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Title Impact of Family Group Conference referrals at pre-proceedings stage on child outcomes: A randomised controlled trial

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Abstract

Previous evidence on the impact of Family Group Conferencing in the UK cannot establish causality. This paper presents results from the first UK randomised controlled trial of Family Group Conferences. All families entering pre-proceedings between September 2020 and May 2022 in 21 local authorities in England were eligible for inclusion in the trial. In total, 1,511 families were randomised and child outcomes were collected from administrative data until up to 21 months post-referral. Half of families were randomly allocated to be referred for a Family Group Conference, on top of usual practice during pre-proceedings. The (pre-registered) primary outcome is the child's looked after status 12 months after the pre-proceedings letter. This was 8.6 percentage points lower among referred families (36.2% vs 44.8%). Children were also less likely to have care proceedings issued (59.2% vs 71.7%) and spent less time in care (87 vs. 115 days). However, there was no significant difference in whether a child's living arrangement remained the same or changed in the months after the decision. These findings have implications for support given to families during pre-

proceedings; they provide evidence in favour of Family Group Conferences as a way to reduce the chances of children going into care.

Key words: Child protection, court proceedings, decision making, Family Group Conferencing, pre-proceedings, randomised controlled trial

Teaser text

Family Group Conferences (FGCs) are meetings led by family members to plan and make decisions for children who are at risk, with the aim of agreeing a plan to keep the children safe. This randomised controlled trial found that children whose families were referred for an FGC before care proceedings began were significantly less likely to be in care twelve months after entering pre-proceedings than those whose families were not referred for an FGC. These findings provide evidence in favour of FGCs as a way to reduce the chances of children going into care.

Introduction

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 (Frost, Abram & Burgess, 2014). FGCs can also be called, for example, Family Group Decision Making or Family Team Meeting. In some cases these are different names for the same model of decision making, in other cases they may refer to similar but different models. The aim of an FGC is to engage families in the planning for the child who is at risk, enable support for the wider network around the child, and improve relationships between the family and services. This means that as well as the immediate family, others may also attend an FGC, including extended family, friends, neighbours, advocates, carers, interpreters and professionals. Ultimately the intention is for children to be able to stay within the family network in a way that is stable and safe (Taylor et al., 2020). They originate from New Zealand, and follow a format that draws upon Māori culture. Their development was a response to the large number of Māori children removed into state institutions. Nowadays, FGCs are used in approximately 30 countries worldwide (Family Rights Group, 2011) and are a legal requirement in some circumstances in some countries (Dijkstra et al., 2019).

FGCs take place during pre-proceedings, sometