered by Daybreak. Seven local authorities with an in-house service reported training costs

<sup>19</sup> These costs covered the whole 14-month programme; we have multiplied by 0.857 (12/14) to estimate the cost of delivery for 1 year.

<sup>20</sup> These costs covered financial years; we have added half the 2020/21 cost to half of the 2021/22 cost.

<sup>21</sup> One local authority provided data for April 2020–March 2021 and April 2021–October 2021. Data from both financial years was used based on the assumption that there were unlikely to be reported costs before delivery started in October 2020. One local authority provided overlapping figures for September 2020–October 2021 and 2020/2021; only figures for September 2020–October 2021 were analysed.

totalling 1.5% of total costs for these local authorities.<sup>22</sup> This ranged by local authority from £225 to £5000, with a mean average training cost of £2408.

**Example of cost breakdown in one local authority**

North East Lincolnshire had typical total costs and provided a breakdown, so we report their first-year costs here in more detail for illustrative purposes (Table 23). The vast majority of their expenditure (89%) was staff costs, in line with the overall picture, where 94% of total costs were staff costs.

**Table 23. Example cost breakdown in one in-house FGC service**

Cost type	Detail	Cost	Percent of cost
<b>Start-up costs</b>	Training, accommodation, mileage	£2400	2%
<b>Ongoing costs</b>	Salaries - 2.5 full-time equivalent FGC coordinators	£84,476	70%
	Oversight of management	£23,795	20%
	Admin costs	£9800	8%
	FGC hospitality	£98	0.1%
	Mileage	£350	0.3%
	Translation	£210	0.2%
<b>Total</b>		<b>£121,128</b>	<b>100%</b>

**External FGC service costs**

Three local authorities commissioned Daybreak to provide their FGC service and two local authorities commissioned Barnardo’s to do this. In these cases, both the commissioner and commissioned organisation incur costs. The costs reported by local authorities will have included activities such as management time to negotiate the contract, monitoring and troubleshooting referrals, legal oversight and administration of referrals and data returns.

Local authorities that commissioned their FGC services externally had a higher conversion rate from referral to delivery; 77% of families referred for an FGC were delivered an FGC, compared with 64% for local authorities that used internally commissioned services (Table 21). There may be a quality–cost trade-off. That is, these tentative findings suggest that external services may provide a higher-quality service, based on conversion rates, but initial analysis also suggests at a higher cost than the alternative of in-house provision. However, the data quality and completeness issues discussed above mean further research is needed on the relative costs of in-house compared with externally commissioned delivery.

<sup>22</sup> e local authority included training among aggregated “non-staffing costs”. As this was not broken down, we did not include this in the analysis of training costs.

## Unit costs

The following estimates should be treated with caution due to the data quality and completeness issues described above.

Looking across all 21 local authorities, the costs to local authorities of delivery in the first year, plus the amount spent to outsource the five outsourced services and the costs incurred by these providers, gives a first-year total programme cost of £2,780,560.

Unit costs, meaning the cost to deliver one FGC at pre-proceedings stage through this programme, varied widely by local authority around a mean of £8911 in the first year, including start-up and ongoing costs and the costs to both local authorities and external providers of delivering both in-house and outsourced FGCs (Table 24).<sup>23</sup> Per child, this is £5242.<sup>24</sup> This unit cost applies to this programme, not the use of FGCs at pre-proceedings stage in general, for which no benchmark costs are available, to our knowledge. One evaluation (Mason et al., 2017) found that expanding FGCs for more families in Leeds in 2015/2016 cost £2418 per FGC.

Due to the factors set out below, it is reasonable to assume that FGCs could be introduced at pre-proceedings stage more cheaply than this programme achieved. We are aware of one factor reducing the unit cost of the current programme:

- The widespread use of hybrid and virtual FGCs. This reduction will not have been large, given staff costs made up such a large percentage of total costs.

But we are aware of several factors increasing the unit cost:

- COVID-19 reduced the numbers of families in the programme, by delaying the programme start, with no corresponding reduction in budget, and reducing the number of families entering pre-proceedings
- A substantial minority of families randomised to the intervention group (29%) were not referred to the FGC service by the local authority
- The unit costs we have calculated include start-up costs
- The unit costs we have calculated look only at the first year of delivery (FGC meetings cannot be delivered from day one of a new service)
- Our evaluation design may have increased unit costs in that FGC services could only reach a maximum of 50% of eligible families, reducing FGC services' ability to achieve economies of scale.

We carried out sensitivity testing on our cost estimates. Using different assumptions about the cost data provided by local authorities, such as assuming start-up, overhead and other costs where these were not provided, and excluding outliers, we calculated a range of unit costs from £6914 to £10,904 per FGC, or £4079 to £6433 per child. During planning of the programme, a unit cost of £2300 per FGC was assumed. Daybreak told us they thought this would be closer to the actual cost of delivering FGCs at pre-proceedings stage in the absence of the factors listed above.

<sup>23</sup> The total first-year programme cost, £2,780,561, divided by 312, the number of FGCs in the first year.

<sup>24</sup> There were an average of 1.7 children per family, so 312 was multiplied by 1.7, equalling 530.4. The total first-year programme cost was then divided by 530.4.

Table 24. Unit costs incurred by local authorities and providers, including start-up costs

Type of FGC service	Mean unit cost	Interquartile range	Lowest unit cost	Highest unit cost	Cost per FGC per child
Total (in-house and external)	£8911	£4645–£14,524	£598	£20,730	£5242

Note: the cost per FGC per child is calculated using the mean number of children per family in our sample. 1.7. Data quality concerns mean these numbers are subject to uncertainty.

## Cost-benefit analysis

Based on Department for Education data for 2019/20, Alma Economics (2022) estimated the cost to a local authority of being in care per year per child at approximately £80,000 (the weighted average of the cost of a foster placement and the cost of a residential placement), including staffing and placement costs (Department for Education, 2020). The estimated cost to local authorities of a Child in Need plan in 2018/19 was £2250 per year; a Child Protection Plan was estimated to cost local authorities £17,950 per year (Department for Education, 2020). As these are children’s social care costs to the local authority, central government costs and other local public services costs, such as pupil premium and free school meals, are not included. We have assumed that the 21 local authorities in this evaluation are typical of England and have adjusted annual cost estimates of different child legal statuses for inflation to 2021 (Table 25) using the consumer prices index for housing (Office for National Statistics, 2022).<sup>25</sup>

The average Child in Need episode is reported to last just under seven months and the average Child Protection Plan just under 10 months (Department for Education, 2020). Based on Department for Education data for 2012/12 and 2016/17, Alma Economics (2022) estimated the duration of an average episode of being looked-after at three years.

Table 25. Costs to local authorities of legal statuses adjusted for inflation

Source: Coram analysis based on Department for Education (2020) and Alma Economics (2022).

Legal status	Average cost to local authority per child per year	Average episode duration	Average cost to local authority per child per year, adjusted for inflation to 2021
Child in Need	£2250 (2018/19)	7 months	£2369
Child Protection Plan	£17,950 (2018/19)	10 months	£18,899
Looked-after child	£80,000 (2019/20)	36 months	£82,820
None of these (including Early Help)	Unknown	Unknown	Unknown, £0 assumed
Additional cost of becoming looked-after from Child in Need			£80,451
Additional cost of becoming looked-after from Child Protection Plan			£63,921
Additional cost of becoming looked-after from neither			£82,820

<sup>25</sup> Adjusted by 1.7% (2019), 1% (2020) and 2.5% (2021), for a total increase of 5.3% (Child in Need and Child Protection Plan costs) and 3.5% (looked-after child costs).

We calculated the average additional cost avoided in the first year by a local authority by referring a family of a child in pre-proceedings for an FGC. To do this, we used our primary outcome analysis of the care statuses of children in the intervention and control groups, which found that children in the intervention group were less likely to be looked-after than children in the control group at 12 months post-pre-proceedings letter. As this was an 8.59 percentage point difference, in this analysis we attribute 8.59% of the total cost saved from a child not becoming looked-after to FGCs (Table 26).

**Table 26. Calculation of financial benefit in first year for local authorities of referring a child for an FGC**

Legal status	Average cost to local authority per child per year	Average episode duration	Average cost to local authority per child per year, adjusted for inflation to 2021
Child in Need	£80,451	12%	£9645
Child Protection Plan	£63,921	55%	£34,942
None	£82,820	33%	£27,618
<b>Total</b>		<b>100%</b>	<b>£72,205</b>

**Saving to local authorities if intervention makes children 8.59 percentage points less likely to become looked-after (8.59% of £72,205) = £6202**

Saving to local authorities if intervention makes children 8.59 percentage points less likely to become looked-after (8.59% of £72,205) = £6202

Children at entry into pre-proceedings were assumed to be on Child Protection Plans (55%), Children in Need (12%) or neither (33%), in line with the baseline characteristics of the sample reported in Appendix 7. We assumed that if children were not taken into care their legal status remained the same as when they entered pre-proceedings. Based on this, we calculated the average additional cost avoided by a local authority by referring a family of a child in pre-proceedings for an FGC. This was a cost of £960 per child in the first year following the pre-proceedings letter, accounting for all costs related to an FGC referral (Table 27). This is a return of £1.18 on an investment of £1 in the first year (no discounting applied). Unobserved costs are incurred for children who do not go into care, which we do not consider here, such as allowances paid to special guardians. As our findings above on unit costs detail, it is reasonable to assume that FGCs could be introduced at pre-proceedings stage more cheaply than this programme achieved, which would increase our headline net benefit of £960 per child. The findings are subject to uncertainty, due to the data quality concerns discussed above.

Our estimates are broadly in line with Rodger et al. (2020), who analysed FGCs delivered as part of a project in one local authority in the Children’s Social Care Innovation Programme and found a return of £3.40 for £1 invested.

Table 27. Cost-benefit analysis summary, per child, all 21 local authorities, 2021 prices

Benefit	-	cost	=	net benefit or cost
<b>Including start-up costs</b>				
£6202	-	£5242	=	£960 benefit
avoided cost due to lower risk of becoming looked-after for 1 year		unit cost incurred by local authorities and external providers of an FGC per child		in first year following pre-proceedings letter (a benefit:cost ratio of 1.18:1)
<b>Excluding start-up costs</b>				
£6202	-	£3562	=	£2640 benefit
avoided cost due to lower risk of becoming looked-after for 1 year		unit cost incurred by local authorities of an FGC per child (in-house services only)		in first year following pre-proceedings letter (a benefit:cost ratio of 1.74:1)

Notes: children at entry into pre-proceedings are assumed to be on Child Protection Plans (55%), Children in Need (12%) or neither (33%), in line with the baseline characteristics of the sample reported in Appendix 7. We assume an FGC referral makes becoming looked-after 8.59 percentage points less likely, in line with the central impact estimate of our primary outcome. Cost is mean unit cost in the first year of an FGC service, divided by 1.7 (average number of children per family) and is subject to uncertainty due to data quality concerns.

## Process evaluation findings

### 1. Was the project implemented as planned?

#### COVID-19

Following months of negotiations, preparation and training, the programme was due to start in April 2020, when the COVID-19 pandemic and national restrictions brought programme delivery to a halt. Even when the majority of local authorities were able to restart delivery in autumn 2020, the way FGCs were delivered and the context within which they were delivered had changed markedly.

Within the Daybreak team there was, and continues to be, a strong view that virtual family meetings are very different and cannot be compared to face-to-face FGCs. And while virtual family meetings played a useful role within the context of the COVID-19 pandemic, there are key elements of FGCs that cannot be delivered online, such as private family time. Daybreak highlighted a range of difficulties with virtual family meetings, such as families' access to suitable technology, issues around confidentiality, the possibility that some family participants may be coerced not to speak or not to speak the truth by others not visible to online participants, and family members using connectivity issues (e.g. such as pretending that their internet connection had dropped out) to avoid difficult conversations.

Some parents at interview talked about some of the technological issues in accessing the FGC, for example:

**“[Connecting to the FGC was a] Nightmare. My mum could not get it [video conferencing] working. I had my mum over WhatsApp.” Parent**

However, Daybreak also recognised that online meetings can make participation easier – for example, by enabling FGC coordinators to more easily meet with social workers and health visitors, and for geographically remote relatives to attend online FGCs.

In the context of the COVID-19 pandemic, the majority of local authorities made the decision to adapt to online FGCs, and only one local authority (Sunderland) delivered face-to-face FGCs throughout the programme.

As well as altering how FGCs were delivered, the pandemic also had an impact on referral rates. The reasons given for this were multiple, but included higher thresholds as children’s services delayed instigating pre-proceedings, as well as an increase in cases going directly to care proceedings and thereby bypassing the pre-proceedings stage and FGC process. The kinds of families entering pre-proceedings during the pandemic may not have been typical, in the 21 local authorities in this programme or in general, but we do not have evidence on this.

Compared with Daybreak’s work with other local authorities, the programme also had a lower conversion rate of referrals to FGC meetings. During the planning stage, Daybreak had hoped for a 70% acceptance rate among families referred for an FGC (compared with 90% in other local authorities). However, the programme achieved an average 56% acceptance rate (ranging from 23% to 78%). Again, multiple reasons influenced this figure, including whether the FGC service was delivered in-house or by an external provider. The increased use of agency social workers during the pandemic was also identified by Daybreak as one factor influencing the lower take-up. Agency workers, and the consequent high turnover, meant that social workers had less-well-established relationships with families. In Daybreak’s view, families may have been less likely to take up the offer of an FGC if they were discussing it with a social worker they did not know well. The short-term nature of agency staff meant the social worker would not have built up a working relationship with the FGC to help referrals, and the social workers would not have been able to develop their knowledge of the FGC service in the local authority.

### **Other factors affecting implementation**

There were occasional examples cited of deviations from the intended model of FGC provision. One social worker we interviewed reported that a case-holding social worker in their local authority was running FGCs. This deviates from FGC good practice standards of using an independent coordinator<sup>26</sup> and would add to the barrier, discussed below, related to social worker lack of time and capacity.

Another example was social workers in Leicestershire contacting families directly, which went against what was stated in the letter before proceedings. Daybreak thought that the social worker may not be able to persuade the family of the benefits of the FGC in the same way as the FGC team:

<sup>26</sup> Family Rights Group: “The FGC coordinator is independent.” (n.d.a) <https://frg.org.uk/family-group-conferences/fgc-accreditation/> [Accessed on 10 May 2023].

**“In that letter [the letter before proceedings] ... basically informed the family that they would be contacted by the FGC service, not by the social worker. What happened in Leicestershire was that process was completely ignored and the social workers were contacting families, informing them about the FGCs, not sounding very convincing and the families were like ‘Nah, we do not want it.’”**

### **Daybreak**

Generally, we found there was high fidelity and high adherence to good standards of practice. Where known, for almost all FGCs local authorities reported to us, in their data returns, that the facilitator was trained to the standards set by Daybreak (n=339, 99%).

Through attending discussions with the local authorities facilitated by Daybreak we learned about further compliance issues. In a January 2022 discussion, two local authorities mentioned their FGC service receiving more internal referrals at the earlier stage of child protection, to avoid having to take part in the programme, with its risk of a “do not refer” randomisation outcome. Our data does not allow us to explore how widespread this trend may have been.

There were also few deviations from the protocol. In the follow-up survey we asked local authorities if they were aware of any families who entered pre-proceedings during the programme but were not randomised into either group, to explore whether the project was implemented as planned. The majority (60%, n=12) reported that there were none. A quarter answered at least one (n=5) and 15% were not sure (n=3).

The five local authorities that reported families that had not been randomised referred to 2–15 families each, an approximate total of 36. Reasons included delays, concerns about prior FGC involvement, technical issues, the death of a child and risks to staff. One local authority cited previous randomisation – a legitimate exclusion criterion as set out in the protocol.

## **2. What were the barriers and enablers to successful implementation?**

### **Barriers**

**Social worker time, capacity and workload** can be a barrier and a motivator to refer to a separate FGC service. Some at interview viewed FGCs as reducing social worker workload but for some it was extra paperwork and organising that they had to do on top of busy schedules.

From Daybreak’s perspective, **COVID-19** was a barrier to successful implementation and take-up. Daybreak saw FGC referrals going down, thresholds for pre-proceedings going up, cases going straight to care skipping pre-proceedings and reduced FGC acceptance rates due to increased agency social worker use because of the pandemic. They did acknowledge it made some processes easier – e.g. FGC coordinators’ meetings with social workers, health visitors etc.

The pandemic was also mentioned in the FGC coordinator and facilitator survey (technology issues, venue availability, vulnerable family members unable to attend, increased workload) and in the local authority follow-up survey (reported by 48% of local authorities). For example:

**“COVID has been our biggest barrier due to meetings needing to be face-to-face and not having rooms available. Pressure in the system due to COVID has also impacted on social work capacity, therefore delay in receiving referrals.” Local authority**

Delays in referrals and staffing issues including **staff turnover** and workload were also mentioned in the local authority and facilitator surveys. A lack of engagement from social workers and a lack of engagement from families were also mentioned by local authorities:

**“The social workers sometimes cannot see the value of FGC, particularly with complex families who are difficult to engage in services. Social workers’ workloads are a barrier to them being able to meet appropriate timescales.” Local authority**

**Timing and timescales** were identified by facilitators as barriers for families and local authorities. In addition to the issues resulting from the pandemic, FGCs took place at a time when families were engaging with a lot of statutory services, and thus at a busy, stressful time, with families **“overwhelmed”** and FGCs **“seen as another meeting”** (emphasis added). Many facilitators in the survey expressed the view that it would have been better if the FGC service had been brought in earlier – “things have escalated to a point in some cases that it’s not always been [possible] to turn things around”. They also reported this view being expressed by some families themselves. Many facilitators felt that it was the timescales outlined by the project that were being worked to rather than the families’, that it was then “professional-led” or “service-led”, and that these could not be met and/or deference to the families’ needs was needed instead.

Daybreak felt the **disconnect between the FGC programme and children’s social care** was a barrier. It sometimes meant that families were not being referred to FGC teams within the local authorities: “... legal panels were happening, families were being randomised and referrals were not coming in”. As part of setting up the programme, Daybreak organised initial meetings in each local authority, asking them to invite representatives from departments that would be involved in the programme. This, for example, included senior representatives from their legal department, the Legal Gateway Panel, Children’s Services and the FGC service, if it existed. While Daybreak felt these initial meetings helped attendees better understand the programme, Daybreak highlighted the importance of Children’s Services and the FGC service trusting each other and working together to each fulfil their part of the process. This included Children’s Services making the FGC service aware of families as early as possible. Where this cooperation did not happen, making referrals and thus arranging FGCs within target timescales was a struggle. Daybreak estimated that this disconnect affected between one-third and half of local authorities.

The programme **lacked leverage to make local authorities deliver what they had agreed to do**. Without legally binding contracts, individual local authorities lacked clear deliverables and responsibilities, and there was little that could be done to compel local authorities to deliver the programme. Though memoranda of understanding were developed to address this, the lack of legal leverage for Daybreak, What Works for Children’s Social Care, the Department for Education and Coram continued to be an issue.

**Barriers for families** also included a lack of understanding around the procedures, the number of other meetings, visits and professional engagement families at pre-proceedings had, the FGC being offered too late, families viewing an FGC as “pointless” because they felt that the case was heading to court anyway and facilitators reporting that the FGCs did not feel voluntary for families. Barriers related to the family network included a lack of family and friends, difficulties getting the extended family to engage and reluctance to share information with them.

For some, the **evaluation design** was a concern. Local authorities were asked as part of their initial expression of interest to guarantee that they were not already offering FGCs at the pre-proceedings stage, but the judiciary in some areas had not been involved from the outset and consequently did not appreciate the need to randomise families for the evaluation. An initial delay and concern was caused in Plymouth and Bath and North East Somerset by the reported unwillingness of family judges to sanction the random selection of families for FGC referrals. Our administrative data and surveys show some instances of undermining the protocol and we heard this may have been due to opposition to randomisation, not previously used as part of evaluating FGCs in the UK, among some social workers.

### **Enablers**

From the very beginning, local authorities demonstrated a good level of interest in the programme, and the initial expression of interest generated responses from 36 local authorities – 21 of whom went on to deliver FGCs in the pre-proceedings stage. While some difficulties were encountered throughout the programme, the most disruptive being the COVID-19 pandemic, the vast majority of participating local authorities demonstrated a **high level of commitment to the programme**. The fact that most local authorities planned to continue to deliver FGCs following the programme demonstrates this commitment.

In our survey of local authorities, **internal communication and support** was most commonly cited as a facilitator to implementation (by 57% of local authorities). Additional staff resources, increasing awareness of FGCs, training and external support and having an existing or previous FGC service in place were mentioned. Two local authorities mentioned benefits of commissioning an external FGC service, such as their independence, while one local authority felt having an in-house service helped them to swiftly iron out issues.

FGC facilitators most commonly cited **relationships and communication** as making an FGC successful, with “trust”, “honesty” and “transparency” often mentioned. Taking time to build rapport, good preparation, empowering the family, being family-led, network engagement and qualities of the facilitators were also mentioned. The importance of this is highlighted by FitzSimons & McCracken’s (2020: 4) analysis of the Children’s Social Care Innovation Programme, where “the centrality of building consistent trusting relationships, and providing time for this” was common among the most effective projects.

Coordinators in our survey were generally positive about the support they had received, referencing management, social worker, administrative and local authority support, but some had a mixed experience with social workers and some reported minimal support.

We also asked coordinators about family engagement with FGCs, which they reported was generally positive. Both support for staff and family engagement can be seen as enablers to successful implementation.

Daybreak believed that the **bringing together of local authorities** as part of the programme was an enabler to implementation. The “hub”, made up of the 21 local authorities, met every two months. Daybreak witnessed the local authorities sharing ideas and challenges at meetings and also heard of local authorities continuing conversations outside meetings. Local authorities met approximately bi-monthly to share learning and best practice, discuss challenges and introduce new ideas. Daybreak believed the forum became an effective way to share information about the programme with participating local authorities and for partners such as the Department for Education and Coram to attend and answer questions. The peer support the forum provided was described as a beneficial element of the programme, as it allowed local authorities to get to know each other and share experiences.

In Daybreak’s opinion having the **buy-in of senior management** in the local authorities helped with the implementation and uptake of the programme:

**“You can get the odd rogue social worker that doesn’t want to do it [refer to FGCs] but that is never going to get traction if you have the senior manager supporting it [FGCs] and I think that showed clearly in Northamptonshire [who had senior management buy-in from the start of the programme].”**

Senior decision-makers who understood and championed FGCs played an important role in facilitating progress on the ground. Daybreak described that FGCs often have a low profile within local authorities, so having a senior manager on board can make things happen that are difficult to achieve without this support and attention. In one local authority, which initially refused to provide data, the Department for Education directly contacting the director was instrumental in resolving the issue.

Finally, one social worker noted at interview that each social work team has a **named FGC contact** person they can ask questions of, and this was working:

**“Like if we’ve got a family that’s unsure about FGC, they’re [the FGC worker] able to come out with us [to visit to the family] and explain it [the FGC] because I feel like they probably have the best understanding of what it is and how it can be facilitated.”**

### **3. Were FGCs carried out as planned and to Daybreak standards? Why or why not?**

Daybreak thought that generally local authorities adhered to good practice standards. The Daybreak team oversaw activity through its monthly data collection and was in regular contact with project managers in the FGC teams, and perceived areas were delivering to a high standard and a recognisable model.

Our case study interviews showed some deviations from good practice models of FGCs, such as a case-holding social worker coordinating FGCs. As described above, in one local

authority, for a time, social workers were approaching families about FGCs, rather than the FGC service, and a succession of families refused the offer until the issue was addressed.

There was variation across local authorities in when and how FGCs were delivered. Local authorities had very different set-ups, starting points (see Appendix 2) and levels of senior buy-in. When we asked local authorities about how their FGC at pre-proceedings stage was organised, they most frequently mentioned a specific in-house FGC team. Five local authorities commissioned an external service: Bromley, Lewisham and Southampton commissioned Daybreak; Leicestershire and Northamptonshire commissioned Barnardo's. In total across the baseline and follow-up surveys, 13 local authorities mentioned offering family meetings to families who were not referred for an FGC, and 8 did not. We know from interviews that these included family network meetings. The number of FGC coordinators, facilitators and practitioners reported ranged from one to eight, with an average of five and a total of 59 for the 13 local authorities that answered this question.

We also asked how the FGCs offered at pre-proceedings stage compared with other FGCs in their authority, which they told us were offered at Child in Need, Child Protection, Early Help and reunification stages. Of the 20 that responded:

- Eight (40%) reported FGCs at other stages were the same as pre-proceedings FGCs
- Six (30%) reported that they do not offer FGCs at other stages
- Six (30%) reported that they were different.

The differences were not substantial.

The difference most frequently highlighted was the shorter timescale of FGCs offered at pre-proceedings stage. Coordinators also commented on this in their surveys. Local authorities mentioned that pre-proceedings FGCs were more structured. The impact of COVID-19 on usual practice was commented on, for example, causing more FGCs to be completed virtually.

Coordinators mentioned reduced flexibility in the questions asked and some felt that this, alongside fixed timescales, compromised the family-led approach of FGCs. Some also felt that the voluntary aspect of FGCs was compromised because families felt that they had to have an FGC because they were at pre-proceedings stage.

#### **4. Were any adaptations made to FGC model that was planned? If so, what, why and where?**

Adaptations were made due to COVID-19 in all local authorities, though in Sunderland provision remained entirely in-person. Respondents to all three of our surveys commented on the impact of COVID-19, particularly its leading to a shift to online or hybrid FGCs. Some saw this as a positive because it accommodated those who would not have been able to access an in-person FGC, and saved time and money. Others commented on technological barriers to virtual/hybrid FGCs, and some believed face-to-face worked better.

In the local authority follow-up survey, 43% reported no changes had been made to their FGC model. Changes reported were minor. Local authorities mentioned staff changes or increasing staff capacity. Two local authorities mentioned delivery by Barnardo's and three mentioned delivery by Daybreak, echoing answers to the baseline survey. One explained

that they had commissioned an external agency but due to COVID-19-related delays the service was brought in-house.

Two local authorities commented on delays due to COVID-19. One local authority modified the Daybreak model “slightly”, adapting the questions to meet the needs of the family and the plan document to make it more accessible. Five local authorities (24%) spoke about offering FGCs in other areas of children’s social care.

We asked FGC coordinators about modifications to the FGC model. Some reported that timescales could not be met and that timescales were led by the service or professionals.

## **5. What, if any, changes were made to usual services?**

The programme occurred within a wider context of disruption to social work practice as a result of COVID-19 and a reduction in the number of cases entering pre-proceedings, in line with the England-wide trend (Figure 1). However, business-as-usual professional practice in and of itself during pre-proceedings did not undergo notable changes that would affect interpretation of our findings (what we are comparing FGCs with).

In the baseline survey, the majority of local authorities (57%) indicated there were no known changes to usual practice. The changes reported were minor, such as moving towards strength-based safety-planning, making the pre-proceedings process and letter sent out to parents clearer, FGC coordinators sitting on the Legal Gateway and COVID-19 adaptations.

When asked about usual practice for families at pre-proceedings in the baseline survey, local authorities most frequently referred to meetings, support and referrals. Local authorities also provided assessments and were involved in planning for children’s care. Legal Gateway Panels and reviews were mentioned.

Nine local authorities (43%) did not anticipate, at the time of the baseline survey, that a referral to FGC would affect usual practice. Anticipated impacts included raising the FGC profile, improving engagement with wider family members at an earlier stage and identifying a more consistent approach.

Only one local authority anticipated that families entering pre-proceedings over the course of the programme may become part of other evaluations. Eight local authorities were unsure. Four local authorities were unsure about the impact on services for families who were participating in more than one evaluation and one did not think there would be an impact.

In the follow-up survey, the majority of local authorities (52%) reported no changes to usual practice for the control group and 43% reported no changes for the intervention group. Like in the baseline survey, any changes described were minor, such as offering alternative types of meetings for the control group and encouraging the use of advocacy and translation services for those receiving FGCs.

One local authority reported no specific changes for the control group, but that wider assessment work was being managed in the PLO stage, and there was a shift in focus of PLO as an intervention rather than an evidence-gathering process, but this was for families in both the control and intervention group.

## **6. How much did staff and families think that what they experienced during pre-proceedings (usual services or usual services plus referral for FGC) impacted their outcomes?**

Previous evaluation of the same model of FGCs at pre-proceedings stage (Munro et al., 2017) found satisfaction among families with their FGCs. Among our eight case study families, four of whom had an FGC and four did not, experiences were mixed. Good usual services from social workers who got on well with families were felt to make a big difference. None of the family members or social workers we interviewed felt the four FGCs had made a major difference to outcomes, though one family network meeting was felt to have had a positive effect and one FGC was considered reassuring by the social worker – a chance for the children to express their views.

The two core questions for discussion in FGC meetings require parents to consider two possibilities: the possibility of their children no longer living with them, or of remaining at home. The idea of losing your children is hard to face, and it was difficult for some of those we interviewed to acknowledge the risk. An alternative option to going into care is for the child/ren to live with friends or relatives, something FGC attendees are prompted to explore. This may not be parents' preference, but was seen as better than becoming looked-after. A social worker in case study 2 acknowledged that the network attending FGCs could offer unexpected support not otherwise available.

We saw the importance of the timing of these difficult choices. The best moment for an FGC may differ for different people, for practical and psychological reasons. It would have been difficult for the mother in case study 4 to engage with an FGC, had it been offered to her, but after her baby's birth she turned things around. The children in case study 6 had been on Child Protection Plans for four years before entering pre-proceedings. The family received a helpful family network meeting, which their mother felt should have come a couple of years earlier.

On the whole, earlier was believed to be better than later. In some case study families, social workers felt an earlier FGC would have been helpful. Several local authorities in our survey also mentioned their view that FGCs should be offered before pre-proceedings stage and/or the need to research the impact of this.

Our survey findings were more positive about the perceived impacts of FGCs than our interview findings. Over three-quarters (76%) of local authorities thought FGCs at pre-proceedings stage had made a difference to how the local authority worked with families. Among the 76% answering positively, one local authority spoke about the role of FGCs in working with families in a relationship-based way, and in seeing children and families as partners so that care proceedings are a last resort.

Other reasons local authorities thought it had made a difference included:

- It increased the FGC offer and raised the FGC profile
- It targeted families early in the PLO process
- It had involved exploring and engaging families' wider networks
- It had led to a more structured approach to FGCs.

Several reported that they believed the FGC had led to positive outcomes for families, such as reducing the number of families progressing to court, and children being placed with family members instead of into local authority care.

In the follow-up survey, we asked local authorities about the impact of staff knowing whether families participated in an FGC on decisions about whether to proceed to court. We asked this to find out whether the unblinded nature of the trial affected the outcome. The decision to escalate or de-escalate cases lies with social workers and their managers, who are aware of which families have taken part in an FGC. They may, for example, feel that families who have taken part in FGCs have done more to change than families who have not.

Four local authorities (19%) reported that there was no impact and one reported that there was only a limited impact. Four were unsure about the impact and two did not provide a substantive answer. Some spoke about FGCs more generally, reporting staff were more likely to delay court proceedings to allow an FGC to take place, staff wanted to give families the opportunity to contribute to a plan and identify alternative carers, and that an FGC is always voluntary but if a family declines an FGC and other interventions, it may reflect negatively on them and affect the decision on whether to proceed to court.

Coordinators were overwhelmingly positive in their views on the impact of FGCs on families' experience at pre-proceedings, particularly in relation to families feeling empowered, included and listened to and the inclusion of the wider network to provide support.

We also asked coordinators if they thought that FGCs affected outcomes. A large majority reported positive outcomes, such as cases not progressing to court or stepping down from PLO after an FGC, and the plans being followed or implemented. They also wrote of children avoiding being taken into care, whether remaining with the parents or within the wider family. The reasons given predominantly concerned family support being identified and agreed. One respondent identified that it is the process that allows families to "explore their own strengths and skills", which were sometimes previously unknown to the social worker. It was seen by the families themselves as an opportunity to provide evidence to local authorities and courts of their plan, strengths and network support. A few respondents reported FGCs "giving the family a voice".



# LIMITATIONS

- The programme took place during a time of COVID-19 restrictions. Interpretation of our findings is complicated by the use of hybrid and virtual FGCs due to COVID-19. Given our intention-to-treat design, we included any kind of FGC in our analysis and thus analysed a mixture of FGC types, which may have had different impacts or impacts of different sizes. Our analysis of the outcomes of different formats was exploratory and based on small numbers.
- Our study did not collect detailed data on business-as-usual services, such as other meetings attended by families during pre-proceedings, in either trial arm. The quality, quantity and nature of these, and their costs, are likely to vary across local authorities. This hinders our ability to interpret the impact, cost and process findings. We asked local authorities to offer FGCs in addition to usual services, but do not know whether the FGCs substituted for these, in whole or part.
- Numbers of families were lower than anticipated. First, fewer families entered pre-proceedings than the previous trend (the 2017/18 peak in care proceedings fell 21% to the 2021/22 number). Second, due to COVID-19 the start was delayed, so the programme length was 22% shorter at 14 months rather than the planned 18 months. Finally, fewer local authorities took part (three local authorities dropping out from the original 24 was a loss of 12.5%). These have particularly impacted our sample size for outcomes at 18 months. Together they might have been expected to approximately halve the sample, and indeed 1511 families were randomised compared with an expected 3300. This means we can have less confidence in our results than we otherwise would, and there was less scope for detailed subgroup analyses.
- COVID-19 slowed court proceedings, complicating our interpretation of child outcomes. For example, in the administrative data from local authorities, we saw almost ten times more Interim Care Orders than Care Orders. We saw a snapshot of child outcomes at a moment in time, during an ongoing process, rather than a longer-term picture of cases. The speed of court proceedings should be considered in interpreting our findings around court diversion, in terms of how FGCs may affect the speed of decision-making.
- There were occasional instances of non-compliance with the trial protocol among staff, such as social workers who did not refer intervention group families for an FGC, or who referred control group families to the FGC service. But under half of families in the intervention trial arm received an FGC, most often due to declining the offer. Future research may wish to further explore reasons for non-uptake and approaches to maximise allocation adherence.
- Like a previous study of FGCs that attempted to gather primary data from families going through pre-proceedings (Munro et al., 2017), the amount of data we were able to gather on parents' views was too low for a meaningful analysis. At 24%, the response rate to our text messages was low. But local authorities also did not provide mobile phone numbers for many parents (we received fewer mobile phone numbers – 1256 – than the number of families randomised, which was 1511).
- Data quality varied widely across local authorities. There were substantial rates of missingness in the data, as well as instances of implausible or out-of-range values. We

were unable to explore all inconsistencies in the data or query all unlikely combinations of outcomes. An example of this is the discrepancy between data provided to Coram and data provided to Daybreak regarding the delivery of FGCs; numbers varied across local authorities between the two data sets. Owing to the nature of our data collection templates and the cleaning assumptions made when dealing with missing data, it is likely that our report overestimates stability or continuity of the outcome variables. Data quality may therefore explain why trial arm was a significant predictor of care status at 12 months after pre-proceedings letter, but not of time spent in care. Time spent in care requires a more consistent and detailed account of the dates at which a child's legal status changed during the programme, whereas legal status at 12 months requires a legal status to be provided for a date 12 months from the pre-proceedings letter.

- Some of the variables we sought seemed challenging for local authorities to obtain, such as step-down dates or mother's previous care history. Some variables did not exist in the form in which we asked for them. For example, we analysed court diversion by looking at the dates of care proceedings being issued or step-down letter. However, for children with section 20 orders, there was not always a date for either. In addition, over the course of the programme we became aware of other variables that it would have been useful to analyse or use in data cleaning, such as the date at which a child moved outside of a local authority and multiple court rulings.
- There are limitations to the data categories we used. For example, parents and adoptive parents were categorised under the same living arrangement. Therefore, if a child lived with their birth parents at the time of the pre-proceedings letter and with adoptive parents 12 months later, this was treated as a sustainment of outcome (we used survival analysis to compensate for this).



# DISCUSSIONS AND CONCLUSIONS

We assessed the impact on child and parent outcomes of families entering pre-proceedings being randomly allocated to be referred or not referred for an FGC. We found children in referred families were statistically significantly less likely to be looked-after 12 months later. We found that these children were less likely to enter care proceedings and that they spent less time in care in the six months after pre-proceedings were issued.

We did not find significant findings for all of our secondary outcomes, such as whether living arrangements were the same at 6 or 12 months after care proceedings were issued or stepped down, or any of our outcomes at 18 months. We suspect this is because these samples were too small to be sufficiently powered.

By controlling for other possible explanations, RCTs are more able than other designs to attribute the differences they find to the programme or service evaluated. But this is only one study and contrasts with the findings of previous international RCTs on entry into care systematically reviewed by Nurmatov et al. (2020). However, our findings do provide evidence in favour of the continued use of FGCs at pre-proceedings stage for families in England. The findings are also evidence in favour of introducing FGCs at pre-proceedings stage, for local authorities not already using them.

FGCs are only one of the interventions and approaches local authorities can use during pre-proceedings. They are currently non-statutory, though the Independent Review of Children's Social Care recommended a new legal entitlement to family group decision-making before a case reaches the Public Law Outline (MacAlister, 2022). Our study provides the best available evidence about the effectiveness of FGCs and should be carefully considered by central and local government decision-makers as they respond to the Review.

The family should feel part of the pre-proceedings process, a busy and difficult time when local authorities are required, in statutory guidance, to enable wider family members to contribute to decision-making. So it is positive to find 53% of parents, whether or not they were referred for an FGC, felt completely or very involved in planning their children's care in the first two months of pre-proceedings. Only a minority responded to our text messages and they are likely to be unrepresentative, confirming the difficulty of primary data-gathering at scale from families going through pre-proceedings.

Becoming looked-after does not necessarily mean a child is happier or unhappier, safer or less safe, but it is an important status in itself, and an important influence on many other child outcomes. A child may become looked-after following an FGC because friends or family step forward to become foster carers, or because appropriate carers in the family network can not be identified. Being looked-after is also expensive and local authorities are obliged to seek value for money. Our analysis of the costs and benefits to local authorities finds FGCs, used in this way, are cost-effective. There may be other financial benefits to public services such as courts, which we have not estimated as part of our cost evaluation.

Like so many evaluations of programmes introduced since March 2020, it is difficult to say how generalisable our findings are to practice in general. Although we found that the intended model of FGCs was generally adhered to, and standards of practice in delivering the FGCs generally high, FGCs were developed as an in-person model. COVID-19

complicated this. More than half of the FGCs we evaluated involved some level of remote attendance, rather than this being a rare occurrence as expected. FGCs are intended to be different from other meetings held during pre-proceedings, with their different attendees, independent facilitators and venues, refreshments and family-only time. Online versions lose some of this and may look and feel more similar to business as usual. So larger effect sizes might be expected in impact evaluations of entirely in-person meetings.

The 21 local authorities had very different set-ups and starting points. Some started from scratch; others had well-established services but were new to using FGCs during pre-proceedings. There was variation in when and how FGCs were delivered, with 16 in-house and five outsourced services. Services were mostly too small to provide statistically significant findings alone: our aggregate findings cover all 21 areas so are likely to conceal variations in effectiveness.

This three-year programme and its evaluation have involved many hundreds of families and professionals. FGC services worked hard and we found that they believe in the work they do. This belief may help to explain our findings: it is possible that the knowledge among staff that a family received an FGC, rather than the FGC itself, had an impact. In any unblinded trial, the fact that all parties were aware of whether families were in the intervention or control group brings the possibility that the mere act of offering families services, rather than the services themselves, has an impact – for example, by making people more hopeful, responsive or engaged. However, our qualitative findings do not support this idea.

Some services expressed initial concerns about randomising half of families to not being offered an FGC. These families were not deprived of anything they would otherwise have received. FGCs were not previously used at pre-proceedings stage in the 21 local authorities. Nor was anything forced on them: FGCs were voluntary. These features reassured those with doubts about RCTs, an evaluation design in widespread use in education and other sectors but not yet in children's services. Local authorities generally abided by the trial protocol.

We have demonstrated the feasibility of further RCTs in the sector. We succeeded in gaining administrative data from all 21 local authorities on almost all randomised families. This allowed us to complete the largest RCT by far of FGCs in the world, by number of families. But, despite the clear definition and importance of looked-after status, going to court and with whom a child lives, the data we received was often difficult for officials to extract. Much of what we received was patchy and incomplete. The low number of children available for analysis may help explain our finding of no difference in some outcomes. These data issues would prove a major limitation to any study other than an RCT, where any noise and error are expected to be equal across both trial arms.

This study has found evidence that FGCs had a positive impact: children in families referred for FGCs were less likely to go into care in the months after entering pre-proceedings. Although we cannot be sure about the validity of the overall picture in which this difference is seen, and we do not know what happens next in children's lives, this

study presents important new evidence that offering FGCs at pre-proceedings reduced entry into care.



# RECOMMENDATIONS

## **For future policy and practice, we recommend:**

- Local authorities should continue the use of FGCs at pre-proceedings stage where services already exist; and consider their introduction in other areas
- The Department for Education, local authorities and partners should consider the findings of this study in their response to the Independent Review of Children's Social Care
- Local authorities should ensure good support is available during pre-proceedings from social workers with positive relationships with families
- Local authorities should improve their data on usual services during pre-proceedings and on FGCs, so that further research can increase understanding about how they work, for whom and in what context
- Local authorities should provide high-quality support to FGC services, including good internal communication, referral mechanisms and senior management buy-in
- FGC services should be delivered consistently and to high standards, including independent facilitators and providing families with private time during the meeting
- FGC services should seek ways to maximise effective working with social workers, as high workloads and turnover were seen as barriers to implementation.

## **We recommend future research on:**

- Where, when, how and how many FGCs are used with families in England
- The impact of FGCs at different stages, especially earlier stages such as for children on Child Protection Plans; and of different types of FGCs: in-person, virtual and hybrid FGCs; and of in-house and externally commissioned FGCs. Any future RCT should mitigate the risk of attrition between randomisation and referral by automatically informing the FGC service each time a family is randomised to the intervention group. It should minimise the data burden by avoiding duplication between the evaluator and delivery partner's monitoring requests. Future evaluations should also gather data on the nature and intensity of the usual services delivered to families
- The costs of FGCs and of usual support during pre-proceedings, including the relative costs of in-person, virtual and hybrid FGCs; and of in-house and externally commissioned FGCs
- The actual and perceived level of involvement in the process by children and wider family
- Any impacts of FGCs on the wider family – our outcomes looked only at impacts on the immediate family – and other impacts on children, such as wellbeing
- The mediators and moderators of the differences we have found between trial arms, which may include, for example, the level of support from the wider family and the quality of the relationship with the social worker.

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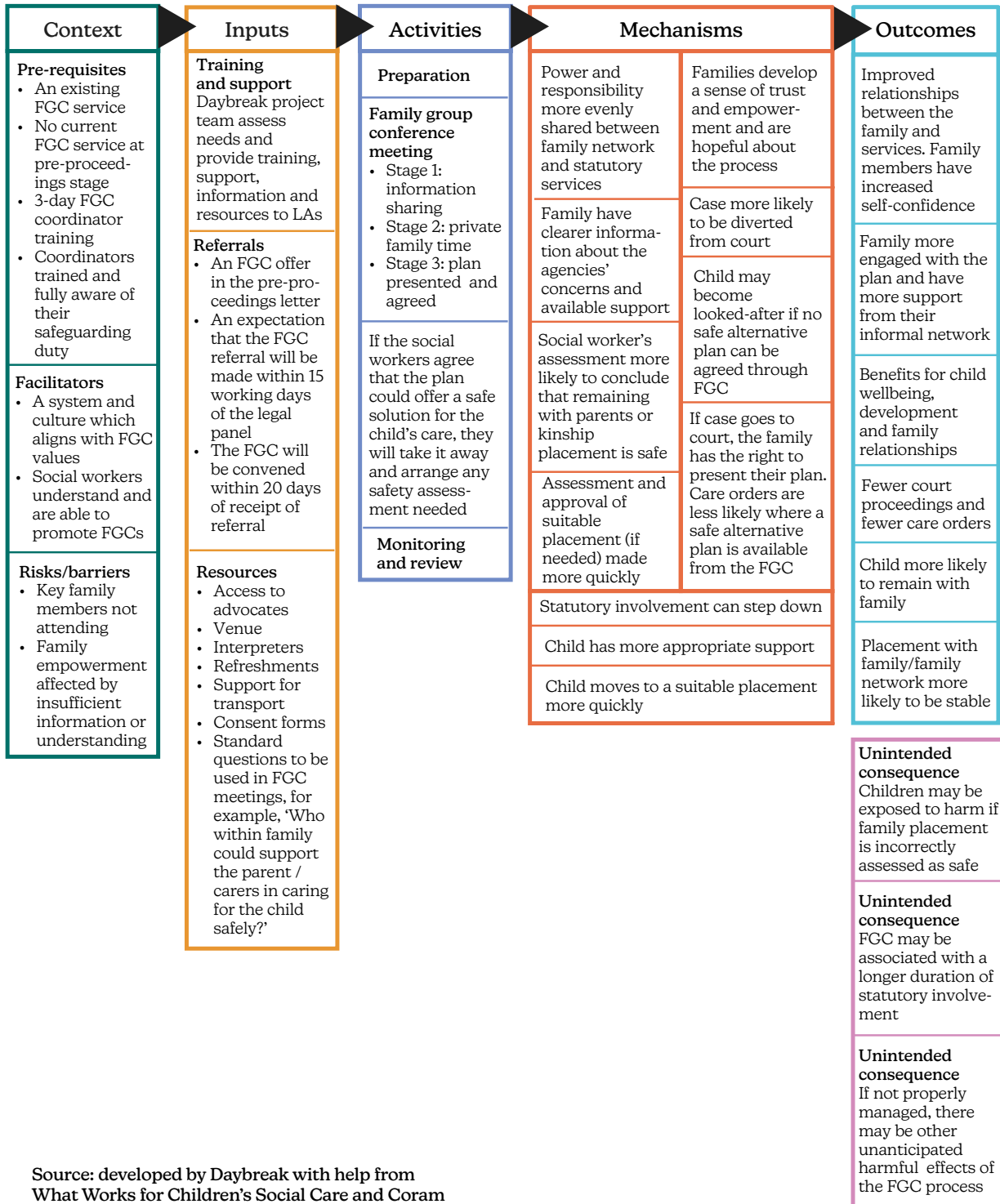
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# APPENDICES



# APPENDICES

## Appendix 1. Logic model



Source: developed by Daybreak with help from What Works for Children's Social Care and Coram and published in Taylor et al. (2020).

## Appendix 2. Details of use of FGCs by local authorities in the programme

Local authority	Pre-programme category	Category at start of programme	Category at end of programme
<b>Source</b>	Application form, June 2019	Responses to baseline survey (October 2020–July 2021)	Responses to follow-up survey (September–December 2021), Department for Education data (December 2021) and Daybreak data (March 2022)
<b>Bath and North East Somerset</b>	Sporadic	Sporadic	Embedded
<b>Birmingham</b>	Embedded	Embedded	Embedded
<b>Bromley</b>	Sporadic	Embedded	Embedded
<b>Derbyshire</b>	New	New	No FGCs
<b>Knowsley</b>	New	New	Embedded
<b>Lambeth</b>	Sporadic	Embedded	Embedded
<b>Lancashire</b>	Sporadic	Sporadic	Embedded
<b>Leicestershire</b>	New	New	No FGCs
<b>Lewisham</b>	New	New	Embedded
<b>Middlesbrough</b>	New	New	Embedded
<b>North East Lincolnshire</b>	Embedded	Embedded	Embedded
<b>Northamptonshire</b>	New	New	Embedded
<b>Nottingham</b>	New	New	Embedded
<b>Plymouth</b>	Embedded	Embedded	Embedded
<b>Redcar &amp; Cleveland</b>	New	New	Embedded
<b>Rotherham</b>	New	New	Embedded
<b>Salford</b>	Embedded	Embedded	Embedded
<b>Sheffield</b>	Embedded	Embedded	Embedded
<b>Shropshire</b>	Embedded	Embedded	Sporadic
<b>Southampton</b>	Embedded	Embedded	Embedded
<b>Sunderland</b>	Embedded	Embedded	Embedded

## Appendix 3. Local authority motivations for taking part in the programme

In the baseline survey we asked local authorities what they hoped to achieve through participation in the programme to explore how much impact staff thought the project would have. Twelve local authorities (57%) referred to enhancing the service offered at pre-proceedings stage.

**“My hope for this project is that we can introduce FGCs as a statutory service to families, that we can promote this in [local authority] by making it more widely accessible and by increasing the take-up of FGC in Early Help.”**

**“On a long-term basis, it would be fantastic if the FGC model would be encouraged as part of the mandatory social care package in the future.”**

Ten local authorities (48%) commented on evidencing the impact of the FGC model on outcomes to inform future practice.

**“We are hoping to evaluate the impact that the FGC has upon keeping children safe in their families, whether we have been able to divert from proceedings and analyse the cost impact upon proceedings and children in long-term care.”**

**“[We hope] to establish the benefit of FGCs at pre-proceedings stage and how this might alter outcomes for children.”**

Nine local authorities (43%) referred to improving outcomes, such as keeping families together.

**“We hope to prevent cases progressing to court from pre-proceedings. We hope that families will engage better with [pre-proceedings process] as they feel listened to and empowered. We hope for better outcomes for children as they have more contact with family and can remain within the family network.”**

**“We are hoping for a reduction in the number of care proceedings having to be initiated to safeguard a child; instead we want to support families to ensure the right support is in place to reduce risks.”**

## Appendix 4. More details on emergency cases that bypassed pre-proceedings

It is difficult to assess how far this trial has achieved complete coverage of families entering pre-proceedings during the programme in participating local authorities. Cafcass publishes local authority-level monthly data on public law care applications, but not all families entering pre-proceedings go into proceedings, and some applications will relate to families who entered pre-proceedings before the programme began. We therefore had to gather our own data to help us put this evaluation in context of the proportion of families in scope.

In the September 2021 follow-up survey we asked local authorities how many emergency cases they had dealt with since the start of the programme (see table on next page). These are children who went straight into care, their families bypassing pre-proceedings, and so were not randomised. Ten local authorities (48%) reported that there had been multiple emergency cases, 2 reported that there had been no emergency cases, 6 were unable to provide this data and 3 did not answer the question. Per local authority, the number of families reported ranged up to 79 and the number of children reported ranged up to 58. Using the mean number of children per family in the sample, we estimated that the approximate total number of emergency case families was 357 and the approximate total number of children in emergency cases was 607 in the 12 local authorities that reported on this. The number of emergency case families relative to the number of randomised families appears to be very high in some local authorities. Assuming that the local authorities that did not provide this data had, on average, the same number of emergency cases as the 12 that did provide this data, a total of 625 families and 1062 children would have been emergency cases across the 21 local authorities.

Bath and North East Somerset	14	24	61%
Bromley	45	77	100%
Derbyshire	18	30	26%
Lancashire	79	134	95%
Leicestershire	9	15	12%
Middlesbrough	0	0	0%
North East Lincolnshire	25	43	42%
Nottingham	52	88	118%
Plymouth	30	51	158%
Rotherham	34	58	27%
Sheffield	0	0	0%
Sunderland	51	87	49%
<b>Total</b>	<b>357</b>	<b>607</b>	<b>80%</b>

Note: where local authorities provided number of families, we calculated the number of children by multiplying by 1.7, the mean number of children per family in the sample. Where they provided number of children, we calculated the number of families by dividing by 1.7. Sunderland did not specify families or children, but we analysed their response as the number of families. Leicestershire reported “less than 10” families, reported here as 9. In a later answer, Lancashire explained that advice from their legal department was that families who have had children removed previously should go straight to care proceedings.

# Appendix 5. R packages used for data cleaning and analysis

- Readxl (Wickham & Bryan, 2022)<sup>27</sup>
- Tidyverse (Wickham et al., 2019)<sup>28</sup>
- Lubridate (Grolemund & Wickham, 2011)<sup>29</sup>
- ggplot2 (Wickham, 2016)<sup>30</sup>
- pander (Daróczy, 2022)<sup>31</sup>
- psych (Revelle, 2022)<sup>32</sup>
- summarytools (Comtois, 2022)<sup>33</sup>
- Hmisc (Harrell Jr, 2022)<sup>34</sup>
- dplyr (Wickham et al., 2022)<sup>35</sup>
- ivreg (Fox et al., 2021)<sup>36</sup>
- broom (Robinson et al., 2022)<sup>37</sup>
- effectsize (Ben-Shachar et al., 2022)<sup>38</sup>
- lmtest (Hothorn et al., 2021)<sup>39</sup>
- sandwich (Zeileis et al., 2022)<sup>40</sup>
- stringr (Wickham, 2022)<sup>41</sup>
- sjPlot (Lüdecke et al., 2022)<sup>42</sup>
- MASS (Ripley et al., 2022)<sup>43</sup>
- lme4 (Bates et al., 2022)<sup>44</sup>
- survival (Therneau et al., 2022)<sup>45</sup>.

27 Wickham, H. & Bryan, J. (2022) readxl: read Excel files. <https://readxl.tidyverse.org>, <https://github.com/tidyverse/readxl>.

28 Wickham et al. (2019) Welcome to the tidyverse. Journal of Open Source Software, 4 (43), 1686. <https://doi.org/10.21105/joss.01686>.

29 Wickham et al. (2019) Welcome to the tidyverse. Journal of Open Source Software, 4 (43), 1686. <https://doi.org/10.21105/joss.01686>.

30 Wickham, H. (2016) ggplot2: elegant graphics for data analysis. New York: Springer-Verlag. <https://ggplot2.tidyverse.org>.

31 Daróczy, G. (2022) pander: An R Pandoc writer. <https://rdocumentation.org/packages/pander/versions/0.6.5>

32 Revelle, W. (2022) psych: procedures for psychological, psychometric, and personality research. Northwestern University, Evanston, Illinois. R package version 2.2.5. <https://CRAN.R-project.org/package=psych>

33 Comtois, D. (2022) summarytools: tools to quickly and neatly summarize data. <https://cran.r-project.org/web/packages/summarytools/index.html>.

34 Harrell Jr, F. (2022) Hmisc: Harrell Miscellaneous. <https://cran.r-project.org/web/packages/Hmisc>

35 Wickham, H., François, R., Henry, L. & Müller, K. (2022) dplyr: a grammar of data manipulation. <https://dplyr.tidyverse.org>, <https://github.com/tidyverse/dplyr>.

36 Fox, J., Kleiber, C., Zeileis, A. & Kuschnig, N. (2021) Ivreg: instrumental-variables regression by “2SLS”, “2SM”, or “2SMM”, with diagnostics. <https://cran.r-project.org/web/packages/ivreg/ivreg.pdf>

37 Robinson, D. et al. (2022) Broom: convert statistical objects into tidy tibbles. <https://cran.r-project.org/web/packages/broom/broom.pdf>.

38 Ben-Shachar, M. et al. (2022) effectsize: indices of effect size and standardized parameters. <https://cran.r-project.org/web/packages/effectsize/effectsize.pdf>

39 Hothorn, T., Zeileis, A., Farebrother, R., Cummins, C., Millo, G. & Mitchell, D. (2021) lmtest: testing linear regression models. <https://cran.r-project.org/web/packages/lmtest/lmtest.pdf>

40 Zeileis, A., Lumley, T., Graham, N. & Koell, S. (2022) sandwich: robust covariance matrix estimators. <https://cran.r-project.org/web/packages/sandwich/sandwich.pdf>

41 Wickham, H. (2022) stringr: simple, consistent wrappers for common string operations. <https://cran.r-project.org/web/packages/stringr/stringr.pdf>

42 Lüdecke et al. (2022) sjPlot: data visualization for statistics in social science. <https://cran.r-project.org/web/packages/sjPlot/sjPlot.pdf>

43 Ripley, B. et al. (2022) MASS: support functions and datasets for Venables and Ripley’s MASS. <https://cran.r-project.org/web/packages/MASS/MASS.pdf>

44 Bates, D. et al. (2022) lme4: linear mixed-effects models using “Eigen” and S4. <https://cran.r-project.org/web/packages/lme4/lme4.pdf>

45 Therneau, T., Lumley, T., Atkinson, E. & Crowson, C. (2022) survival: survival analysis. <https://cran.r-project.org/web/packages/survival/survival.pdf>

## Appendix 6. Assumptions used when data cleaning

Field heading	Instructions and codes to use	Assumptions
Local authority name	Pre-populated by Coram. Please do not edit.	<ul style="list-style-type: none"> <li>• If missing, added according to which data set the case belongs to.</li> <li>• If rows completely blank except for LA name, these were deleted.</li> </ul>
Family unique ID (original as randomised)	Pre-populated by Coram based on the family ID entered into the randomisation platform by the local authority on the study website, at the point of entering pre-proceedings.	<ul style="list-style-type: none"> <li>• If missing, imputed as Family unique ID (corrected).</li> <li>• If still missing, back-coded from randomisation spreadsheet according to date of randomisation/phone number etc.</li> <li>• Created a new variable – Fam_ID_as_randomised for use as a unique identifier.</li> <li>• Where Fam_ID_as_randomised does not appear in randomisation spreadsheet, this is manually checked and back-coded.</li> <li>• Recent LAMB family ID codes were entered as child ID codes. These were manually back-coded.</li> <li>• Where Fam_ID_as_randomised cannot be traced back to the randomisation spreadsheet, these IDs are coded as “Cannot trace to randos” in the Exclude variable.</li> </ul>
Family unique ID (corrected)	Pre-populated by Coram. Occasionally family ID numbers were entered incorrectly by LAs into the randomisation website (e.g. a typo was made). This column provides the correct ID. We want to keep the original (incorrectly entered) IDs from the randomisation website too as a record.	<ul style="list-style-type: none"> <li>• If missing, imputed as Family unique ID (original as randomised).</li> </ul>
Child unique ID	The ID must be unique for that child. Only use alphabetic and numeric characters.	<ul style="list-style-type: none"> <li>• If missing, imputed as Family unique ID (original as randomised) plus A1 (and so on for siblings).</li> <li>• Where child ID codes are missing (43 children from 43 different families), these are computed by adding a unique code (CIDCompute) to the family ID.</li> <li>• If there is a duplicate child ID, keep only one row.</li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Randomisation outcome: “refer for FGC” (intervention) group or “do not refer for FGC” (control) group</b>	<p>Pre-populated by Coram based on randomisation platform on study website.</p> <p>The options are:</p> <p>1: Randomised into “refer for FGC” intervention group</p> <p>0: Randomised into “do not refer” control group.</p>	<ul style="list-style-type: none"> <li>• If missing, imputed from randomisation spreadsheet.</li> <li>• Text responses recoded as either 1 or 0.</li> <li>• If outcome inconsistent between siblings, replaced with randomisation outcome from spreadsheet.</li> </ul>
<b>Date of randomisation</b>	<p>Pre-populated by Coram based on randomisation platform on study website. The date should be recorded as DD/MM/YYYY.</p>	<ul style="list-style-type: none"> <li>• If missing, imputed from randomisation spreadsheet.</li> <li>• Check for discrepancies against randomisation spreadsheet.</li> </ul>
<b>Date on letter before proceedings</b>	<p>This column is validated, meaning a date can only be inputted in a DD/MM/YYYY format and the date needs to be before 31/05/2022.</p> <p>For example, 16 October 2020 needs to be recorded as 16/10/2020.</p> <p>If the letter has not been sent please enter “not sent” – the cell will allow this to be entered.</p> <p>If the date of the letter is not known please leave blank.</p>	<ul style="list-style-type: none"> <li>• If missing, imputed as the same date as siblings.</li> <li>• If still missing, imputed as the date of randomisation (new variable PP_letter_imputed).</li> <li>• If out of plausible range (over 60 days before or after date of randomisation), imputed as the date of randomisation.</li> <li>• If inconsistent with siblings, made to be the same.</li> <li>• If text is “unsent”, “not known” etc. this is deleted and added as text to column IL.</li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Mover flag (whether the child has moved address to outside the local authority since 1 September 2020)</b>	<p>Please select from:</p> <p>1: Mover: child is known to have moved to outside the local authority since 01/09/2020</p> <p>0: Non-mover: child has not moved to outside the local authority since 01/09/2020</p> <p>-1: Lost contact: local authority has lost contact with family since 01/09/2020 and does not know their whereabouts.</p>	<ul style="list-style-type: none"> <li>• Missing mover flags imputed to be same as siblings if sibling status is known.</li> <li>• If still missing, mover flag is imputed as 0 non-mover.</li> </ul>
<b>Date of birth of child</b>	<p>This column is validated, meaning a date can only be inputted in a DD/MM/YYYY format. The date also needs to be before 31/05/2022. For example, 16 October 2020 should be recorded as 16/10/2020.</p>	<ul style="list-style-type: none"> <li>• If text is “unborn”, “not known”, “baby died before birth” etc. this is deleted and added as text to column II.</li> <li>• Where text in II includes “unborn”, “pregnancy terminated” or “baby died before birth” this is recorded as 1 in a new variable called Unborn.</li> </ul>
<b>Gender</b>	<p>Please select from:</p> <p>0: Not known (gender not recorded or unknown for unborn children)</p> <p>1: Male</p> <p>2: Female</p> <p>9: Neither (indeterminate i.e. unable to be classed as either male or female).</p> <p>This column is validated, meaning only a 0, 1, 2 or 9 can be inputted.</p>	<ul style="list-style-type: none"> <li>• Missing gender is imputed as 0 Not known.</li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Ethnic group of child</b>	<p>Please select from:</p> <p><b>WBRI:</b> White British</p> <p><b>WIRI:</b> White Irish</p> <p><b>WOTH:</b> Any other White background</p> <p><b>WIRT:</b> Traveller of Irish Heritage</p> <p><b>WROM:</b> Gypsy/Roma</p> <p><b>MWBC:</b> White and Black Caribbean</p> <p><b>MWBA:</b> White and Black African</p> <p><b>MWAS:</b> White and Asian</p> <p><b>MOTH:</b> Any other Mixed background</p> <p><b>AIND:</b> Indian</p> <p><b>APKN:</b> Pakistani</p> <p><b>ABAN:</b> Bangladeshi</p> <p><b>AOTH:</b> Any other Asian background</p> <p><b>BCRB:</b> Caribbean</p> <p><b>BAFR:</b> African</p> <p><b>BOTH:</b> Any other Black background</p> <p><b>CHNE:</b> Chinese</p> <p><b>OOTh:</b> Any other ethnic group</p> <p><b>REFU:</b> Refused</p> <p><b>NOBT:</b> Information not yet obtained.</p> <p>This column has been validated, meaning only a code from the list can be selected.</p>	<ul style="list-style-type: none"> <li>• Missing ethnicity is imputed as NOBT.</li> <li>• WNRI (White Northern Irish) is recoded as WOTH as assume they want to distinguish themselves from WBRI.</li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Has the child's mother ever had a child taken into care?</b>	<p>Please select from:</p> <p>0: Not known/recorded</p> <p>1: Yes</p> <p>2: No.</p> <p>This column has been validated, meaning only a 0, 1 or 2 can be selected or inputted.</p>	<ul style="list-style-type: none"> <li>• Missing previous care history is imputed as 0 Not known.</li> </ul>
<b>Child's postcode</b>	<p>Please input a full postcode – e.g. WC1N 1AZ.</p>	<ul style="list-style-type: none"> <li>• Postcode districts created from first 4 digits of postcode.</li> <li>• Postcode districts matched with region and town data.</li> </ul>
<b>Did the family receive a family group conference [by 31/05/2022]?</b>	<p>Please select from:</p> <p>1 Yes</p> <p>2 No</p> <p>0 Not known.</p> <p>This column is validated, meaning only a 0, 1 or 2 can be inputted.</p>	<ul style="list-style-type: none"> <li>• If whether a child has received an FGC is missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• If G0 is missing but there is an FGC date, this is imputed as 1 (yes).</li> <li>• If whether a child has received an FGC is missing and the condition is control, this is imputed as 2 No.</li> <li>• If G0 is still missing this is imputed as 0 Unknown.</li> <li>• If G0 = 2 (there was not an FGC), but there is a date, this is imputed as 1.</li> <li>• If G0 = 0 (unknown), but there is a date, this is imputed as 1.</li> <li>• Cross-checking with Daybreak's data: <ul style="list-style-type: none"> <li>• If FGC = 0 (unknown) but Daybreak's data says that there was an FGC, FGC is imputed as 1.</li> <li>• If FGC = 2 (no FGC) but Daybreak's data says that there was an FGC, FGC is imputed as 1.</li> </ul> </li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Date of family group conference (FGC) meeting</b>	This column is validated, meaning a date can only be inputted in a DD/MM/YYYY format or “unknown” if date not known. For example, 16 October 2020 can only be inputted as 16/10/2020. Leave blank if no FGC took place.	<ul style="list-style-type: none"> <li>• If the date of an FGC is missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• Where there is an inconsistency between siblings, this is imputed as the date that the majority of siblings have.</li> <li>• Date of FGC is categorised as out of range if it is before the pre-proceedings letter, before randomisation or after the end of the reporting period. However, this is not used to exclude that FGCs have occurred, as the assumption is that the dates have typos etc.</li> <li>• Cross-checking with data given to Daybreak by local authorities: <ul style="list-style-type: none"> <li>• If G1a is missing but FGC = 1, G1a is imputed as the date FGC held from Daybreak data.</li> <li>• If G1a is more than 30 days’ difference from Daybreak’s data, G1a is imputed as FGC date in Daybreak data.</li> <li>• Recomputed date out of range based on this data.</li> </ul> </li> </ul>
<b>Why did an FGC meeting not happen (“refer for FGC” group) or why did an FGC meeting happen (“do not refer” group)?</b>	Free text field. Please provide a short explanation of around 100 words or less.	<ul style="list-style-type: none"> <li>• If the reason for mismatch is missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• If randomisation outcome is 1 and G0 is 1, this is made blank.</li> <li>• If randomisation outcome is 0 and G0 is 2, this is made blank.</li> <li>• If randomisation outcome is 1, G0 is 2 and this is missing, this is imputed as unknown.</li> <li>• If randomisation outcome is 0, G0 is 1 and this is missing, this is imputed as unknown.</li> <li>• Cross-checking with data given to Daybreak by local authorities: <ul style="list-style-type: none"> <li>• If FGC = 2 (no FGC) and F3 =1 (intervention condition), but reason for mismatch is missing, this is imputed as reason in Daybreak data for FGC not going ahead.</li> <li>• Also if Daybreak data has a reason, but ours is unknown, replace this with reason in Daybreak data.</li> </ul> </li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Was the FGC commissioned externally or in-house?</b>	<p>Please select from:</p> <p>-1: Not applicable (no FGC)</p> <p>0: Not known/recorded</p> <p>1: Commissioned externally</p> <p>2: Internal.</p> <p>This column is validated, so only -1, 0, 1 or 2 can be selected or inputted.</p>	<ul style="list-style-type: none"> <li>• If missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• If FGC is 2 No, G1c (commissioning) is imputed as -1 Not applicable.</li> <li>• If FGC is 1 Yes, this is imputed as 0 Not known.</li> <li>• If FGC happened and Commission is -1 make this not known.</li> <li>• If FGC is 0 not known, this is imputed as blank.</li> </ul>
<b>If applicable: number of FGC attendees from family network</b>	<p>This column is validated, so only a whole number can be inputted (for example: “5” not “five”).</p>	<ul style="list-style-type: none"> <li>• If missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• If FGC = 2 or 0 unknown, this is imputed as N/A.</li> <li>• If FGC = 1 this should be greater than 0, otherwise made blank.</li> <li>• Cross-checking with Daybreak’s data: <ul style="list-style-type: none"> <li>• If the total family attending is different from Daybreak’s total family attending for Ps who had FGC, use Daybreak’s number.</li> <li>• Create other numbers and dates based on Daybreak’s data if FGC = 1 (FGC).</li> <li>• If FGC = 2 make these numbers and dates blank (NA).</li> </ul> </li> </ul>
<b>Of whom, number attending by phone or video conference</b>	<p>This column is validated, so only a whole number can be inputted (for example: “2” not “two”).</p>	<ul style="list-style-type: none"> <li>• If missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• If FGC = 1 (FGC) and the number of attendees &gt;0 but no number for virtual attendants, this is imputed to be 0.</li> <li>• If FGC = 2 (no FGC) or FGC = 0 (unknown), this is imputed as NA.</li> <li>• Cross-checking with Daybreak’s data: <ul style="list-style-type: none"> <li>• If the virtual family attending is different from Daybreak’s virtual family attending for Ps who had an FGC, use Daybreak’s number.</li> </ul> </li> <li>• Create delivery format variable – if virtual &gt; 0 and in-person &gt; 0 then hybrid, if in-person = 0 and virtual &gt; 0 then virtual, if virtual = 0 and in-person &gt; 0 then in-person. If missing for either then unknown.</li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Status of FGC coordinator/facilitator</b>	<p>Please select from:</p> <p>Not applicable (no FGC)</p> <p>Employee of the local authority</p> <p>Self-employed</p> <p>Zero hours contract with local authority</p> <p>Worker for independent provider of FGCs</p> <p>Paid via an agency per case</p> <p>Other</p> <p>Not known/recorded.</p> <p>This column has been validated, so only one of the options listed above can be selected.</p>	<ul style="list-style-type: none"> <li>• If missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• If FGC is 2, G2a (coordinator status) is imputed as -1 Not applicable.</li> <li>• If missing and FGC is 1, G2a is imputed as Not known/recorded.</li> <li>• If Not applicable and FGC is 1, this is imputed as Not known/recorded.</li> <li>• If -1 and if FGC is 0 (unknown), G2a is imputed as blank.</li> </ul>
<b>Was the FGC coordinator/facilitator trained to standards set by Daybreak?</b>	<p>Please select from:</p> <p>-1: Not applicable (no FGC)</p> <p>0: Not known/recorded</p> <p>1: Yes</p> <p>2: No.</p> <p>This column has been validated, meaning only -1, 0, 1 or 2 can be inputted or selected.</p>	<ul style="list-style-type: none"> <li>• If missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• If FGC is 2 (no FGC), G2b (training) is imputed as -1 Not applicable.</li> <li>• If FGC = 1 (FGC) and G2b is missing, this is imputed as 0 (not known).</li> <li>• If FGC = 1 (FGC) and G2b is not applicable, this is imputed as 0 (not known).</li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Stage of court proceedings [as of 31/05/2022]</b>	<p>The column is now validated, meaning there are 7 drop-down options to choose from.</p> <p>As on 31/05/2022 please select the option that fits best from:</p> <p>0: Child accommodated by agreement – section 20</p> <p>1: Care proceedings have been issued – child is currently in proceedings</p> <p>2: Care proceedings were not issued and the PLO is stepped down</p> <p>3: Care proceedings have yet to be issued – still in pre-proceedings</p> <p>4: Emergency escalation, child is now looked-after</p> <p>5: Proceedings were entered and have since come to an end (court has ruled)</p> <p>6: Proceedings were entered but then withdrawn.</p>	<ul style="list-style-type: none"> <li>• If missing, this is imputed to be the same as their siblings.</li> <li>• 7 (Unknown) created for missing values.</li> </ul>
<b>Date court proceedings issued (if applicable)</b>	<p>This can only be recorded in a DD/MM/YYYY format or “NA” if not applicable. For example, 16 April 2020 can only be recorded as 16/04/2020. Leave blank if the date is not known or recorded.</p>	<ul style="list-style-type: none"> <li>• If missing, this is imputed to be the same as their siblings.</li> <li>• If the date court proceedings issued is out of plausible range (before randomisation, before pre-proceedings letter and/or before the FGC), make the date court proceedings issued and the stage of court proceedings blank (NA).</li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Date of letter informing families that local authority will not pursue court proceedings (if applicable)</b>	This date should be recorded in a DD/MM/YYYY format or “NA” if not applicable. For example, 16 October 2020 should be recorded as 16/10/2020.	<ul style="list-style-type: none"> <li>• If missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• If date court proceedings issued is not missing and date of step-down letter is not missing, date of step-down letter is deleted.</li> <li>• If the date of step-down is out of plausible range (before randomisation, before pre-proceedings letter and/or before the FGC), make the step-down date and the stage of court proceedings blank (NA).</li> <li>• If text is “unsent”, “not known” etc. this is deleted and added as text to column II.</li> <li>• This was not routinely collected and stored for a number of local authorities. Occasionally, LAs had to make an estimate based on when step-down conversations were taking place. Where LAs provided a month and year, the date was imputed to be the first of that month.</li> </ul>
<b>Date of court ruling or date of section 20 agreement (if applicable)</b>	This date should be recorded in a DD/MM/YYYY format or “NA” if not applicable. For example, 16 October 2020 should be recorded as 16/10/2020.	<ul style="list-style-type: none"> <li>• If missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• If text is “not known” etc. this is deleted and added as text to column II.</li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Nature of court ruling(s) (or section 20)</b>	<p>This a free text box, because there may be more than one court ruling in place. Please select a court ruling(s) from:</p> <p>1 Not applicable (no court ruling)</p> <p>2 Section 20</p> <p>3 Adoption Order</p> <p>4 Care Order</p> <p>5 Interim Care Order</p> <p>6 Child Arrangements Order</p> <p>7 Interim Child Arrangements Order</p> <p>8 Placement Order</p> <p>9 Special Guardianship Order</p> <p>10 Supervision Order</p> <p>11 Other order (not listed)</p> <p>12 Order not granted.</p>	<ul style="list-style-type: none"> <li>• If missing, unless there is a discrepancy between siblings, this is imputed to be the same as their siblings.</li> <li>• If multiple orders, have created a category 13 “Multiple orders”.</li> </ul>
<b>Start date of living arrangement</b>	<p>A date can only be recorded in a DD/MM/YYYY format or “Unknown” if date not known. For example, 16 October 2020 can only be inputted as 16/10/2020.</p>	<ul style="list-style-type: none"> <li>• If missing, this is imputed as the date of randomisation.</li> <li>• If text is “not known” etc. this is deleted and added as text to column II.</li> <li>• Reasonable assumptions were used to clean dates that were out of plausible range: <ul style="list-style-type: none"> <li>• Manually edited year if appeared to be an obvious error.</li> <li>• Manually edited year if it appeared to be in US format.</li> <li>• If it could not be imputed as a reasonable estimate, it was marked as poor quality and the case was excluded from data analysis.</li> </ul> </li> </ul>

Field heading	Instructions and codes to use	Assumptions
Living arrangement [at 31/05/2022]	<p>If living arrangement has changed for children since the last data return then please add this information in the relevant columns on righthand columns on (column AK onwards – see below for more guidance).</p> <ol style="list-style-type: none"> <li>1. Parent(s) including adoptive parent(s)</li> <li>2. Relative(s)</li> <li>3. Family friend(s)</li> <li>4. Independent or semi-independent living</li> <li>5. Foster carer(s) (unrelated and not a family friend)</li> <li>6. Children’s home</li> <li>7. Prospective adopter(s)</li> <li>8. Parent(s) and relative(s)</li> <li>9. Mother and Baby Unit</li> <li>10. Parent and child foster placement</li> <li>11. Emergency accommodation</li> <li>12. Not known.</li> </ol>	<ul style="list-style-type: none"> <li>• If missing, this is imputed as 12 Not known.</li> <li>• If living arrangement is given as 0, this is imputed as 1 (living with parents) – most of these children are still in pre-proceedings and on a CPP.</li> <li>• Have made the following categorisations across all living arrangements: <ul style="list-style-type: none"> <li>• “Placement with Parent” = 1. Parent(s) including adoptive parent(s).</li> <li>• “Residential assessment” = 6. Children’s home.</li> <li>• “Residential with parents” = 1. Parent(s) including adoptive parent(s).</li> </ul> </li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>End date of living arrangement</b>	A date can only be recorded in a DD/MM/YYYY format, or “unknown”. For example, 16 October 2020 can only be inputted as 16/10/2020.	<ul style="list-style-type: none"> <li>• If missing, this is imputed as 31/05/2022 unless there is a subsequent episode of a living arrangement.</li> <li>• If missing and there is a subsequent episode, this is imputed as the day before the start date of the subsequent episode of a living arrangement.</li> <li>• If text is “not known” etc. this is deleted and added as text to column II.</li> <li>• Reasonable assumptions were used to clean dates that were out of plausible range: <ul style="list-style-type: none"> <li>• Manually edited year if appeared to be an obvious error.</li> <li>• Manually edited year if it appeared to be in US format.</li> <li>• Manually edited to be the start date of the subsequent living arrangement.</li> <li>• If it could not be imputed as a reasonable estimate, it was marked as poor quality and the case was excluded from data analysis.</li> </ul> </li> </ul>
<b>Start date of legal status</b>	A date can only be inputted in a DD/MM/YYYY format or “Unknown” can be entered if date not known. For example, 16 October 2020 can only be inputted as 16/10/2020. The legal status options are listed in cell C35 below (for reference).	<ul style="list-style-type: none"> <li>• If text is “not known” etc. this is deleted and added as text to column II.</li> <li>• If missing, this is imputed as the date of randomisation.</li> <li>• If the start date of legal status ep 2 is missing, but there is a second episode of legal status and an end date of ep 1 legal status, make the start date of ep2 legal status a day later than the end date of ep 1 legal status.</li> <li>• Reasonable assumptions were used to clean dates that were out of plausible range: <ul style="list-style-type: none"> <li>• Manually edited year if appeared to be an obvious error.</li> <li>• Manually edited year if it appeared to be in US format.</li> <li>• If it could not be imputed as a reasonable estimate, it was marked as poor quality and the case was excluded from data analysis.</li> </ul> </li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Legal status [at 31/05/2022]</b>	<p>As of 31/05/2022 please select an option that best fits from:</p> <p>1 Child in Need</p> <p>2 Child Protection Plan</p> <p>3 Looked-after child</p> <p>4 None of these</p> <p>5 Not applicable.</p> <p>This column has been validated so only 1, 2, 3, 4 or 5 can be selected or inputted.</p>	<ul style="list-style-type: none"> <li>• Have made the following categorisations across all legal statuses: <ul style="list-style-type: none"> <li>• If “10” have imputed 1 (Child in Need) as have assumed typo.</li> <li>• If “6” or “9” have imputed as missing.</li> <li>• If “section 20” have imputed as 3 Looked-after child.</li> </ul> </li> </ul>
<b>End date of legal status</b>	<p>This date can only be recorded in a DD/MM/YYYY format, or “unknown”. For example, 16 October 2020 can only be inputted as 16/10/2020. Please leave blank if this legal status is still in place (no end date).</p>	<ul style="list-style-type: none"> <li>• If missing, this is imputed as 31/05/2022 unless there is a subsequent episode of a legal status.</li> <li>• If missing and there is a subsequent episode, this is imputed as the day before the start date of the subsequent episode of a legal status.</li> <li>• If text is “not known” etc. this is deleted and added as text to column I1.</li> <li>• Reasonable assumptions were used to clean dates that were out of plausible range: <ul style="list-style-type: none"> <li>• Manually edited year if appeared to be an obvious error.</li> <li>• Manually edited year if it appeared to be in US format.</li> <li>• Manually edited to be the day before the subsequent legal status start date.</li> <li>• If it could not be imputed as a reasonable estimate, it was marked as poor quality and the case was excluded from data analysis.</li> </ul> </li> </ul>
<b>Comments on data (optional)</b>	Free text field.	<ul style="list-style-type: none"> <li>• Check all data quality comments and action as required.</li> <li>• Created a “Delete” column to delete rows where I1 explained that the cases were parents or duplicates.</li> <li>• Created an “Exclude” column to categorise rows where I1 explained that participants were randomised in error or were ineligible. Also used this variable to indicate participants who had opted out.</li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Start date of living arrangement (Episode 2)</b>	These columns are provided to record changes in the child's living arrangements and legal status. Please record any changes. We have provided up to 6 sets of columns for this information (if there are more changes than this please make a comment in column AJ ("comments on data").	<ul style="list-style-type: none"> <li>• If missing, this is imputed as a day after the end date of the previous episode of a living arrangement.</li> <li>• Reasonable assumptions were used to clean dates that were out of plausible range:</li> <li>• Manually edited year if appeared to be an obvious error.</li> <li>• Manually edited year if it appeared to be in US format. <ul style="list-style-type: none"> <li>• Manually edited to be the end date of the previous living arrangement.</li> <li>• If it could not be imputed as a reasonable estimate, it was marked as poor quality and the case was excluded from data analysis.</li> </ul> </li> </ul>
<b>Living arrangement (Episode 2)</b>		
<b>End date of living arrangement (Episode 2)</b>		<ul style="list-style-type: none"> <li>• If missing, this is imputed as 31/05/2022 unless there is a subsequent episode of a living arrangement.</li> <li>• If missing and there is a subsequent episode, this is imputed as the day before the start date of the subsequent episode of a living arrangement.</li> <li>• Reasonable assumptions were used to clean dates that were out of plausible range: <ul style="list-style-type: none"> <li>• Manually edited year if appeared to be an obvious error.</li> <li>• Manually edited year if it appeared to be in US format.</li> <li>• Manually edited to be the start date of the subsequent living arrangement.</li> <li>• If it could not be imputed as a reasonable estimate, it was marked as poor quality and the case was excluded from data analysis.</li> </ul> </li> </ul>

Field heading	Instructions and codes to use	Assumptions
<b>Start date of legal status (Episode 2)</b>	These columns are provided to record changes in the child's living arrangements and legal status. Please record any changes. We have provided up to 6 sets of columns for this information (if there are more changes than this please make a comment in column AJ ("comments on data").	<ul style="list-style-type: none"> <li>• If missing, this is imputed as a day after the end date of the previous episode of a legal status.</li> <li>• Reasonable assumptions were used to clean dates that were out of plausible range:</li> <li>• Manually edited year if appeared to be an obvious error. <ul style="list-style-type: none"> <li>• Manually edited year if it appeared to be in US format.</li> <li>• Manually edited to be the start date of the previous legal status.</li> <li>• If it could not be imputed as a reasonable estimate, it was marked as poor quality and the case was excluded from data analysis.</li> </ul> </li> </ul>
<b>Legal status (Episode 2)</b>		
<b>End date of legal status (Episode 2)</b>		<ul style="list-style-type: none"> <li>• If missing, this is imputed as 31/05/2022 unless there is a subsequent episode of a legal status.</li> <li>• If missing and there is a subsequent episode, this is imputed as the day before the start date of the subsequent episode of a legal status.</li> <li>• Reasonable assumptions were used to clean dates that were out of plausible range: <ul style="list-style-type: none"> <li>• Manually edited year if appeared to be an obvious error.</li> <li>• Manually edited year if it appeared to be in US format.</li> <li>• Manually edited to be the day before the subsequent legal status start date.</li> <li>• If it could not be imputed as a reasonable estimate, it was marked as poor quality and the case was excluded from data analysis.</li> </ul> </li> </ul>

## Appendix 7. Baseline characteristics

Variable		Arm					
		Combined		Intervention		Control	
		N	%	N	%	N	%
Total N	Children	2548	100.0%	1289	50.6%	1259	49.4%
Total N	Families	1471	100.0%	740	50.3%	731	49.7%
Children per family	Number of children (mean sd)	1.7	1.2	1.7	1.1	1.7	1.2
Gender	Male	1281	50.3%	658	51.0%	623	49.5%
	Female	1136	44.6%	560	43.4%	576	45.8%
	Other	1	0.04%	1	0.1%	0	0.0%
	Unknown	130	5.1%	70	5.4%	60	4.8%
Age	Years (mean sd)	6.8	5.4	6.8	5.4	6.7	5.4
	Unborn	8	0.3%	6	0.5%	2	0.2%
Ethnicity	White or White British	1980	77.7%	1004	77.9%	976	77.5%
	Black or Black British	156	6.1%	75	5.8%	81	6.4%
	Asian or Asian British	58	2.3%	26	2.0%	32	2.5%
	Mixed background	233	9.1%	123	9.5%	110	8.7%
	Other background	12	0.5%	5	0.4%	7	0.6%
	Unknown	109	4.3%	56	4.3%	53	4.2%
	Local authority	Bath and North East Somerset	39	1.5%	15	1.2%	24
	Bromley	88	3.5%	31	2.4%	57	4.5%
	Birmingham	131	5.1%	69	5.4%	62	4.9%
	Derbyshire	126	5.0%	59	4.6%	67	5.3%
	Knowsley	83	3.3%	49	3.8%	34	2.7%
	Lambeth	87	3.4%	42	3.3%	45	3.6%
	Lancashire	181	7.1%	95	7.4%	86	6.8%
	Leicestershire	136	5.3%	77	6.0%	59	4.7%
	Lewisham	158	6.2%	91	7.1%	67	5.3%
	Middlesbrough	100	3.9%	55	4.3%	45	3.6%
	North East Lincolnshire	113	4.4%	47	3.7%	66	5.2%
	Northamptonshire	202	7.9%	92	7.1%	110	8.7%
	Nottingham	80	3.1%	43	3.3%	37	2.9%
	Plymouth	37	1.5%	16	1.2%	21	1.7%
	Redcar & Cleveland	120	4.7%	65	5.0%	55	4.4%
	Rotherham	207	8.1%	103	8.0%	104	8.3%

Variable		Arm					
		Combined		Intervention		Control	
		N	%	N	%	N	%
Local authority	Salford	97	3.8%	53	4.1%	44	3.5%
	Sheffield	122	4.8%	74	5.7%	48	3.8%
	Shropshire	160	6.3%	75	5.8%	85	6.8%
	Southampton	79	3.1%	30	2.3%	49	3.9%
	Sunderland	202	7.9%	108	8.4%	94	7.5%
Previous care history <sup>1</sup>	Yes	605	23.8%	278	21.6%	327	26.0%
	No	1355	53.2%	720	55.9%	635	50.4%
	Unknown	588	23.1%	291	22.6%	297	23.6%
Mover status <sup>2</sup>	Mover	226	8.9%	115	8.9%	111	8.8%
	Non-mover	2023	79.4%	1030	80.0%	993	78.9%
	Unknown	299	11.7%	144	11.2%	155	12.3%
Legal status at pre-proceedings letter date	Child in Need	222	8.7%	97	7.5%	125	9.9%
	Child Protection Plan	1377	54.0%	734	56.9%	643	51.1%
	Neither	167	6.6%	91	7.1%	76	6.0%
	Unknown	782	30.7%	367	28.5%	415	33.0%

1 Whether mother had previously had a child or children removed.

2 Whether the child moved outside of the local authority during the trial period.

When compared with looked-after children (Office for National Statistics, 2021), our sample was marginally more white (78% versus 75%) and had nearly half the proportion of those from Asian or Asian British backgrounds (2.3% versus 4%) and a sixth from “other backgrounds” (0.5% versus 3%).

## Primary outcome – care status: balance checks

Balance checks for sample of children for whom care status at 12 months post-pre-proceedings letter is observed:

Variable	Inter-vention	N	Control	N	2-sided test (p-value)
Total	52.4%	643	47.6%	584	
Age (mean)	6.9	637	6.8	581	0.72
Sex (female)	44.6%	277 (621)	46.9%	263 (561)	0.47
Ethnicity (White British)	82.7%	513 (620)	82.8%	470 (568)	1.00
Number of children per family <sup>1</sup> (mean)	1.8	352 families	1.7	342 families	0.18
Mover <sup>2</sup>	8.9%	52 (586)	7.4%	40 (539)	0.44
Care history <sup>3</sup>	23.7%	124 (523)	31.5%	149 (473)	0.007

Variable	Inter- vention	N	Control	N	2-sided test (p-value)
Legal status at pre-proceedings letter date (Child in Need)	9.7%	46 (476)	12.4%	51 (411)	0.41
Legal status at pre-proceedings letter date (Child Protection Plan)	75.4%	359 (476)	73.7%	303 (411)	
Legal status at pre-proceedings letter date (neither)	14.9%	71 (476)	13.9%	57 (411)	

1 At family level. All other balance checks were analysed at child level.

2 Whether the child moved outside of the local authority during the trial period.

3 Whether mother had previously had a child or children removed.

## Secondary outcome – perceived inclusiveness: balance checks

Balance checks for children whose parents were sent text messages and those whose parents responded:

Trial arm (intervention)	56.5%	186	50.9%	1394	0.18
Age (mean)	7.2	184	6.9	1356	0.52
Sex (female)	39.8%	72 (181)	48.3%	639 (1322)	0.04
Ethnicity (White British)	73.5%	133 (181)	77.9%	1038 (1333)	0.22
Number of children per family <sup>1</sup> (mean)	1.7	186	1.8	1394	0.33
Mover <sup>2</sup>	8.2%	14 (170)	12.3%	158 (1282)	0.15
Care history <sup>3</sup>	43.1%	59 (137)	30.5%	309 (1013)	0.004
Legal status at pre-proceedings letter date (Child in Need)	7.7%	10 (130)	13.0%	128 (983)	0.05
Legal status at pre-proceedings letter date (Child Protection Plan)	83.1%	108 (130)	79.8%	785 (983)	
Legal status at pre-proceedings letter date (neither)	9.2%	12 (130)	7.1%	70 (983)	

1 At family level. All other balance checks were analysed at child level.

2 Whether the child moved outside of the local authority during the trial period.

3 Whether mother had previously had a child or children removed.

## Secondary outcome – sustainment of outcome: balance checks

Balance checks for sample of children for whom sustainment of outcome at 12 months after the pre-proceedings letter is observed

Variable	Inter- vention arm	N	Control arm	N	2-sided test (p-value)
Trial arm	45.6%	183	54.4%	218	0.18
Age (mean)	6.4	182	6.5	217	0.90
Sex (female)	48.6%	88 (181)	47.9%	101 (211)	0.96
Ethnicity (White British)	92.8%	167 (180)	89.0%	186 (209)	0.27
Number of children per family <sup>1</sup> (mean)	1.7	108 families	1.6	137 families	0.48
Mover <sup>2</sup>	11.5%	20 (174)	7.1%	15 (210)	0.19
Care history <sup>3</sup>	22.4 %	38 (170)	38.5%	72 (187)	0.001
Legal status at pre-proceedings letter date (Child in Need)	14.7%	20 (136)	13.0%	18 (138)	0.16
Legal status at pre-proceedings letter date (Child Protection Plan)	81.6%	111 (136)	77.5%	107 (138)	
Legal status at pre-proceedings letter date (neither)	3.7%	5 (136)	9.4%	13 (138)	

1 At family level. All other balance checks were analysed at child level.

2 Whether the child moved outside of the local authority during the trial period.

3 Whether mother had previously had a child or children removed.

## Secondary outcome – time spent in care: balance checks

Balance checks for sample of children for whom time in care in the 12 months from the pre-proceedings letter is observed

Variable	Inter- vention arm	N	Control arm	N	2-sided test (p-value)
Trial arm	52.3%	749	47.7%	684	
Age (mean)	7.0	735	6.8	677	0.56
Sex (female)	45.6%	328 (720)	47.6%	311 (654)	0.49
Ethnicity (White British)	83.3%	599 (719)	83.4%	549 (658)	1.00
Number of children per family <sup>1</sup> (mean)	1.8	411 families	1.7	399 families	0.18
Mover <sup>2</sup>	8.8%	60 (683)	8.6%	54 (628)	0.98
Care history <sup>3</sup>	22.8%	141 (619)	30.7%	173 (563)	0.002

Variable	Inter- vention arm	N	Control arm	N	2-sided test (p-value)
Legal status at pre-proceedings letter date (Child in Need)	10.9%	61 (559)	14.4%	68 (472)	0.24
Legal status at pre-proceedings letter date (Child Protection Plan)	75.9%	424 (559)	72.9%	344 (472)	
Legal status at pre-proceedings letter date (neither)	13.2%	74 (559)	12.7%	60 (472)	

1 At family level. All other balance checks were analysed at child level.

2 Whether the child moved outside of the local authority during the trial period.

3 Whether mother had previously had a child or children removed.

## Secondary outcome – court diversion: balance checks

Balance checks for sample of children for whom court diversion during the reporting period is observed

Variable	Inter- vention arm	N	Control arm	N	2-sided test (p-value)
<b>Trial arm (intervention)</b>	56.5%	186	50.9%	1394	0.18
Age (mean)	7.2	184	6.9	1356	0.52
Sex (female)	39.8%	72 (181)	48.3%	639 (1322)	0.04
Ethnicity (White British)	73.5%	133 (181)	77.9%	1038 (1333)	0.22
Number of children per family <sup>1</sup> (mean)	1.7	186	1.8	1394	0.33
Mover <sup>2</sup>	8.2%	14 (170)	12.3%	158 (1282)	0.15
Care history <sup>3</sup>	43.1%	59 (137)	30.5%	309 (1013)	0.004
Legal status at pre-proceedings letter date (Child in Need)	7.7%	10 (130)	13.0%	128 (983)	0.05
Legal status at pre-proceedings letter date (Child Protection Plan)	83.1%	108 (130)	79.8%	785 (983)	
Legal status at pre-proceedings letter date (neither)	9.2%	12 (130)	7.1%	70 (983)	

1 At family level. All other balance checks were analysed at child level.

2 Whether the child moved outside of the local authority during the trial period.

3 Whether mother had previously had a child or children removed.

# Appendix 8. Details of local authorities from which case study families were recruited

## **Bath and North East Somerset**

- A unitary authority located in south-west England
- Small population: 193,000
- 90% White British
- Among the least economically deprived local authorities
- Rate of looked-after children is 53 per 10,000 of the population – below the 2019 England average (65 per 10,000)
- FGC practice: not routinely offered as of 2019
- Ofsted rating: good (2017).

## **Leicestershire:**

- East Midlands of England, largely rural (does not include the city of Leicester)
- Large population: 706,000
- Not ethnically diverse – 89% White British
- Leicestershire (excluding the city of Leicester) is among the least deprived local authorities nationally
- Rate of looked-after children is 42 per 10,000 – below the 2019 England average
- No FGC practice as of 2019
- Ofsted rating: requires improvement (2017).

## **Lewisham**

- Inner London borough
- Population: 306,000
- Ethnically diverse borough with 40% from ethnic minority backgrounds
- Among the most deprived local authorities in England
- Rate of looked-after children is 71 per 10,000 – above the 2019 England average
- No FGC practice in 2019
- Ofsted rating: requires improvement (2019).

## **Rotherham**

- South Yorkshire, predominantly urban
- Population: 265,000
- Predominantly White British (91.9%)
- 30% of population is in most deprived 5th in England, 8% in the least deprived 5th
- Rate of looked-after children is 112 per 10,000 – 1.7 times the England average
- Newly established FGC practice as of 2019
- Ofsted rating: good (2018).

## Appendix 9. Interview participants

Case no.	FGC?	Number of parents interviewed	Number of children or young people interviewed	Number of social workers interviewed	Same social worker interviewed both times?
1	FGC took place	1	0	3	No
2	FGC not offered	0	0	2	No
3	FGC took place	2 (joint interview with both parents)	0	2 (initial interview only)	N/A
4	FGC not offered	1	0	1	Yes
5	FGC took place	1	1	3 (group interview)	No
6	FGC not offered	1	0	1 (initial interview only)	N/A
7	FGC took place	1	1 (joint interview with parent)	2	No
8	FGC not offered	0	0	2	Yes
<b>Total</b>		<b>7</b>	<b>2</b>	<b>16</b>	

## Appendix 10. Power calculation for primary outcome at 18 months

		18-month sample for care status
MDES (proportion of a standard deviation)		0.428
Baseline/endline correlations	Child	N/A
Intracluster correlations (ICCs)	Family	0.97
Alpha		0.05
Power		0.8
1-sided or 2-sided?		2-sided
Level of intervention clustering		Family
Average cluster		1.82
Final sample size (children)	Intervention	152
	Control	160
	Total	312
Final sample size (families)	Intervention	82
	Control	89
	Family	171

## Appendix 11. Findings on perceived inclusiveness outcome

We received analysable text message responses for 58 families (7.8%) in the intervention trial arm and 53 families (7.3%) in the control trial arm. This translates to 106 children (8.2%) in the intervention arm and 81 children (6.4%) in the control arm. Our protocol advised against analysing this data if there was a 5% or greater difference in response rate between the trial arms. A chi-square test confirmed that there was no significant difference between trial arms in terms of whether an analysable text response was provided at the family level ( $p=0.64$ ) and at the child level ( $p=0.18$ ).

Children for whom we had analysable text responses were from 16 local authorities, compared with the total eligible sample which came from all 21 local authorities. Trial arms were balanced across all baseline characteristics except for care history ( $p<0.05$ ).

### Balance checks for participants with analysable text responses between trial arms

Variable	Inter- vention	N	Conbtrol arm	N	2-sided test (p-value)
<b>Total</b>	<b>56.9%</b>	<b>106</b>	<b>43.4%</b>	<b>81</b>	
Age (mean)	7.5	104	6.7	81	0.33
Sex (female)	33.7%	35 (104)	47.4%	37 (78)	0.08
Ethnicity (White British)	68.6%	70 (102)	80.0%	64 (80)	0.12
Number of children per family <sup>1</sup> (mean)	1.8	58 families	1.5	53 families	0.10
Mover <sup>2</sup>	9.3%	9 (97)	8.1%	6 (74)	1.00
Care history <sup>3</sup>	32.9%	25 (176)	55.7%	34 (61)	0.01
Legal status at pre-proceedings letter date (Child in Need)	9.0%	7 (78)	5.7%	3 (53)	0.05
Legal status at pre-proceedings letter date (Child Protection Plan)	82.1%	64 (78)	84.9%	45 (53)	
Legal status at pre-proceedings letter date (neither)	9.0%	7 (78)	9.4%	5 (53)	

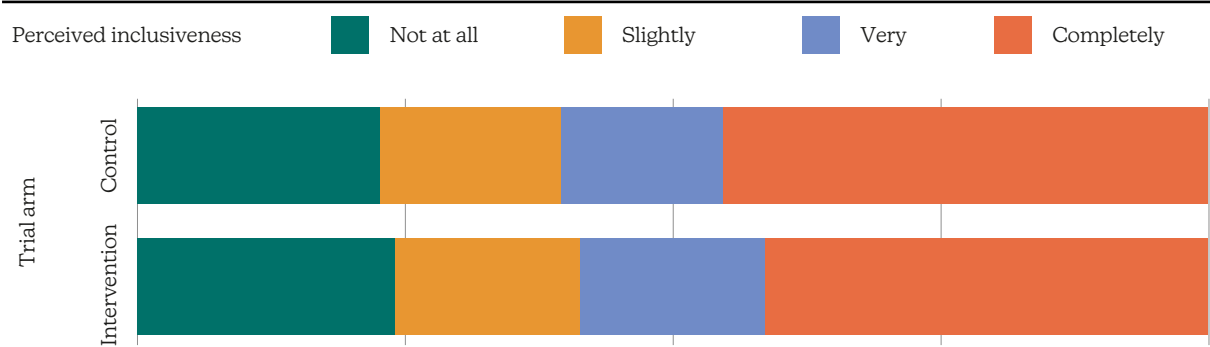
1 At family level. All other balance checks were analysed at child level.

2 Whether the child moved outside of the local authority during the trial period.

3 Whether mother had previously had a child or children removed.

The majority of parents in both trial arms (59% overall) felt either “completely” or “very” involved in the planning of their child(ren)’s care.<sup>46</sup> Approximately a quarter of parents from both the intervention (24%) and control groups (23%) felt “not at all” involved in planning their child(ren)’s care in the last 2 months.

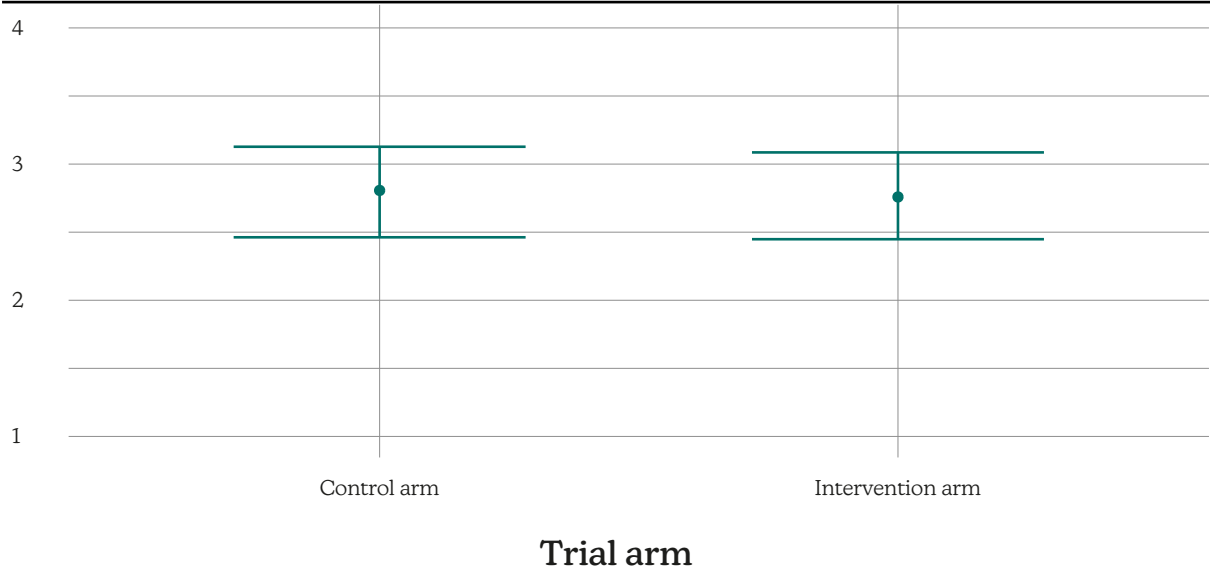
**Responses to text message by trial arm at family level**



n=111. The question read: “How involved have you been in planning your child(ren)’s care in the last two months?”

To further illustrate the small difference between trial arms in terms of perceived inclusiveness, see the forest plot below. The mean perceived inclusiveness for families in both trial arms, on a scale of 1 to 4 where 1 was “not at all” involved and 4 was “completely” involved, was 2.8 out of 4.

**Forest plot showing mean perceived inclusiveness score at family level by trial arm with 95% bootstrapped confidence interval (1000 reps)**



N=111. Note: 1 is “not at all”, 2 is “slightly”, 3 is “very” and 4 is “completely” involved in planning child(ren)’s care.

46 For the purposes of these proportions and the graph below, responses were rounded to the nearest whole number.

A simple linear regression was fitted to the data with fixed effects for local authority to predict perceived inclusiveness score by trial arm. We included care history as a predictor, as this was unbalanced across trial arms. According to this model, there was no difference in terms of perceived inclusiveness for children randomised to the intervention arm compared with the control arm ( $p=0.90$ ). Clustered standard errors were used to account for nesting within families.

## Basic regression analysis of perceived inclusiveness scores

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	Glass' delta	p-value
Intercept	1.43	0.50	2.36	0.47		0.00
Treatment	-0.04	-0.62	0.55	0.30	-0.06 (-0.36- 0.23)	0.90
<b>Fixed effects</b>						
<b>Bath and North East Somerset (base)</b>						
Birmingham	2.61	1.92	3.29	0.35		0.00
Bromley	2.20	1.35	3.04	0.43		0.00
Knowsley	2.06	0.96	3.15	0.56		0.00
Lambeth	1.37	-0.42	3.17	0.91		0.13
Lancashire	1.59	0.58	2.60	0.51		0.00
Lewisham	1.35	0.06	2.63	0.65		0.04
Middlesbrough	0.97	-0.27	2.20	0.63		0.12
North East Lincolnshire	-0.65	-1.60	0.31	0.48		0.18
North-hamptonshire	0.61	-0.85	2.06	0.74		0.41
Nottingham	0.67	-0.67	2.01	0.68		0.32
Plymouth	2.35	1.40	3.31	0.48		0.00
Redcar and Cleveland	2.00	0.92	3.08	0.55		0.00
Rotherham	0.78	-0.26	1.82	0.53		0.14
Sheffield	1.11	0.00	2.21	0.56		0.05
Shropshire	0.97	0.01	1.92	0.48		0.05
Sunderland	1.58	0.17	2.98	0.71		0.03
Care history (unknown)	0.25	-0.48	0.99	0.37		0.50
Care history (yes)	0.21	-0.51	0.94	0.37		0.57
Observations	187					

We also fitted a linear regression with fixed effects for local authority and additional predictor variables to estimate the influence of perceived inclusiveness score by trial arm. According to this model, there was no difference in terms of perceived inclusiveness for children randomised to the intervention arm compared with the control arm (p=0.85). Clustered standard errors were used to account for nesting within families.

### Linear regression with all predictor variables for perceived inclusiveness score

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	1.13	-0.66	2.92	0.91	0.22
Treatment	-0.06	-0.63	0.52	0.29	0.85
<b>Fixed effects</b>					
Bromley	2.66	1.46	3.86	0.61	0.00
Birmingham	2.50	1.20	3.80	0.66	0.00
Knowsley	2.41	0.84	3.98	0.79	0.00
Lambeth	1.91	-0.16	3.97	1.04	0.07
Lancashire	1.32	-0.04	2.69	0.69	0.06
Lewisham	1.14	-0.61	2.89	0.89	0.20
Middlesbrough	1.18	-0.24	2.60	0.72	0.10
North East Lincolnshire	-1.79	-3.15	-0.43	0.69	0.01
Northamptonshire	0.82	-0.86	2.51	0.85	0.34
Nottingham	0.88	-0.85	2.61	0.88	0.31
Plymouth	1.79	0.38	3.21	0.72	0.01
Redcar and Cleveland	2.38	0.82	3.95	0.79	0.00
Rotherham	0.92	-0.42	2.26	0.68	0.18
Sheffield	0.97	-0.44	2.39	0.72	0.18
Shropshire	1.31	-0.09	2.71	0.71	0.07
Sunderland	1.83	-0.07	3.73	0.96	0.06
Gender (male)	0.19	-0.19	0.57	0.19	0.33
Gender (unknown)	0.54	-0.81	1.89	0.68	0.43
Ethnicity (White)	0.12	-0.55	0.78	0.33	0.73
Age (4 to 7)	-0.12	-0.64	0.40	0.26	0.64
Age (8 to 11)	-0.11	-0.59	0.37	0.24	0.64
Age (12 to 19)	-0.36	-0.89	0.16	0.27	0.17
Age (unknown)	-2.32	-5.47	0.83	1.59	0.15
Number of children in family (2)	-0.51	-1.21	0.18	0.35	0.15
Number of children in family (3)	-0.44	-1.15	0.27	0.36	0.23
Number of children in family (4)	-0.17	-1.87	1.53	0.86	0.84
Number of children in family (5+)	1.10	-0.31	2.52	0.72	0.13

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Legal status at pre-proceedings letter date (Child Protection Plan)	0.55	-0.36	1.47	0.46	0.24
Legal status at pre-proceedings letter date (neither)	0.64	-0.32	1.61	0.49	0.19
Legal status at pre-proceedings letter date (unknown)	-0.03	-0.91	0.84	0.44	0.94
Implementation time (10+ months)	0.49	-0.34	1.31	0.42	0.25
Implementation time (5 to 9 months)	-0.16	-0.83	0.51	0.34	0.64
Care history (unknown)	0.38	-0.45	1.20	0.42	0.37
Care history (yes)	0.01	-0.66	0.68	0.34	0.97
Observations	187				

We also fitted a linear regression with fixed effects for local authority and significant or unbalanced predictor variables to estimate the influence of perceived inclusiveness score by trial arm. According to this model, there was no difference in terms of perceived inclusiveness for children randomised to the intervention arm compared with the control arm ( $p=0.66$ ). Clustered standard errors were used to account for nesting within families.

### Linear regression with significant predictor variables for perceived inclusiveness score

We also used instrumental variable analysis to calculate a complier average causal effect (CACE) estimate. The role of FGC delivery was not significant for perceived inclusiveness, with a regression coefficient of -0.06 and a p-value of 0.90.

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	1.20	-0.38	2.78	0.80	0.14
Treatment	-0.12	-0.69	0.44	0.29	0.66
<b>Fixed effects</b>					
Bath and North East Somerset (base)	2.66	1.46	3.86	0.61	0.00
Bromley	2.89	1.80	3.99	0.56	<0.01
Birmingham	2.05	0.91	3.20	0.58	<0.01
Knowsley	1.86	0.37	3.35	0.76	0.01
Lambeth	1.29	-0.73	3.31	1.02	0.21
Lancashire	1.30	0.02	2.57	0.65	0.05
Lewisham	1.26	-0.27	2.79	0.78	0.11
Middlesbrough	0.87	-0.55	2.30	0.72	0.23

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
North East Lincolnshire	-1.02	-2.25	0.21	0.62	0.10
Northamptonshire	0.44	-1.20	2.09	0.83	0.59
Nottingham	0.68	-0.87	2.22	0.78	0.39
Plymouth	2.54	1.27	3.82	0.65	<0.01
Redcar and Cleveland	1.77	0.43	3.10	0.68	0.01
Rotherham	0.51	-0.78	1.80	0.66	0.44
Sheffield	0.82	-0.57	2.21	0.70	0.25
Shropshire	0.90	-0.31	2.12	0.61	0.14
Sunderland	1.62	0.18	3.07	0.73	0.03
Legal status at pre-proceedings letter date (Child Protection Plan)	0.60	-0.38	1.58	0.50	0.23
Legal status at pre-proceedings letter date (neither)	0.72	-0.31	1.74	0.52	0.17
Legal status at pre-proceedings letter date (unknown)	0.04	-0.91	0.98	0.48	0.94
Care history (unknown)	0.35	-0.33	1.03	0.34	0.31
Care history (yes)	0.22	-0.50	0.94	0.36	0.54
Observations	187				

## Appendix 12. FGC characteristics

We merged Daybreak’s data with families randomised into the programme. This was important because Daybreak’s data also includes a number of families who received FGCs but did not take part in the programme (e.g. over 18 months after first randomisation in the local authority).

FGCs were delivered to 376 families containing 694 children in the eligible sample, representing 25.6% of eligible RCT families (n=1471) and 27.2% of eligible children (n=2548). Of these FGCs, 351 (93.4% of total FGCs) were delivered to families in the intervention arm (647 children) and 25 (6.6%) were delivered to families in the control arm (47 children). According to Daybreak’s data, FGCs were delivered to 329 families in the intervention arm. For a further breakdown of these figures, please see the section “FGC delivery – Compliance analysis”.

By merging Daybreak’s data with the data returns from local authorities, we know the total number of family member attendees for 360 of the 376 FGCs. This figure does not include professionals in attendance. The number of family member attendees ranged between 1 and 15, with a mean average of 6.0 (SD=2.6). The total number of family network attendees was 2167. We were able to provide a more detailed breakdown of FGC attendees for 308 families. The number of attendees (including professionals) ranged from 1 to 19, with a mean average of 8.3 (SD=3.0).

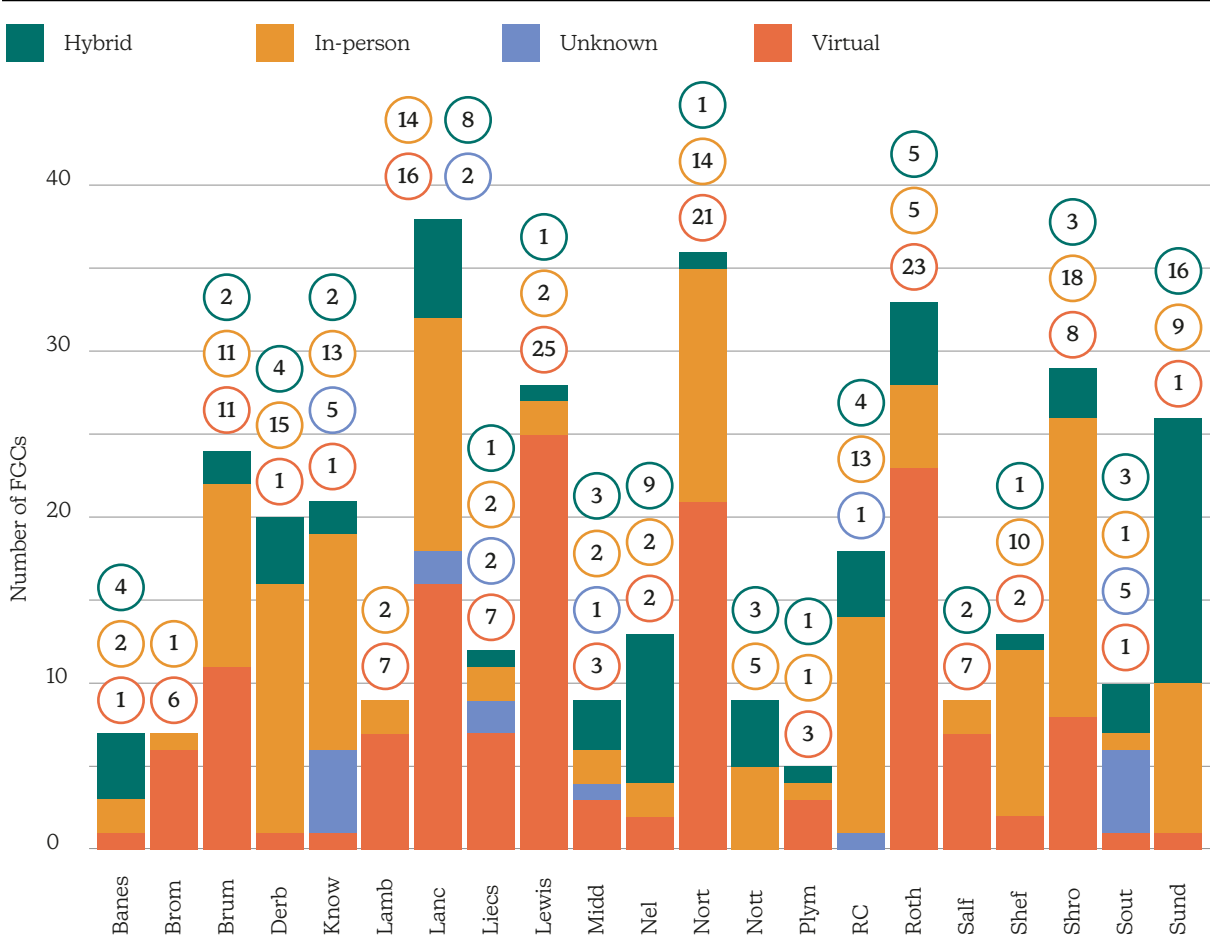
### Family network attendance at FGCs

At least 1 ... attending FGC	Number of FGCs	Percentage of FGCs (%)
Child	105	34.1%
Mother	292	94.8%
Father	184	59.7%
Member of maternal family network	257	83.4%
Member of paternal family network	161	52.3%
Member of other support network	124	40.3%
<b>Total FGCs where details of attendees available</b>	<b>308</b>	<b>100%</b>

According to our analysis of Daybreak’s data, at the majority of FGCs (n=265, 70.5%) children were not supported by an advocate or support person. At 33 FGCs (8.8%) an advocate or support person for the child was present, but for 78 FGCs (20.7%) this information is unknown. Similarly, at the majority of FGCs (n=265, 70.5%) there was not an advocate or support person present for a vulnerable family member(s). This advocate was present at 29 FGCs (7.7%), but this information was unknown for 82 FGCs (21.8%). We do not know how often an advocate or support person may have been needed but not provided.

A similar percentage of FGCs were delivered virtually (n=146, 38.8%) and in-person (n=144, 38.3%), with 70 delivered in a hybrid format (18.6%) and 16 with an unknown delivery format (4.3%). Where known, therefore, 41% were virtual, 40% in-person and 19% hybrid. The delivery format varied across local authorities. Among all family network attendees, 52.8% attended in-person and 47.2% by phone or video call.

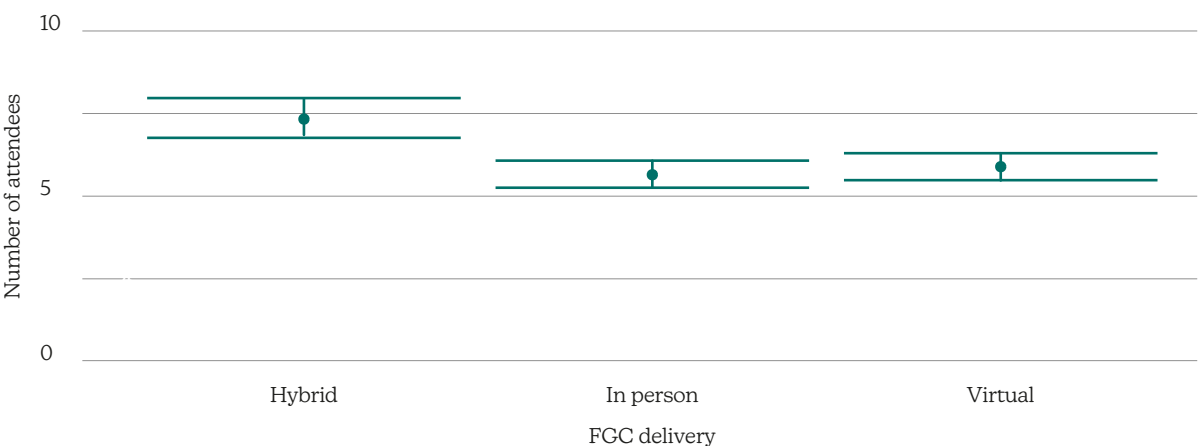
Number of FGCs by delivery format per local authority



Note: local authority abbreviations are detailed later in Appendix 12.

We observed a higher number of family network member attendees for FGCs that were delivered in a hybrid format compared with other formats. The lower bound for hybrid FGC attendees was higher than the mean (the dot) for the other two formats, meaning we can be confident, at this level of statistical significance, that there is a difference in attendance. In contrast, the mean and the confidence intervals (upper and lower bounds) for in-person and virtual FGCs overlap, meaning we cannot say with confidence that there was a difference in attendance between these two types. The mean average number of attendees (non-professionals) for hybrid FGCs was 7.3 (SD=2.6), compared with in-person FGCs which had an average of 5.6 attendees (SD=2.5) and virtual FGCs which had an average of 5.9 attendees (SD=2.4). It should be noted that other shared factors may influence both the delivery format and the number of attendees (e.g. the organising social worker).

Forest plot of mean number of attendees (excluding professionals) by FGC delivery format with 95% bootstrapped confidence interval (1000 reps)

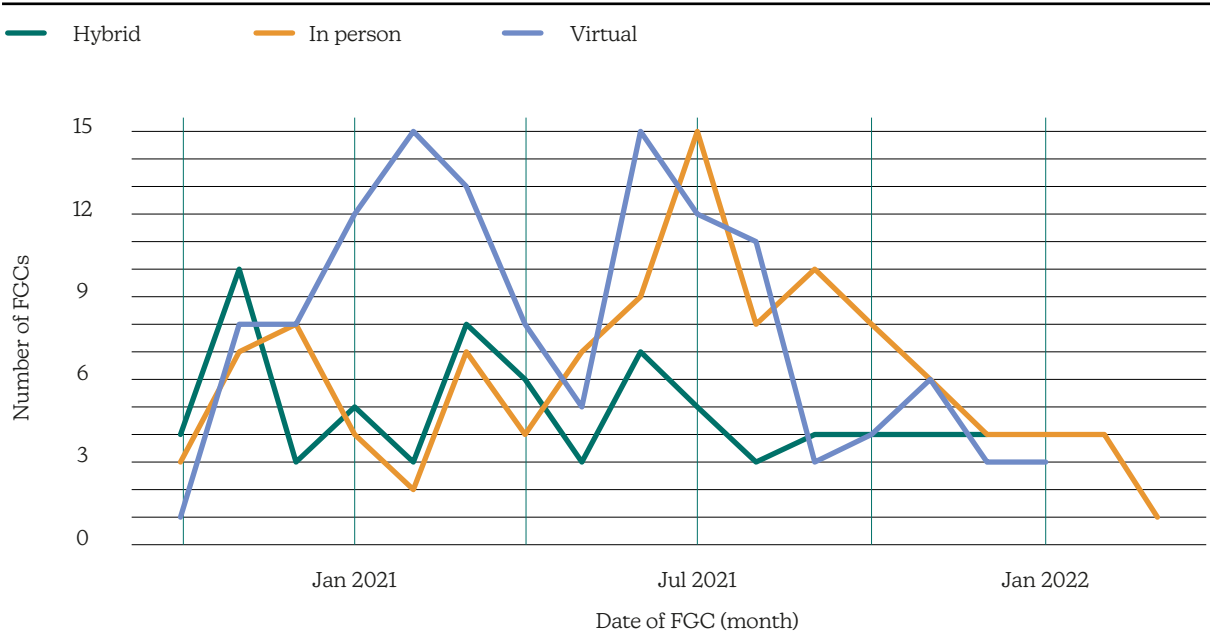


We also observed a higher proportion of hybrid FGCs, compared with virtual and in-person FGCs, had attendance from a child, mother, father, member of the paternal family network and member of the extended family network. Shared factors may influence both the delivery format and the types of attendee present.

### Types of attendee by FGC delivery format

At least 1 ... attending FGC	FGC delivery							
	Hybrid		In-person		Virtual		Overall	
	N	%	N	%	N	%	N	%
<b>Child</b>	36	51.4%	34	32.7%	35	26.1%	105	34.1%
<b>Mother</b>	70	100%	98	94.2%	124	92.5%	292	94.8%
<b>Father</b>	48	69.6%	53	51.0%	83	61.9%	184	59.7%
<b>Maternal family network</b>	57	81.4%	86	82.7%	114	85.1%	257	83.4%
<b>Paternal family network</b>	42	60.9%	50	48.1%	69	51.5%	161	52.3%
<b>Other network</b>	37	52.9%	38	36.5%	49	36.6%	124	40.3%
<b>Total FGCs where details of attendees available and format known</b>	70	22.7%	104	33.8%	134	43.5%	308	100%

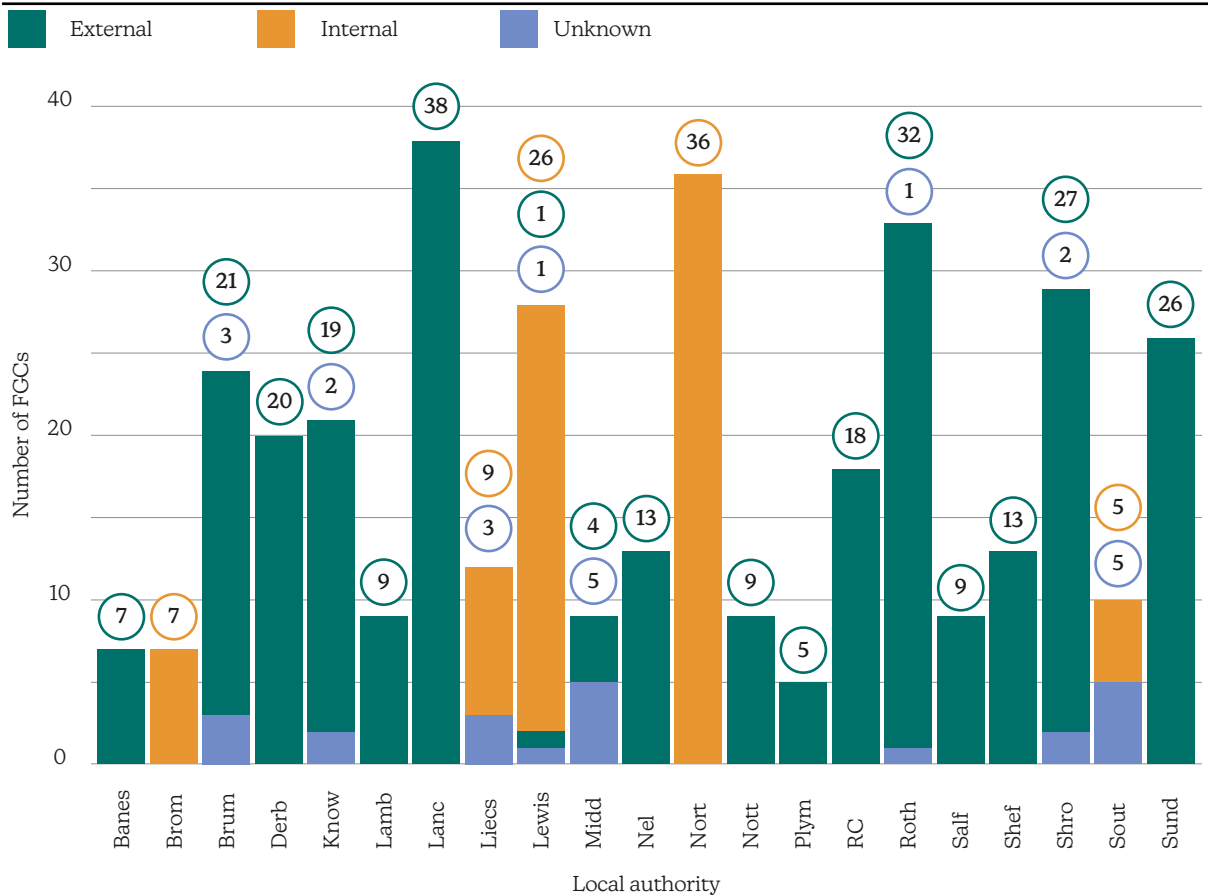
Line graph showing dates of FGC delivery by delivery format (n=305)



The majority of FGCs were commissioned internally (n=271, 72.1%), with 83 (22.1%) commissioned externally and the remainder unknown (n=22, 5.9%). So, where known, 77% were internal; 23% external. The approach to commissioning was largely consistent within local authorities. In accordance with this approach to commissioning, the majority of FGCs were delivered by facilitators who were employees of the local authority (n=269, 71.5%). N=85 FGCs were delivered by workers for independent providers of FGCs (22.6%), 3 FGCs (0.8%) were delivered by employees on zero hours contracts with the local authority and for 19 FGCs (5.1%) the status of the facilitator was unknown. The majority of FGCs (n=340, 90.4%) were delivered by facilitators who were trained to the standards set by Daybreak. A small number of FGCs (n=4, 1.1%) were delivered by facilitators who were not trained to these standards, and for 32 (8.5%) FGCs the training status of the facilitators was unknown.

For both hybrid and in-person FGCs, over 80% were commissioned internally. However, for virtual FGCs, just over half (56.9) were commissioned internally, with 38.4% being commissioned externally.

Commissioning of FGCs by local authority



Note: for local authority abbreviations, see table below.

For both hybrid and in-person FGCs, over 80% were commissioned internally. However, for virtual FGCs, just over half (56.9) were commissioned internally, with 38.4% being commissioned externally.

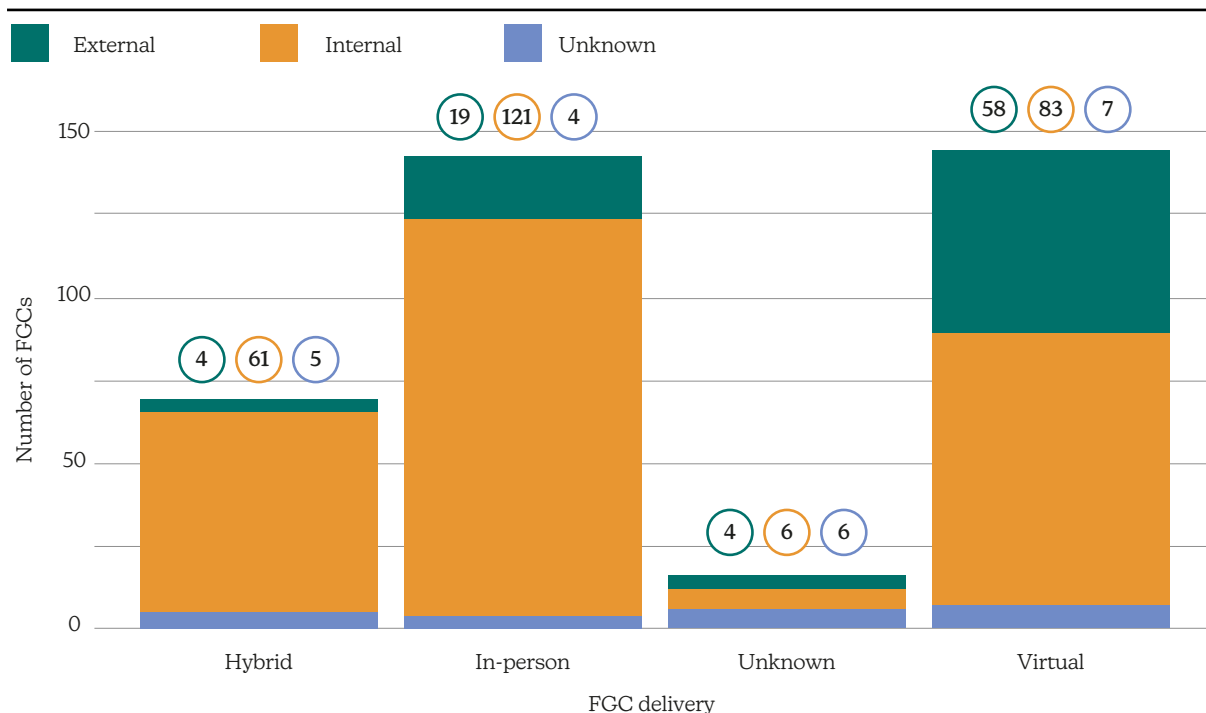
Local authority	Abbreviation
Bath and Nest East Somerset	BANES
Bromley	BROM
Birmingham	BRUM
Derbyshire	DERB
Knowsley	KNOW
Lambeth	LAMB
Lancashire	LANC
Leicestershire	LEICS
Lewisham	LEWIS
Middlesbrough	MIDD
North East Lincolnshire	NEL
Northamptonshire	NORT
Nottingham City	NOTT
Plymouth	PLYM
Redcar and Cleveland	RC

Rotherham	ROTH
Salford	SALF
Sheffield	SHEF
Shropshire	SHRO
Southampton	SOUT
Sunderland	SUND

## FGC commissioning arrangement against delivery format

FGC delivery	Commissioning			
	Internal	External	Unknown	Total
	N (%)	N (%)	N (%)	N
Hybrid	61 (87.1%)	4 (5.7%)	5 (7.1%)	70
In-person	121 (84.0%)	19 (13.2%)	4 (2.8%)	144
Virtual	83 (56.9%)	56 (38.4%)	7 (4.8%)	146

Bar chart showing the number of FGCs delivered by format of delivery and type of commissioning



According to Daybreak's data, review meetings took place for just under half of the FGCs (n=155, 41.2%). However, this may be an underestimate because not all of Daybreak's data could be matched with cases in our sample.

## Appendix 13. Additional impact analysis tables with inclusion of all and significant predictor variables

Primary outcome – logistic regression of care status with fixed effects for all predictor variables

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	-4.57	-6.37	-2.77	0.92	<0.01
Trial arm (intervention)	-0.71	-1.25	-0.18	0.28	0.01
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	0.19	-1.27	1.64	0.74	0.80
Birmingham	2.01	0.26	3.75	0.89	0.02
Derbyshire	2.08	0.58	3.58	0.77	0.01
Knowsley	-0.26	-1.63	1.10	0.70	0.71
Lambeth	13.18	11.12	15.25	1.05	<0.01
Lancashire	3.49	1.90	5.07	0.81	<0.01
Leicestershire	1.24	-0.55	3.02	0.91	0.17
Lewisham	1.02	-1.38	3.43	1.23	0.41
Middlesbrough	0.69	-0.60	1.98	0.66	0.30
North East Lincolnshire	4.13	2.65	5.61	0.75	<0.01
Northamptonshire	1.34	-0.30	2.98	0.84	0.11
Nottingham	-0.28	-1.89	1.34	0.83	0.74
Plymouth	0.49	-1.52	2.50	1.02	0.63
Redcar and Cleveland	0.55	-2.12	3.22	1.36	0.69
Rotherham	-0.27	-1.59	1.05	0.68	0.69
Salford	1.99	-0.02	3.99	1.02	0.05
Sheffield	2.83	1.26	4.39	0.80	<0.01
Shropshire	0.34	-1.18	1.86	0.77	0.66
Southampton	1.30	-0.43	3.03	0.88	0.14
Sunderland	2.09	0.48	3.69	0.82	0.01
Gender (male)	-0.13	-0.52	0.25	0.20	0.50
Gender (unknown)	0.64	-0.98	2.25	0.82	0.44
Ethnicity (White)	0.78	0.00	1.56	0.40	0.05
Age (4 to 7)	-0.12	-0.65	0.42	0.27	0.67
Age (8 to 11)	-0.32	-0.99	0.36	0.35	0.36
Age (12 to 19)	-0.28	-0.89	0.32	0.31	0.36
Age (unknown)	0.86	-1.33	3.06	1.12	0.44

Number of children in family (2)	0.03	-0.67	0.73	0.36	0.94
Number of children in family (3)	0.03	-0.75	0.81	0.40	0.94
Number of children in family (4)	-0.16	-1.23	0.92	0.55	0.77
Number of children in family (5+)	0.38	-0.64	1.39	0.52	0.47
Legal status at pre-proceedings letter date (Child Protection Plan)	1.05	0.21	1.89	0.43	0.01
Legal status at pre-proceedings letter date (neither)	-25.43	-27.76	-23.09	1.19	<0.01
Legal status at pre-proceedings letter date (unknown)	6.28	5.09	7.46	0.61	<0.01
Implementation time (5 to 9 months)	-0.23	-0.79	0.34	0.29	0.43
Care history (unknown)	-0.11	-1.07	0.85	0.49	0.82
Care history (yes)	1.30	0.57	2.03	0.37	<0.01
Observations	1219				

### Primary outcome – logistic regression of care status with backwise deletion of predictor variables

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	-4.24	-5.69	-2.79	0.74	<0.01
Trial arm (intervention)	-0.65	-1.19	-0.12	0.27	0.02
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	0.15	-1.17	1.46	0.67	0.82
Birmingham	1.93	0.35	3.52	0.81	0.02
Derbyshire	2.30	0.84	3.75	0.74	<0.01
Knowsley	-0.06	-1.31	1.19	0.64	0.93
Lambeth	13.01	11.21	14.80	0.92	<0.01
Lancashire	3.54	1.99	5.09	0.79	<0.01
Leicestershire	1.43	-0.25	3.11	0.86	0.09
Lewisham	0.57	-2.01	3.16	1.32	0.66
Middlesbrough	0.81	-0.39	2.01	0.61	0.19
North East Lincolnshire	4.26	2.80	5.71	0.74	<0.01
Northamptonshire	1.50	-0.03	3.03	0.78	0.06
Nottingham	-0.13	-1.62	1.37	0.76	0.87

Plymouth	0.80	-1.03	2.63	0.93	0.39
Redcar and Cleveland	0.68	-1.90	3.25	1.32	0.61
Rotherham	-0.08	-1.33	1.17	0.64	0.90
Salford	2.18	0.12	4.23	1.05	0.04
Sheffield	2.83	1.33	4.34	0.77	<0.01
Shropshire	0.49	-0.97	1.94	0.74	0.51
Southampton	1.67	0.05	3.28	0.83	0.04
Sunderland	2.28	0.75	3.81	0.78	<0.01
Legal status at pre-proceedings letter date (Child Protection Plan)	1.05	0.23	1.87	0.42	0.01
Legal status at pre-proceedings letter date (neither)	-25.40	-27.68	-23.13	1.16	<0.01
Legal status at pre-proceedings letter date (unknown)	6.17	4.97	7.38	0.61	<0.01
Care history (unknown)	-0.24	-1.20	0.72	0.49	0.63
Care history (yes)	1.30	0.61	1.99	0.35	<0.01
Observations	1,227				

## Secondary outcome – regression model with all predictors for sustainment of outcome

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	0.35	-2.62	3.32	1.51	0.82
Trial arm (intervention)	-0.25	-1.09	0.59	0.43	0.56
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	-2.36	-5.78	1.07	1.75	0.18
Birmingham	-20.86	-23.05	-18.67	1.12	<0.01
Derbyshire	-1.56	-3.56	0.44	1.02	0.13
Knowsley	-21.50	-24.43	-18.57	1.49	<0.01
Lancashire	-1.07	-3.11	0.97	1.04	0.30
Leicestershire	-0.18	-3.20	2.85	1.54	0.91
Lewisham	-3.96	-6.95	-0.96	1.53	0.01
Middlesbrough	-20.94	-23.16	-18.71	1.13	<0.01
North East Lincolnshire	1.19	-1.04	3.41	1.14	0.30
Northamptonshire	-18.63	-21.57	-15.68	1.50	<0.01
Nottingham	-19.86	-22.38	-17.35	1.28	<0.01
Plymouth	-21.98	-25.35	-18.61	1.72	<0.01

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Redcar and Cleveland	-0.75	-2.95	1.45	1.12	0.50
Rotherham	-4.50	-7.00	-1.99	1.28	<0.01
Salford	-1.76	-4.18	0.65	1.23	0.15
Sheffield	-1.86	-4.16	0.43	1.17	0.11
Shropshire	-4.12	-6.57	-1.68	1.25	<0.01
Southampton	-19.37	-22.85	-15.88	1.78	<0.01
Sunderland	-2.23	-4.41	-0.06	1.11	0.04
Gender (male)	-0.50	-1.12	0.12	0.32	0.11
Gender (unknown)	-21.06	-23.97	-18.16	1.48	<0.01
Ethnicity (White)	-1.09	-2.56	0.38	0.75	0.15
Age (4 to 7)	-1.63	-2.88	-0.38	0.64	0.01
Age (8 to 11)	-0.69	-1.93	0.55	0.63	0.27
Age (12 to 19)	-0.18	-1.33	0.97	0.59	0.76
Age (unknown)	19.81	17.60	22.02	1.13	<0.01
Number of children in family (2)	-0.48	-2.14	1.18	0.85	0.57
Number of children in family (3)	1.20	-0.25	2.65	0.74	0.10
Number of children in family (4)	-1.41	-3.87	1.05	1.26	0.26
Number of children in family (5+)	1.05	-0.46	2.56	0.77	0.17
Legal status at pre-proceedings letter date (Child Protection Plan)	0.65	-0.96	2.27	0.82	0.43
Legal status at pre-proceedings letter date (neither)	1.42	-1.70	4.55	1.59	0.37
Legal status at pre-proceedings letter date (unknown)	2.31	0.34	4.27	1.00	0.02
Implementation time (5 to 9 months)	1.23	-0.06	2.52	0.66	0.06
Care history (unknown)	-0.59	-2.00	0.82	0.72	0.41
Care history (yes)	0.97	-0.04	1.99	0.52	0.06
Observations	393				

## Secondary outcome – regression model with significant predictors for sustainment of outcome

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	-1.04	-3.36	1.27	1.18	0.38
Trial arm (intervention)	-0.11	-0.92	0.71	0.42	0.80
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	-1.56	-4.95	1.82	1.73	0.37
Birmingham	-19.53	-21.63	-17.44	1.07	<0.01
Derbyshire	-0.81	-2.73	1.11	0.98	0.41
Knowsley	-19.79	-22.12	-17.46	1.19	<0.01
Lancashire	-0.17	-2.28	1.93	1.07	0.87
Leicestershire	-0.11	-3.00	2.78	1.47	0.94
Lewisham	-2.59	-5.48	0.30	1.47	0.08
Middlesbrough	-19.96	-22.01	-17.90	1.05	<0.01
North East Lincolnshire	1.21	-0.94	3.37	1.10	0.27
Northamptonshire	-18.78	-21.58	-15.97	1.43	<0.01
Nottingham	-18.93	-20.87	-16.99	0.99	<0.01
Plymouth	-20.04	-22.54	-17.55	1.27	<0.01
Redcar and Cleveland	-0.59	-3.04	1.85	1.25	0.64
Rotherham	-3.17	-5.95	-0.38	1.42	0.03
Salford	-0.76	-3.14	1.62	1.22	0.53
Sheffield	-0.65	-2.76	1.45	1.07	0.54
Shropshire	-2.50	-4.62	-0.39	1.08	0.02
Southampton	-19.40	-23.20	-15.60	1.94	<0.01
Sunderland	-1.57	-3.69	0.56	1.08	0.15
Gender (male)	-0.44	-1.02	0.15	0.30	0.15
Gender (unknown)	-19.36	-21.99	-16.73	1.34	<0.01
Age (4 to 7)	-1.43	-2.60	-0.26	0.60	0.02
Age (8 to 11)	-0.37	-1.38	0.65	0.52	0.48
Age (12 to 19)	0.24	-0.52	1.00	0.39	0.53
Age (unknown)	19.14	16.92	21.36	1.13	<0.01
Legal status at pre-proceedings letter date (Child Protection Plan)	0.46	-0.84	1.76	0.66	0.49
Legal status at pre-proceedings letter date (neither)	1.31	-1.98	4.60	1.68	0.43
Legal status at pre-proceedings letter date (unknown)	1.43	-0.19	3.05	0.83	0.08
Care history (unknown)	0.16	-1.42	1.75	0.81	0.84
Care history (yes)	1.14	0.24	2.04	0.46	0.01
Observations	401				

## Secondary outcome – regression model with all predictors for time spent in care

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	6.22	-37.54	49.98	22.31	0.78
Trial arm (intervention)	-8.87	-22.32	4.57	6.85	0.20
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	22.45	-22.91	67.80	23.12	0.33
Birmingham	-5.46	-48.71	37.80	22.05	0.80
Derbyshire	71.60	20.37	122.82	26.11	0.01
Knowsley	0.77	-42.68	44.22	22.15	0.97
Lambeth	-12.08	-109.45	85.30	49.64	0.81
Lancashire	90.49	38.09	142.89	26.71	0.00
Leicestershire	-12.07	-65.04	40.90	27.00	0.65
Lewisham	7.06	-45.33	59.46	26.71	0.79
Middlesbrough	46.43	4.84	88.02	21.20	0.03
North East Lincolnshire	126.86	61.84	191.88	33.15	<0.01
Northamptonshire	88.21	36.68	139.73	26.27	0.00
Nottingham	10.53	-30.77	51.83	21.05	0.62
Plymouth	15.36	-44.01	74.73	30.27	0.61
Redcar and Cleveland	31.90	-14.58	78.38	23.69	0.18
Rotherham	14.25	-25.02	53.52	20.02	0.48
Salford	49.04	0.43	97.65	24.78	0.05
Sheffield	90.14	35.50	144.79	27.86	<0.01
Shropshire	25.67	-15.03	66.37	20.75	0.22
Southampton	67.81	17.55	118.08	25.62	0.01
Sunderland	34.12	-10.68	78.93	22.84	0.14
Gender (male)	-1.60	-11.78	8.58	5.19	0.76
Gender (unknown)	4.49	-31.54	40.52	18.37	0.81
Ethnicity (White)	0.25	-18.31	18.82	9.46	0.98
Age (4 to 7)	-6.42	-21.26	8.41	7.56	0.40
Age (8 to 11)	-16.62	-33.11	-0.12	8.41	0.05
Age (12 to 19)	-18.46	-34.29	-2.62	8.07	0.02
Age (unknown)	-116.83	-176.28	-57.39	30.30	<0.01
Number of children in family (2)	-5.68	-21.80	10.45	8.22	0.49
Number of children in family (3)	-3.80	-24.51	16.91	10.56	0.72
Number of children in family (4)	-21.48	-56.67	13.70	17.94	0.23

Number of children in family (5+)	-14.08	-39.34	11.19	12.88	0.27
Legal status at pre-proceedings letter date (Child Protection Plan)	11.87	-5.92	29.66	9.07	0.19
Legal status at pre-proceedings letter date (neither)	-3.59	-27.82	20.63	12.35	0.77
Legal status at pre-proceedings letter date (unknown)	229.09	204.63	253.56	12.47	<0.01
Implementation time (5 to 9 months)	5.53	-9.88	20.94	7.85	0.48
Care history (unknown)	-25.87	-52.15	0.41	13.40	0.05
Care history (yes)	31.17	10.49	51.84	10.54	<0.01
Observations	1424				

### Secondary outcome – regression model with significant predictors for time spent in care

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	1.82	-38.06	41.69	20.33	0.93
Trial arm (intervention)	-8.08	-21.68	5.52	6.93	0.24
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	21.69	-21.99	65.37	22.27	0.33
Birmingham	-0.20	-42.14	41.74	21.38	0.99
Derbyshire	72.70	23.64	121.76	25.01	0.00
Knowsley	1.85	-41.50	45.20	22.10	0.93
Lambeth	-12.63	-106.29	81.03	47.74	0.79
Lancashire	91.91	39.24	144.58	26.85	<0.01
Leicestershire	-9.39	-59.43	40.65	25.51	0.71
Lewisham	6.86	-45.81	59.54	26.85	0.80
Middlesbrough	46.13	4.71	87.54	21.11	0.03
North East Lincolnshire	126.83	63.12	190.53	32.47	<0.01
Northamptonshire	85.86	35.54	136.17	25.65	0.00
Nottingham	14.97	-23.97	53.91	19.85	0.45
Plymouth	12.87	-43.16	68.91	28.56	0.65
Redcar and Cleveland	31.46	-15.48	78.40	23.93	0.19
Rotherham	16.61	-21.72	54.95	19.54	0.40
Salford	50.21	2.09	98.33	24.53	0.04

Sheffield	92.07	37.13	147.00	28.00	0.00
Shropshire	28.91	-11.33	69.16	20.52	0.16
Southampton	66.28	16.74	115.81	25.25	0.01
Sunderland	37.92	-5.82	81.66	22.30	0.09
Age (4 to 7)	-8.90	-23.56	5.77	7.48	0.23
Age (8 to 11)	-20.32	-35.83	-4.81	7.91	0.01
Age (12 to 19)	-21.43	-36.99	-5.87	7.93	0.01
Age (unknown)	-113.43	-167.59	-59.27	27.61	<0.01
Legal status at pre-proceedings letter date (Child Protection Plan)	10.02	-7.87	27.91	9.12	0.27
Legal status at pre-proceedings letter date (neither)	-2.65	-25.73	20.44	11.77	0.82
Legal status at pre-proceedings letter date (unknown)	229.40	204.88	253.91	12.50	<0.01
Care history (unknown)	-22.37	-48.37	3.62	13.25	0.09
Care history (yes)	33.56	12.84	54.27	10.56	<0.01
Observations	1433				

## Secondary outcome – logistic regression model, court diversion, all predictor variables

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	0.65	-0.97	2.28	0.83	0.43
Trial arm (intervention)	-0.76	-1.20	-0.32	0.22	<0.01
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Birmingham	0.66	-1.60	2.92	1.15	0.57
Bromley	-1.26	-2.79	0.27	0.78	0.11
Derbyshire	-2.13	-3.62	-0.64	0.76	<0.01
Knowsley	15.66	14.04	17.27	0.82	<0.01
Lambeth	-1.10	-4.64	2.44	1.81	0.54
Lancashire	-0.07	-1.60	1.46	0.78	0.93
Leicestershire	-2.36	-4.02	-0.69	0.85	0.01
Lewisham	-0.90	-2.86	1.06	1.00	0.37
Middlesbrough	-1.00	-3.65	1.66	1.35	0.46
North East Lincolnshire	-1.13	-2.68	0.43	0.79	0.15
Northamptonshire	-3.22	-5.34	-1.11	1.08	<0.01

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Nottingham	-1.11	-2.92	0.70	0.92	0.23
Plymouth	15.72	13.85	17.59	0.95	0.00
Redcar and Cleveland	0.25	-1.85	2.34	1.07	0.82
Rotherham	-0.37	-2.63	1.89	1.15	0.75
Salford	-1.41	-3.61	0.80	1.12	0.21
Sheffield	-1.08	-2.67	0.51	0.81	0.18
Shropshire	-2.07	-3.54	-0.59	0.75	0.01
Southampton	-2.48	-4.30	-0.65	0.93	0.01
Sunderland	-2.18	-3.62	-0.74	0.73	<0.01
Gender (male)	0.13	-0.17	0.43	0.15	0.40
Gender (unknown)	1.21	0.17	2.25	0.53	0.02
Ethnicity (White)	0.43	-0.20	1.06	0.32	0.18
Age (4 to 7)	-0.12	-0.53	0.29	0.21	0.56
Age (8 to 11)	-0.15	-0.60	0.29	0.23	0.50
Age (12 to 19)	-0.22	-0.69	0.24	0.24	0.34
Age (unknown)	1.55	-0.25	3.34	0.92	0.09
Number of children in family (2)	-0.46	-1.00	0.08	0.28	0.10
Number of children in family (3)	0.38	-0.18	0.94	0.28	0.18
Number of children in family (4)	-0.45	-1.47	0.56	0.52	0.38
Number of children in family (5+)	0.41	-0.74	1.55	0.59	0.49
Legal status at pre-proceedings letter date (Child Protection Plan)	0.70	0.05	1.34	0.33	0.03
Legal status at pre-proceedings letter date (neither)	-0.36	-1.39	0.66	0.52	0.49
Legal status at pre-proceedings letter date (unknown)	2.82	1.90	3.74	0.47	<0.01
Implementation time (10+ months)	0.34	-0.32	1.00	0.34	0.32
Implementation time (5 to 9 months)	0.56	0.03	1.09	0.27	0.04
Care history (unknown)	-0.26	-1.11	0.60	0.44	0.56
Care history (yes)	1.13	0.51	1.75	0.32	<0.01
Observations	1242				

## Secondary outcome – logistic regression model, court diversion, significant predictors

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	0.74	-0.80	2.28	0.79	0.35
Trial arm (intervention)	-0.67	-1.10	-0.25	0.22	<0.01
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Birmingham	0.66	-1.54	2.86	1.12	0.56
Bromley	-1.40	-2.94	0.14	0.79	0.07
Derbyshire	-2.04	-3.55	-0.53	0.77	0.01
Knowsley	15.61	14.04	17.18	0.80	<0.01
Lambeth	-1.20	-5.06	2.67	1.97	0.54
Lancashire	-0.01	-1.58	1.56	0.80	0.99
Leicestershire	-2.49	-4.12	-0.86	0.83	<0.01
Lewisham	-1.00	-3.09	1.09	1.07	0.35
Middlesbrough	-1.17	-3.77	1.43	1.33	0.38
North East Lincolnshire	-1.05	-2.61	0.52	0.80	0.19
Northamptonshire	-2.99	-5.01	-0.97	1.03	<0.01
Nottingham	-1.33	-3.20	0.53	0.95	0.16
Plymouth	16.11	14.22	18.00	0.96	<0.01
Redcar and Cleveland	0.34	-1.75	2.42	1.06	0.75
Rotherham	-0.56	-2.76	1.64	1.12	0.62
Salford	-1.20	-3.57	1.17	1.21	0.32
Sheffield	-0.97	-2.55	0.60	0.80	0.22
Shropshire	-1.94	-3.43	-0.45	0.76	0.01
Southampton	-2.18	-3.91	-0.45	0.88	0.01
Sunderland	-2.14	-3.58	-0.71	0.73	<0.01
Gender (male)	0.12	-0.18	0.42	0.15	0.43
Gender (unknown)	1.39	0.43	2.36	0.49	<0.01
Legal status at pre-proceedings letter date (Child Protection Plan)	0.78	0.14	1.42	0.33	0.02
Legal status at pre-proceedings letter date (neither)	-0.15	-1.15	0.85	0.51	0.76
Legal status at pre-proceedings letter date (unknown)	2.82	1.88	3.76	0.48	<0.01
Implementation time (10+ months)	0.30	-0.32	0.93	0.32	0.34
Implementation time (5 to 9 months)	0.52	-0.01	1.05	0.27	0.05
Care history (unknown)	-0.32	-1.14	0.49	0.42	0.44
Care history (yes)	1.27	0.65	1.89	0.32	<0.01
Observations	1242				

## Appendix 14. Additional impact analysis tables

Primary outcome, care status, at 6 months post-pre-proceedings letter

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value	Risk ratio
Intercept	1.65	0.54	2.76	0.57	<0.01	
Treatment	0.42	0.14	0.70	0.14	<0.01	0.79 (0.78-0.80)
<b>Fixed effects</b>						
<b>Bath and North East Somerset (base)</b>						
Bromley	0.42	-1.14	1.99	0.80	0.59	
Birmingham	-1.10	-2.29	0.08	0.61	0.07	
Derbyshire	-0.56	-1.81	0.70	0.64	0.39	
Knowsley	-0.71	-2.12	0.70	0.72	0.33	
Lambeth	-0.81	-2.15	0.53	0.68	0.23	
Lancashire	-1.91	-3.14	-0.69	0.63	<0.01	
Leicestershire	-1.79	-3.06	-0.51	0.65	0.01	
Lewisham	-0.99	-2.26	0.27	0.65	0.12	
Middlesbrough	-1.47	-2.76	-0.19	0.66	0.02	
North East Lincolnshire	-1.69	-2.94	-0.44	0.64	0.01	
Northamptonshire	-1.70	-3.00	-0.40	0.66	0.01	
Nottingham	-0.14	-1.61	1.34	0.75	0.85	
Plymouth	-0.99	-2.52	0.53	0.78	0.20	
Redcar and Cleveland	-0.04	-1.48	1.40	0.73	0.95	
Rotherham	-0.46	-1.64	0.71	0.60	0.44	
Salford	-0.68	-1.99	0.64	0.67	0.31	
Sheffield	-0.66	-2.01	0.68	0.69	0.33	
Shropshire	-1.59	-2.81	-0.38	0.62	0.01	
Southampton	-1.27	-2.70	0.16	0.73	0.08	
Sunderland	-0.37	-1.60	0.86	0.63	0.55	
Care history (unknown)	-0.16	-0.66	0.34	0.25	0.53	
Care history (yes)	-1.20	-1.53	-0.87	0.17	<0.01	
Observations	2209					

## Primary outcome, care status, at 18 months post-pre-proceedings letter

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value	Risk ratio
Intercept	1.79	-0.46	4.04	1.15	0.12	
Treatment	0.65	-0.17	1.47	0.42	0.12	0.76*
<b>Fixed effects</b>						
<b>Bath and North East Somerset (base)</b>						
Bromley	-19.01	-21.75	-16.26	1.40	<0.01	
Birmingham	-1.53	-4.07	1.02	1.30	0.24	
Derbyshire	-0.50	-3.24	2.24	1.40	0.72	
Knowsley	-0.08	-2.69	2.53	1.33	0.95	
Lancashire	0.02	-3.24	3.28	1.66	0.99	
Leicestershire	-5.13	-8.39	-1.88	1.66	<0.01	
Lewisham	-1.38	-4.32	1.56	1.50	0.36	
Middlesbrough	-1.24	-4.22	1.74	1.52	0.41	
North East Lincolnshire	-3.10	-5.86	-0.35	1.41	0.03	
Northamptonshire	-15.80	-19.04	-12.56	1.65	<0.01	
Nottingham	14.25	11.67	16.83	1.32	<0.01	
Plymouth	0.50	-2.66	3.67	1.61	0.75	
Redcar and Cleveland	-0.65	-3.80	2.50	1.61	0.69	
Rotherham	-0.40	-2.95	2.16	1.30	0.76	
Salford	-2.74	-6.05	0.56	1.69	0.10	
Sheffield	-0.83	-3.80	2.14	1.51	0.58	
Shropshire	-1.60	-4.19	0.99	1.32	0.23	
Sunderland	-0.38	-2.95	2.19	1.31	0.77	
Care history (unknown)	15.07	13.04	17.10	1.04	<0.01	
Care history (yes)	-2.16	-3.25	-1.07	0.56	<0.01	
Observations	312					

\* Risk ratio not adjusted for local authority and care history, owing to small sample size.

## Secondary outcome, sustainment of outcome, at 6 months post-pre-proceedings letter

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value	Risk ratio
Intercept	-2.27	-4.37	-0.18	1.07	0.03	
Treatment	-0.48	-1.08	0.13	0.31	0.12	0.69 (0.64-0.73)
<b>Fixed effects</b>						
<b>Bath and North East Somerset (base)</b>						
Bromley	0.09	-3.02	3.21	1.59	0.95	
Birmingham	-0.50	-3.11	2.10	1.33	0.70	
Derbyshire	0.87	-1.44	3.18	1.18	0.46	
Knowsley	-16.28	-18.53	-14.03	1.15	<0.01	
Lambeth	-16.66	-19.15	-14.16	1.27	<0.01	
Lancashire	1.39	-0.86	3.64	1.15	0.23	
Leicestershire	1.15	-1.35	3.65	1.28	0.37	
Lewisham	-1.03	-3.99	1.93	1.51	0.50	
Middlesbrough	-16.60	-18.92	-14.28	1.19	<0.01	
North East Lincolnshire	1.34	-0.93	3.61	1.16	0.25	
Northamptonshire	-15.90	-18.30	-13.50	1.22	<0.01	
Nottingham	-16.04	-18.29	-13.79	1.15	<0.01	
Plymouth	0.09	-2.64	2.81	1.39	0.95	
Redcar and Cleveland	0.60	-1.98	3.18	1.32	0.65	
Rotherham	-0.93	-3.93	2.08	1.54	0.55	
Salford	-0.28	-3.40	2.85	1.59	0.86	
Sheffield	1.27	-0.99	3.54	1.16	0.27	
Shropshire	-0.04	-2.29	2.21	1.15	0.97	
Southampton	-15.97	-18.22	-13.73	1.15	<0.01	
Sunderland	-0.54	-3.00	1.91	1.25	0.66	
Care history (unknown)	-0.26	-1.28	0.77	0.52	0.62	
Care history (yes)	0.73	0.01	1.45	0.37	0.05	
Observations	835					

## Secondary outcome, sustainment of outcome, at 18 months post-pre-proceedings letter

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value	Risk ratio
Intercept	-73.16	-78.24	-68.07	2.59	<0.01	
Treatment	1.58	-1.83	4.99	1.74	0.36	2.29*
<b>Fixed effects</b>						
<b>Bath and North East Somerset (base)</b>						
Derbyshire	48.48	43.04	53.92	2.77	<0.01	
Lancashire	-2.03	-6.73	2.67	2.40	0.40	
Lewisham	47.59	41.69	53.50	3.01	<0.01	
Middlesbrough	-1.07	-5.31	3.17	2.16	0.62	
North East Lincolnshire	50.06	45.82	54.30	2.16	<0.01	
Northamptonshire	47.59	42.04	53.15	2.83	<0.01	
Redcar and Cleveland	47.41	41.97	52.85	2.77	<0.01	
Rotherham	-2.65	-8.09	2.79	2.77	0.34	
Shropshire	47.59	41.69	53.50	3.01	<0.01	
Sunderland	50.06	45.82	54.30	2.16	<0.01	
Care history (yes)	48.66	44.56	52.77	2.10	<0.01	
Observations	23					

\* Risk ratio not adjusted for local authority and care history, owing to small sample size.

## Secondary outcome, time spent in care, at 6 months post-pre-proceedings letter

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value	Glass's delta
Intercept	18.93	-1.35	39.21	10.34	0.07	
Treatment	-10.36	-18.08	-2.64	3.94	0.01	0.19 (0.11-0.28)
<b>Fixed effects</b>						
<b>Bath and North East Somerset (base)</b>						
Bromley	-3.17	-28.42	22.09	12.88	0.81	
Birmingham	18.50	-4.52	41.53	11.74	0.12	
Derbyshire	13.96	-11.85	39.76	13.16	0.29	
Knowsley	21.49	-12.72	55.70	17.45	0.22	
Lambeth	22.65	-11.26	56.57	17.30	0.19	
Lancashire	45.41	19.51	71.30	13.21	0.00	
Leicestershire	23.22	-1.08	47.52	12.39	0.06	

Lewisham	24.32	-2.82	51.45	13.84	0.08
Middlesbrough	42.23	13.18	71.27	14.81	0.00
North East Lincolnshire	34.16	8.02	60.29	13.33	0.01
Northamptonshire	64.06	33.67	94.45	15.50	0.00
Nottingham	-4.01	-26.86	18.84	11.65	0.73
Plymouth	20.29	-14.21	54.80	17.60	0.25
Redcar and Cleveland	-1.37	-24.72	21.97	11.91	0.91
Rotherham	14.89	-8.07	37.85	11.71	0.20
Salford	21.35	-8.91	51.61	15.43	0.17
Sheffield	15.82	-9.75	41.39	13.04	0.23
Shropshire	39.00	14.46	63.55	12.52	0.00
Southampton	48.42	9.86	86.98	19.66	0.01
Sunderland	9.21	-13.48	31.90	11.57	0.43
Care history (unknown)	-1.53	-12.65	9.60	5.67	0.79
Care history (yes)	27.37	18.01	36.72	4.77	0.00
Observations	2398				

### Secondary outcome, time spent in care, at 18 months post-pre-proceedings letter

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value	Glass's delta
Intercept	44.83	-58.65	148.30	52.63	0.39	
Treatment	-17.80	-77.07	41.46	30.14	0.56	0.17 (-0.04-0.37)
<b>Fixed effects</b>						
<b>Bath and North East Somerset (base)</b>						
Bromley	283.37	-9.63	576.38	149.02	0.06	
Birmingham	86.22	-49.91	222.36	69.24	0.21	
Derbyshire	97.45	-37.24	232.13	68.50	0.16	
Knowsley	15.75	-119.25	150.75	68.66	0.82	
Lancashire	118.75	-47.29	284.80	84.45	0.16	
Leicestershire	232.87	58.02	407.72	88.93	0.01	
Lewisham	36.09	-124.81	197.00	81.83	0.66	
Middlesbrough	157.58	-54.91	370.06	108.07	0.15	
North East Lincolnshire	277.65	120.03	435.28	80.17	<0.01	

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value	Glass's delta
Northamptonshire	186.85	15.74	357.95	87.02	0.03	
Nottingham	-45.31	-158.76	68.14	57.70	0.43	
Plymouth	8.70	-148.86	166.27	80.13	0.91	
Redcar and Cleveland	76.43	-110.69	263.55	95.17	0.42	
Rotherham	128.74	-24.35	281.82	77.86	0.10	
Salford	172.40	-23.58	368.37	99.67	0.08	
Sheffield	50.14	-119.03	219.30	86.04	0.56	
Shropshire	92.31	-57.96	242.58	76.42	0.23	
Sunderland	13.48	-101.57	128.53	58.51	0.82	
Care history (unknown)	-110.51	-178.28	-42.75	34.46	<0.01	
Care history (yes)	110.67	43.54	177.80	34.14	<0.01	
Observations	401					

## Appendix 15. Additional information on text messages

### Breakdown of text messages by month

Month	Number of mobile phone numbers to which texts delivered	Number of replies received*	Response rate (after reminder)
November 2020	21	8	38%
December 2020	45	14	31%
January 2021	68	19	28%
February 2021	25	4	16%
March 2021	36	11	31%
April 2021	68	15	22%
May 2021	43	9	21%
June 2021	54	16	30%
July 2021	64	12	19%
August 2021	54	11	20%
September 2021	53	12	23%
October 2021	58	14	24%
November 2021	44	14	32%
December 2021	55	10	18%
January 2022	36	6	17%
February 2022	1	1	100%
March 2022	1	0	0%
April 2022	11	2	18%
May 2022	6	1	17%
June 2022	3	1	33%
<b>Total</b>	<b>746</b>	<b>180</b>	<b>24%</b>

\* Includes responses received by 20 June 2022 to first and reminder texts. We count a maximum of 1 reply per phone number.

## Number of phone numbers provided by number of families

Number of phone numbers provided	Number of families from control group	Number of families from intervention group	Total number of families	Percentage of families
1 phone number provided	348	353	701	46%
2 phone numbers provided	129	126	255	17%
3 phone numbers provided	8	6	14	1%
No phone number provided	269	272	541	36%
<b>Total</b>	<b>754</b>	<b>757</b>	<b>1511</b>	<b>100%</b>

\* High percentages for some local authorities are due to >1 number being provided per family.

\*\* All responses, not phone numbers; includes some replies where the number sent more than 1 response; includes substantive and other responses.

Local authority	N phone numbers provided	N texts delivered	N texts delivered as % of families randomised in local authority*	N responses**	Response rate per local authority	% of total responses
Bath and North East Somerset	38	23	100%	7	30%	3%
Birmingham	43	17	14%	7	41%	3%
Bromley	5	4	9%	1	25%	0%
Derbyshire	1	0	0%	0		0%
Knowsley	40	21	44%	10	48%	5%
Lambeth	53	27	56%	8	30%	4%
Lancashire	106	67	53%	29	43%	14%
Leicestershire	0	0	0%	0		0%
Lewisham	123	71	90%	14	20%	7%
Middlesbrough	72	39	72%	17	44%	8%
North East Lincolnshire	7	7	12%	2	29%	1%
Northamptonshire	103	68	64%	18	26%	9%
Nottingham	54	31	66%	9	29%	4%
Plymouth	2	2	10%	1	50%	0%
Redcar & Cleveland	64	48	76%	14	29%	7%
Rotherham	210	131	97%	22	17%	11%
Salford	0	0	0%	0		0%
Sheffield	68	46	75%	15	33%	7%
Shropshire	99	58	59%	21	36%	10%
Southampton	8	5	10%	0	0%	0%
Sunderland	160	81	69%	9	11%	4%
<b>Total</b>	<b>1256</b>	<b>746</b>	<b>49%</b>	<b>204</b>	<b>27%</b>	<b>100%</b>

\* High percentages for some local authorities are due to >1 number being provided per family.

\*\* All responses, not phone numbers; includes some replies where the number sent more than 1 response; includes substantive and other responses.

## Appendix 16. Regression analysis of trial arm and primary outcome (care status at 12 months) excluding non-compliant cases

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value	Risk ratio
Intercept	1.48	-0.14	3.09	0.82	0.07	
Treatment	0.29	-0.17	0.76	0.24	0.22	0.76
<b>Fixed effects</b>						
<b>Bath and North East Somerset (base)</b>						
Bromley	0.03	-2.32	2.38	1.20	0.98	
Birmingham	-1.17	-2.90	0.57	0.88	0.19	
Derbyshire	-0.62	-2.40	1.16	0.91	0.49	
Knowsley	1.03	-0.99	3.05	1.03	0.32	
Lambeth	-1.01	-3.24	1.23	1.14	0.38	
Lancashire	-2.01	-3.91	-0.11	0.97	0.04	
Leicestershire	-1.34	-3.25	0.58	0.98	0.17	
Lewisham	-0.73	-2.57	1.12	0.94	0.44	
Middlesbrough	-0.92	-2.75	0.92	0.94	0.33	
North East Lincolnshire	-2.93	-4.90	-0.96	1.00	<0.01	
Northamptonshire	-1.04	-2.94	0.87	0.97	0.29	
Nottingham	0.08	-2.15	2.31	1.14	0.94	
Plymouth	-0.53	-3.03	1.98	1.28	0.68	
Redcar and Cleveland	0.75	-1.56	3.05	1.17	0.53	
Rotherham	-0.14	-1.85	1.57	0.87	0.87	
Salford	-1.10	-3.16	0.95	1.05	0.29	
Sheffield	-1.03	-2.84	0.78	0.92	0.27	
Shropshire	-1.63	-3.43	0.16	0.92	0.07	
Southampton	-1.91	-4.20	0.38	1.17	0.10	
Sunderland	-0.41	-2.20	1.37	0.91	0.65	
Care history (unknown)	-0.33	-1.12	0.46	0.40	0.42	
Care history (yes)	-1.55	-2.17	-0.93	0.32	<0.01	
Observations	928					

## Appendix 17. Complier average causal effect (CACE) regression analyses

### Primary outcome: care status at 12 months

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	0.21	-0.06	0.49	0.14	0.12
FGC received	-0.16	-0.31	-0.02	0.07	0.03
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	0.02	-0.33	0.38	0.18	0.90
Birmingham	0.17	-0.12	0.47	0.15	0.25
Derbyshire	0.15	-0.16	0.46	0.16	0.35
Knowsley	-0.03	-0.32	0.25	0.14	0.82
Lambeth	0.02	-0.33	0.38	0.18	0.89
Lancashire	0.36	0.02	0.70	0.17	0.04
Leicestershire	0.21	-0.14	0.55	0.17	0.24
Lewisham	0.12	-0.20	0.45	0.16	0.45
Middlesbrough	0.20	-0.11	0.52	0.16	0.21
North East Lincolnshire	0.46	0.15	0.77	0.16	0.00
Northamptonshire	0.26	-0.09	0.60	0.18	0.14
Nottingham	-0.06	-0.37	0.24	0.16	0.68
Plymouth	-0.04	-0.37	0.28	0.17	0.80
Redcar and Cleveland	-0.02	-0.33	0.30	0.16	0.90
Rotherham	0.01	-0.28	0.30	0.15	0.95
Salford	0.20	-0.15	0.55	0.18	0.26
Sheffield	0.16	-0.16	0.48	0.16	0.32
Shropshire	0.38	0.07	0.69	0.16	0.02
Southampton	0.36	-0.06	0.79	0.22	0.09
Sunderland	0.09	-0.21	0.39	0.15	0.55
Care history (unknown)	0.05	-0.10	0.20	0.07	0.50
Care history (yes)	0.29	0.19	0.38	0.05	0.00
Observations	1227				

## Secondary outcome: sustainment of outcome at 12 months

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	0.29	-0.21	0.79	0.25	0.26
FGC received	0.01	-0.21	0.22	0.11	0.95
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	-0.12	-0.74	0.50	0.31	0.70
Birmingham	-0.32	-0.82	0.18	0.25	0.21
Derbyshire	-0.07	-0.58	0.44	0.26	0.79
Knowsley	-0.35	-0.83	0.14	0.25	0.16
Lancashire	0.00	-0.52	0.53	0.27	0.99
Leicestershire	0.03	-0.61	0.68	0.33	0.92
Lewisham	-0.26	-0.76	0.24	0.25	0.31
Middlesbrough	-0.35	-0.85	0.15	0.25	0.17
North East Lincolnshire	0.26	-0.31	0.82	0.29	0.37
Northamptonshire	-0.37	-0.90	0.17	0.27	0.18
Nottingham	-0.31	-0.79	0.18	0.25	0.21
Plymouth	-0.33	-0.82	0.15	0.25	0.18
Redcar and Cleveland	-0.10	-0.65	0.44	0.28	0.71
Rotherham	-0.29	-0.84	0.25	0.28	0.29
Salford	0.07	-0.57	0.71	0.32	0.83
Sheffield	-0.12	-0.64	0.40	0.26	0.64
Shropshire	-0.24	-0.74	0.26	0.25	0.34
Southampton	-0.29	-0.78	0.20	0.25	0.24
Sunderland	-0.22	-0.71	0.28	0.25	0.39
Care history (unknown)	0.08	-0.16	0.31	0.12	0.52
Care history (yes)	0.12	-0.01	0.26	0.07	0.07
Observations	401				

## Secondary outcome: time in care at 12 months

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	41.13	-24.77	107.04	33.60	0.22
FGC received	-36.45	-75.34	2.43	19.82	0.07
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	19.38	-73.99	112.74	47.59	0.68
Birmingham	34.47	-36.41	105.35	36.13	0.34
Derbyshire	43.87	-31.16	118.90	38.25	0.25
Knowsley	2.93	-67.12	72.98	35.71	0.93
Lambeth	37.87	-63.27	139.01	51.56	0.46
Lancashire	69.75	-7.40	146.90	39.33	0.08
Leicestershire	46.73	-33.18	126.63	40.73	0.25
Lewisham	41.85	-38.42	122.11	40.92	0.31
Middlesbrough	89.80	6.04	173.56	42.70	0.04
North East Lincolnshire	105.87	27.85	183.89	39.77	0.01
Northamptonshire	115.43	24.57	206.28	46.32	0.01
Nottingham	-15.21	-88.73	58.32	37.48	0.69
Plymouth	8.58	-71.94	89.10	41.05	0.83
Redcar and Cleveland	17.30	-60.57	95.17	39.70	0.66
Rotherham	30.82	-40.02	101.65	36.11	0.39
Salford	52.58	-28.36	133.52	41.26	0.20
Sheffield	64.30	-14.87	143.47	40.36	0.11
Shropshire	99.04	22.61	175.46	38.96	0.01
Southampton	116.31	7.39	225.24	55.53	0.04
Sunderland	13.07	-55.38	81.51	34.89	0.71
Care history (unknown)	8.83	-25.78	43.43	17.64	0.62
Care history (yes)	73.77	49.03	98.50	12.61	<0.01
Observations	1433				

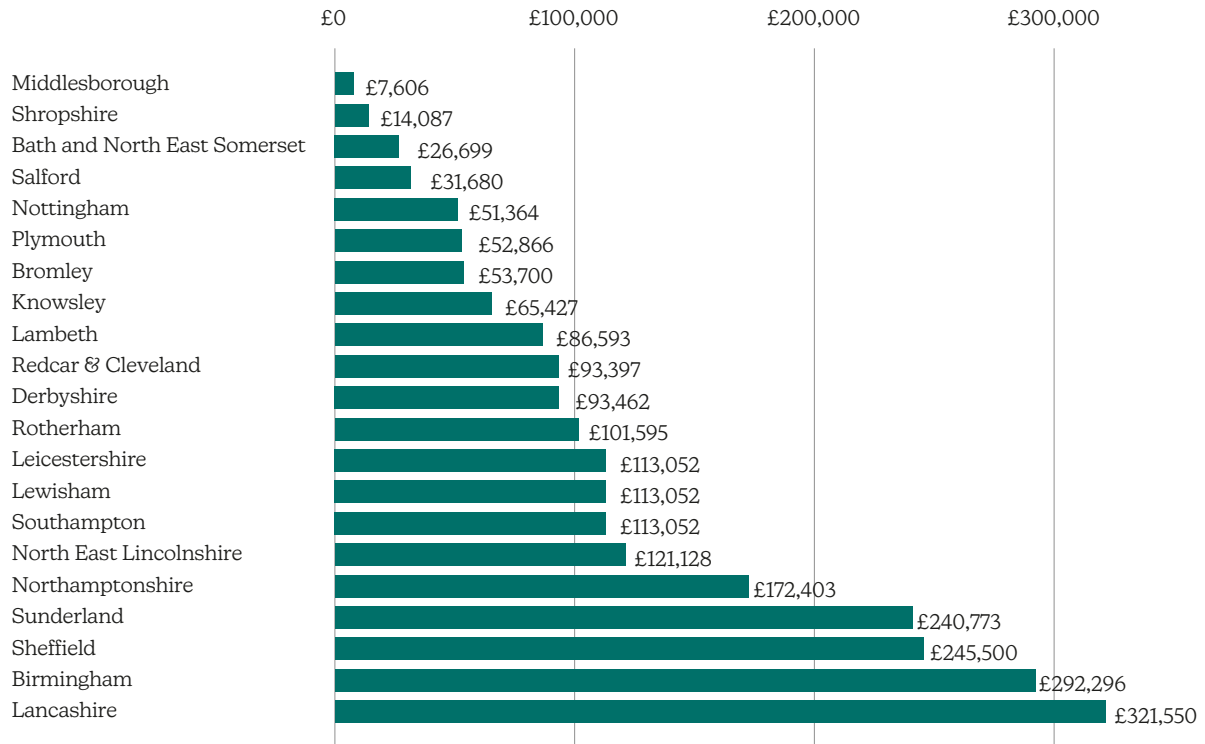
## Secondary outcome: court diversion

	Co-efficient	Lower 95% CI	Upper 95% CI	Clustered standard error	p-value
Intercept	0.94	0.74	1.14	0.10	<0.01
FGC received	-0.28	-0.44	-0.12	0.08	<0.01
<b>Fixed effects</b>					
<b>Bath and North East Somerset (base)</b>					
Bromley	0.00	-0.25	0.24	0.12	1.00
Birmingham	-0.33	-0.58	-0.08	0.13	0.01
Derbyshire	-0.40	-0.64	-0.16	0.12	<0.01
Knowsley	0.17	-0.03	0.37	0.10	0.10
Lambeth	-0.20	-0.61	0.21	0.21	0.33
Lancashire	-0.07	-0.29	0.14	0.11	0.50
Leicestershire	-0.38	-0.64	-0.12	0.13	<0.01
Lewisham	-0.11	-0.36	0.14	0.13	0.38
Middlesbrough	-0.23	-0.60	0.13	0.18	0.21
North East Lincolnshire	-0.26	-0.49	-0.03	0.12	0.03
Northamptonshire	-0.62	-0.92	-0.32	0.15	<0.01
Nottingham	-0.31	-0.60	-0.02	0.15	0.03
Plymouth	0.14	-0.10	0.37	0.12	0.25
Redcar and Cleveland	-0.02	-0.28	0.24	0.13	0.89
Rotherham	-0.12	-0.34	0.10	0.11	0.28
Salford	-0.13	-0.48	0.22	0.18	0.46
Sheffield	-0.30	-0.57	-0.04	0.14	0.03
Shropshire	-0.28	-0.51	-0.05	0.12	0.02
Southampton	-0.52	-0.82	-0.22	0.15	<0.01
Sunderland	-0.50	-0.72	-0.28	0.11	<0.01
Care history (unknown)	0.06	-0.08	0.20	0.07	0.39
Care history (yes)	0.19	0.10	0.28	0.04	<0.01
Observations	1251				

# Appendix 18. Breakdown of programme costs by local authority

Data quality concerns mean the numbers below are subject to uncertainty.

## Total costs of the first year of the programme to local authorities, by local authority



Note: data mostly relates to financial year 2021/22 and includes estimates for the three local authorities that did not provide costs data (Leicestershire, Lewisham and Southampton). Excludes costs incurred by outsourced providers in the areas with external FGC services.

## Unit cost per FGC by local authority in the first year of the programme, including costs to outsourced providers

	Total cost, including start-up costs in first year	Number of FGCs in first year	Unit cost per FGC, including start-up costs	Unit cost of FGC per child, including start-up costs
Bath and North East Somerset	£26,699	6	£4450	£2618
Bromley	£69,771	7	£9967	£5863
Birmingham	£292,296	14	£20,730	£12,194
Derbyshire	£93,462	20	£4673	£2749
Knowsley	£65,427	17	£3844	£2261
Lambeth	£86,593	8	£10,691	£6289
Lancashire	£321,550	31	£10,214	£6008
Leicestershire	£194,881	14	£13,920	£8188
Lewisham	£196,951	20	£10,028	£5899
Middlesbrough	£7606	9	£880	£518
North East Lincolnshire	£121,128	8	£15,141	£8906
Northamptonshire	£306,620	26	£11,793	£6937
Nottingham	£51,364	6	£8561	£5036
Plymouth	£52,866	4	£14,524	£8543
Redcar & Cleveland	£93,397	18	£5241	£3083
Rotherham	£101,595	29	£3547	£2087
Salford	£31,680	7	£4645	£2732
Sheffield	£245,500	13	£18,885	£11,109
Shropshire	£14,087	24	£598	£352
Southampton	£166,315	9	£18,645	£10,968
Sunderland	£240,773	24	£10,185	£5991
<b>Total</b>	<b>£2,780,561</b>	<b>312</b>	<b>£8911</b>	<b>£5242</b>

# Appendix 19. Case study families

## Case study 1 (FCG took place)

This was a complex case with an extremely vulnerable birth mother. Four children had been previously removed from her care.

The FGC was hybrid (with a mixture of 17 people joining online and meeting in-person). A review meeting of 16 people, 4 months later, was fully online using Zoom.

The outcome for the child who was the subject of the FGC was that they were to be placed for adoption. Therefore, at the point of the last follow-up interview with a social worker the child was still technically looked-after (but settled and doing well in an early permanence placement).

Usual services for this family during pre-proceedings meant a 14-week parent and baby residential placement comprising a Parenting Assessment Manual (PAMS) assessment.<sup>47</sup> The social worker described the assessment centre as “quite good” because it provided detailed reports.

The FGC did not make a significant difference from the social worker’s perspective. In their opinion the outcome was a foregone conclusion due to the significant risk of harm. However, the FGC did give professionals the opportunity to understand more about the extended family network and the support available from family and friends. A family friend was assessed as a potential carer as a result, though this option later fell through. It also provided the extended family with a better understanding of the significant challenges that existed. In one social worker’s opinion, the FGC review meeting was more successful and it helpfully kept the extended family up to date with the likelihood of the child being adopted.

At the point of the interview with the mother she was hopeful that her child would return to her and her partner’s care from the foster carers. She felt that the removal of her child soon after birth was sudden and unexpected. The mother did not feel the social workers had been helpful or supportive, and did not feel listened to. The mother, who had had two changes in social worker during pre-proceedings, had a negative experience of the residential placement, describing it like “a prison”. During her 14 weeks there, she felt cut off from family and friends, especially as it was in a different city.

Her experience of the FGC was good. Although she felt she was not given a choice about whether to attend, she invited lots of family to attend and felt able to explore what support was available to her if her child moved home. She felt it helped her understand who was going to give her and the baby support. However, the mother felt that the FGC did not make a difference in the long term and that the social worker did not take on board the suggestions.

<sup>47</sup> An assessment that considers the parent’s personal history, views and attitudes towards parenting, understanding of child development and the child’s needs, the resourcefulness of the parent to seek help and support and who forms their support network. The assessment can take up to 12 weeks.

The internet connection for the FGC was not good. The mother's understanding of the meeting was to explore what support was available to them. Generally, she felt hard done by and reported:

**“They [social workers] could have listened to our views, they could have given more support in [the residential placement in the different city] or moved us somewhere else. They could have listened to us, and they could have bought [the baby] home and see what support we would be getting from home.”**

The follow-up interviews with two social workers confirmed that the baby was doing well and meeting all developmental milestones.

On reflection both social workers at the follow-up interviews thought that the FGC had a small effect on the outcome for the child. One social worker reflected that an aunt and uncle were very supportive to the mother and this could have been as a result of the FGC process.

## Case study 2 (no FGC offered)

This family was not referred for an FGC. The interviews were an opportunity to hear about usual services during pre-proceedings.

The parents were sent a letter before proceedings two months before their baby was born. They had had previous children removed from their care, though the mother was in contact with most of them.

At the time of the birth, the parents were receiving intensive daily daytime support from a family support practitioner and from an external family support organisation. The support worker discussed caring for the baby and meeting its needs. The social worker felt this intensive support was beneficial:

**“They’re there to do that hands-on support alongside parents. I think at times they spend five or six hours a day in the family home, so they were there for quite a long time. So I guess they had quite a good idea of the parenting that the parents were offering. I guess that was a real positive in the pre-proceedings.”**

During the night the parents were supported by the baby’s maternal grandparents.

During pre-proceedings one parent had an advocate and received a parenting assessment. However, a social worker reported this was not very “in-depth and robust”. There was a “family meeting” between the social workers, parents and maternal grandparents. This meeting was productive and focused on putting in place a safety plan for the baby and establishing everyone’s role in caring for the baby. The social worker at the follow-up interview felt that this meeting was not at all similar to an FGC:

**“They didn’t have any sort of meetings with the whole family. I guess there were kind of conversations with grandparents around kind of the support they offer to them, but it wasn’t sort of done in a formal way.”**

One parent received support from adult social care and primary health care mental health support, but this was limited. The baby’s paternal grandparents also offered some support, but lived further away.

Generally, the parents felt a lot of pressure during the pre-proceedings stage and they experienced many social worker changes. They had a good relationship with their social worker at the time of the parent interview.

The parenting assessment was positive and pre-proceedings ended. At the time of the follow-up interview, the baby lived with the parents under a Child Protection Plan and was doing fairly well, although behind on some developmental milestones. The Child Protection Plan was, their social worker felt, likely to stay in place “for a while” to give the parents time to demonstrate positive changes will be sustained.

In the social workers’ view, an FGC may have been helpful for this family at pre-proceedings stage. This was because it would have shared some of the child-caring responsibilities with the wider family and would have helped them feel more in control of their situation:

**“And feeling like they [the parents] have a bit more ownership of their situation themselves, being able to make some of those decisions about how support looks for them themselves. I guess that’s a real benefit of an FGC is that it brings everyone together and they know the family better than anyone. They can maybe suggest things that professionals wouldn’t have thought of. It’s amazing what parents and grandparents and friends are willing to offer.”**

In the social worker’s view, usual support received during pre-proceedings was beneficial and influenced the outcome for the child:

**“I think the parenting assessment definitely made mum feel reassured that when she’s alongside someone that can kind of support her appropriately that her parenting is acceptable once she’s supported.”**

### Case study 3 (FGC took place)

This family's FGC was held online and followed up by a review meeting.

This family had had children previously removed many years ago, so social workers had a high degree of concern about their ability to safely care for the new baby. The mother and father had been traumatised by the previous removal of their children. However, this time things felt different, because the parents felt they had more control over their situation. The FGC was planned for the baby's due date and, when the baby came a few weeks early, the FGC took place as scheduled. A lot of safety-planning (i.e. planning what actions to take if any risky situations arise) was completed before the FGC. So in the social worker's view, when the FGC happened, there was "quite a robust plan in place in terms of baby returning home with mum and dad. And in terms of maternal uncle moving in with parents, fully living there."

The parents told us they had a positive experience of receiving a parenting assessment from a support and assessment worker during pre-proceedings. The parents described the support they received as "daily" (a mixture of phone calls, video calls and in-person visits) and said the worker was "lovely" and "good to work with".

Their social worker felt the FGC was helpful for reiterating the safety plan, but thought an FGC would have been more helpful earlier in the process. The social worker was hesitant to attribute the child's outcome to the FGC.

The family had a good relationship with their social worker and felt that social care "let us have an input rather than just throw everything at us".

The parents said the FGC was not helpful because they had already had a family network meeting and the FGC duplicated the plan. However, they recognised how an FGC could be helpful for people with fewer support networks. They found their FGC chaotic and an important family member experienced technical issues trying to join. One of the parents suggested:

**"The second [review FGC meeting] we spoke for 20 mins, it was the same meeting, nothing happened after the second meeting. They should do one or the other - FGC or family support meeting. There should not be both."**

The family told us they had felt "completely" involved in care planning for their child (using the same scale we asked of parents in text messages). As they had such a positive relationship with their social worker and the support and assessment worker, this can probably be attributed to that.

Pre-proceedings ended with the baby still living with mum and dad. The baby was on a Child Protection Plan and the social worker was hopeful this would soon step down to a Child in Need plan. When we got in touch with the social worker to arrange a follow-up interview, they stated that there were no new updates to give.

## Case study 4 (no FGC offered)

Care proceedings were entered for this family two months after the letter before proceedings. The social worker reported that the parent did not engage well during the pre-proceedings phase and pre-proceedings were therefore “ineffective”. There was no FGC.

The mother has two children. Pre-proceedings were entered for youngest child. The other child lived with grandparents, was not on a Child Protection Plan and their mother had regular contact.

The mother reported having a good relationship and support from her social worker and other services. The same social worker had been supporting the family for a few years through Child Protection Plans.

When the youngest was born, the mother and baby went to live in a parent and baby foster placement at a foster carer’s home. She described this to us as “the best experience” and “completely turned my life around”. The baby was under an Interim Care Order at this stage. Care proceedings ended around a year after they had been entered. The mother felt apprehensive about the support stopping. However, according to our follow-up interview with the social worker, she and her baby were living together independently and “went from strength to strength”. The local authority had closed the case. The older child was having more contact time with their mum.

The usual support received were a parenting assessment, a psychological assessment, regular social worker visits and housing support. In this social worker’s view, the support received was not different from the usual support received when a child is on a Child Protection Plan but not in pre-proceedings. The mother also received support from the foster carers, health visitors, substance misuse worker and an online 12-week domestic violence support programme. The mother welcomed the support and the various assessments she received.

There was no family network meeting because the social worker felt the maternal grandparents were already aware of the situation and there was nothing more a network meeting could add. The social worker felt the same way about an FGC at the initial interview – it would not have added anything beneficial. However, at the follow-up interview, the social worker reflected that an FGC could have been helpful but the mother would have been resistant to the idea because she would not have liked having her difficulties discussed in a room full of people, preferring smaller group or one-to-one conversations.

The parent discussed a relative being assessed as a back-up carer and how she felt isolated and alone. The parent also discussed having a “safety meeting” that was chaired by their social worker, which “brought everyone together and got everyone on board”. During this meeting the parent was given an hour alone with the extended network and then came back to review. This approach seems similar to an FGC.

This mother felt completely involved in the care planning of her child: “100% [I feel involved], no decisions are made without my say-so”.

## Case study 5 (FGC took place)

There are four children in this family. The mother is care-experienced. Pre-proceedings began and then the mother signed a section 20, although she described not understanding what this meant and found the process “very, very confusing”. Pre-proceedings lasted longer than usual for this case and “dragged on and on with no decision” being made, in the mother’s words. The mother felt “slightly” involved in the care planning during pre-proceedings, when we asked this, using same scale as the text message question.

Two FGCs took place. The first one was unsuccessful and disorganised. The second FGC was therefore not a review FGC but a replacement of the first failed FGC.

The first FGC was held online, two months after entering pre-proceedings. The aim, according to the social worker, was to “look at the family network and other potential carers” but it became a space for attendees to express their negative views about the children’s social care team. This FGC would not meet the standards set out in FGC good practice. The family only had ten minutes of private time to discuss plans and the meeting was shorter than a typical FGC. The family were not well prepared for the FGC. The relationship between the mother and one of her family members broke down following the FGC. The mother felt there were people at the meeting who should not have been there – previous partners who were fathers of the children. The mother found the meeting disorganised and overwhelming:

**“It was a bit too much. I didn’t have a phone that was compatible so I couldn’t join in a video. I could just about hear what people were saying. It was right at the beginning [of pre-proceedings], and I wasn’t doing too well right at the beginning. There was a lot of falling out between family members, like myself and my family members and it was a shambles really.”**

The FGC did not result in an agreed care plan:

**“No [care] plan was made because it was confusing and everyone bickering on the line, including myself. No one could hear me anyway because I wasn’t actually in it properly [because the mother attended via telephone].”**

A second FGC took place four months later and went more smoothly. It was attended by fewer people. The mother talked about the core questions being misleading<sup>48</sup> because they focus on the child remaining with the birth parent(s). The parents therefore felt surprised and found it contradictory when court proceedings were discussed:

**“The main [core] question was highlighting the return [of the children] to me so everyone, what they [the network] would do to support me, and vice versa for the carers. I actually had high hopes the children would come to me and the next thing I know it’s going to court.”**

The mother received a parenting assessment, a psychological assessment, daily calls from the social worker and regular virtual and in-person visits. The social worker had contact

<sup>48</sup> The two compulsory core questions are:

1. What support can the family network provide to help ensure that the child/ren can live in a sustainable, safe and well-cared-for environment with parent/s?
2. If assessments show that it is not possible for child/ren to live with parent/s, who, within the wider family network, is willing to put themselves forward to provide suitable, safe and sustainable care for the child/ren? What support can others in the family network offer to those putting themselves forward to care for the child/ren?

with the school, supervised contact was arranged and financial assistance was provided, in the form of new furniture for a relative's home. A special guardianship assessment of the relative took place simultaneously. The mother also talked about a hair strand (drug and alcohol) test. She felt that she had received a lot of assessments but not much support as a parent – for instance, someone to talk through challenges with:

**“The psychological assessment, the parenting assessment, that isn't really support that's you being, the parent being assessed by people you don't know. It's only through a couple of hours or maybe a couple of sessions and they judge your whole life basically and write it all up.”**

She also found that the parenting assessment was like a test, rather than a parenting course that aims to upskill and support parents:

**“I've heard about parenting courses and I thought that was what I was going to maybe have to do. That actually helps the parents. The [parenting] assessment wasn't like that, it's like a test. [You need to] understand the questions in the assessment because they can say it to you in any way and they say a dozen and a half questions in a short space of time. A course probably would be better.”**

The mother felt quite uncertain and lost about the plan forward. This was partly because of the number of changes in social worker during her involvement with social care:

**“But it's kind of like ok well the judge has spoken in court, where do we go from now? They never said 'you can call on someone else while I'm not here.' There's a new one [social worker] now. So actually, we've had four social workers in one year.”**

At the follow-up interview, with another new social worker, they reflected on how children's social care were more cautious to step the case down because of the high turnover of social workers. This meant there was not a clear, longer-term established picture of what was going on for the mother and child:

**“I think when there is a lot of changes in [social] workers I think our decisions can be really risk-averse and I see that a lot, when there is a high turnover of staff because there isn't that continuity of a worker knowing you.”**

They also discussed how the mother had received a poor service from children's social care:

**“The experience she has had with social services has been pretty horrendous considering that she has had so many different social workers; so much gets lost in that constant transition of workers.”**

By the time of the follow-up interview with the social worker, all children were living with their mother at home under a Supervision Order. There were delays to the court hearing but the social worker was confident that the Supervision Order would end at the next court hearing. The social worker described two additional family network meetings that had taken place during the social worker's time holding the case. These were described as not positive and the family spent a lot of time berating social services. The social worker believed that talking individually with family members was the best way to work with this

family. The social worker has recommended that the parent attend the Triple P parenting programme and a programme about positive relationships with partners.

### **Case study 6 (no FGC offered)**

This was a family of 3 children, but pre-proceedings related to 2 of them, as 1 was over 18. Before the letter before proceedings, the children had been subject to Child Protection Plans for four years. Pre-proceedings lasted five months, after which the children, still living with their mother, moved to a Child in Need plan. Pre-proceedings caused a lot of anxiety for the mother.

As part of usual services during pre-proceedings the family received a psychiatric assessment and a parenting assessment that lasted around two months. The parent assessment workers visited the mother every day for three weeks. This was a positive experience, she told us:

**“That [the visits everyday] was fine, they were lovely ladies, reassuring. They checked on [me] and the kids, watching interactions and how the kids were.”**

In addition, core group meetings were held every six weeks. There was also some support around substance misuse, which mother found “horrendous” and a course about recognising healthy relationships (in relation to domestic violence). By contrast the mother thought these classes were “fantastic”.

The family experienced a high level of social worker turnover during pre-proceedings.

An online family network meeting was held in the final month in pre-proceedings. The mother was involved in selecting who should be invited to the meeting. The family were given private time to discuss the plan. The mother said it was “really good” and the meeting stood out as a significant positive experience and it helped her see the support network she had:

**“Yeah it does [feel different to other meetings] because it was sort of my network, my friends, [child’s name]’s grandad, [paternal] Mum. It weren’t no professionals saying: ‘This is what you should do.’ How do you know? Have you walked in my shoes? Have you been through this? You just see different cases and stereotype everyone.”**

The social worker at the initial interview echoed this:

**“She has her network. It illustrated that for her – she felt not alone. The family network symbolically pulled out that network.”**

Although the mother described the difference the family network meeting made, she also thought it would have been more helpful to have it a couple of years earlier:

**“It [the family network meeting] changed a lot for me as I heard from my dad, how he feels and how my best friend feels and how [child’s name]’s nan feels – all different levels of support. As I say, [name of best friend]’s a best friend but she’s more there for the kids. My dad is like for me, like his little girl, and then you had [paternal] mum, which is to do with [name of child]. Everyone was covered but in**

**different aspects, which was really good. Really it should have been done sooner.”**

The 13-year-old we interviewed was not involved with the family network meeting, but had been involved in other meetings with social workers. The young person felt confused by different meetings and some of the questions they were asked at meetings:

**“... like when they say meetings, it’s like, I don’t know what meeting they mean because there’s so many.”**

There was no follow-up interview with the social worker after several attempts to arrange one.

## Case study 7 (FGC took place)

This family contained 2 children, aged 11 and 12, on Child Protection Plans. An online FGC took place the month after the letter before proceedings was sent. Six months post-FGC, the Child Protection Plans ended, and one month after that pre-proceedings ended.

The FGC was attended by nine people, including the children. This was the only case study where children were reported to have attended the FGC, although many of the children in other cases were very young. There were four possible options for the children's living arrangements to be explored at the FGC. From the social worker's perspective, the FGC taking place during pre-proceedings made it well-timed. The social worker believed the coordinator communicated the purpose of the meeting well to the family before the meeting so they knew what was expected of them. However, the social worker thought the online format meant that the meeting was less formal. For example, the children kept dropping in and out of the session, which brings into question how meaningfully involved they were in the meeting. A care plan was successfully made and agreed at the FGC. In the social worker's opinion:

**“The plan [made at the FGC] gives [the children] reassurance that they will still get to see the rest of their family.”**

The usual support received during this time was about five parenting assessments carried out by the case-holding social worker and drug and alcohol testing. The social worker also discussed the other routine meetings that took place (the PLO meeting and Child Protection Conferences) and how the number of meetings may have caused some confusion for the family.

The final arrangement for the children was living with an aunt and her partner under a (pending) Child Arrangement Order (through private law) with a Child in Need plan. The social worker was confident that the children would step off the Child in Need plan. The arrangement was working well, and the children were settled with their aunt. They had, the social worker said, “been clear that they love [parent] but they're happier with auntie”. The children were having supervised contact with their mother and seeing their father.

For this case, in the social worker's view, the FGC reassured the children that they would not be cut off from extended family. We were not able to interview the family directly.

## Case study 8 (no FGC offered)

There were 3 children in the family, aged between 11 and 15 years old. The family received a lot of support from usual services. This included a parenting (PAMS) assessment, a cognitive assessment, practical and financial support with cleaning, decorating and repairing their home, support from a visiting Early Help worker every one to two weeks and a domestic abuse programme (which the mother described as “useful” and said she “learned a lot” from). The child we interviewed received regular support from a nurse around healthy eating and the children were referred to CAMHS. As the social worker put it in the follow-up interview:

**“I just think we’ve given the family everything ... there’s nothing else we could have done for the family, the only thing missing is a library card, because we’ve given them everything, you know we’ve given, we have involved Early Help, health, RAP, social worker, solicitors, [extended family] up until recently, [father] was back in, education ...”**

The interview with the mother and child revealed that the mother expected the children to remain in her care. She felt that she was treated fairly sometimes but generally held quite a negative view of the pre-proceedings process and children’s social care. She said she understood what was happening during the process and that the children, although they did not attend any meetings, had some understanding of what was happening. The mother felt there were no other support services that would have been helpful for the family. She did talk about a local five-week parenting course that was aimed at supporting children with special educational needs – she enjoyed this and it was helpful:

**“It were understanding children with special needs and how to deal with them, you know, when they were in a meltdown, and how not to do accidental rewards ... just a few things that I’ve now put in place with her, and it’s made differences.”**

At the follow-up interview with the same social worker, we heard that the parent had declined. Healthy eating and obesity were still a major problem for one child. At the same time, in their interview with us, this child expressed how they adamantly wanted to stay in their mother’s care. The children were, however, doing well at school and college and had excellent attendance. The family were still in pre-proceedings. A viability assessment had been due to take place of an aunt, but the aunt withdrew. The social worker was extremely concerned about this withdrawal of support:

**“And they’re [the aunt and her partner] the only positivity in these children’s lives other than the professionals going in and out and I, I, just I do feel if we’ve not got eyes on the children from a family’s perspective, I’m really concerned.”**

The social worker reflected that an FGC would have been useful for this family to link in these family members before their withdrawal. It felt like a missed opportunity. At a previous panel meeting a family network meeting was requested, but as there were now no family or friends to bring together it did not seem relevant. The case was going back to the legal panel and care proceedings may be commenced (and in the social worker’s opinion should be):

**“After two years [of working with the family] if we can’t bring about change then we have to recognise that change isn’t going to be achieved and make decisions in the best interest for the children.”**


In the social worker’s view, the children would have gone into care if they were younger. They expected the children either to go into foster care or continue to receive Early Help support until they turned 18.

# // Foundations

What Works Centre for Children & Families

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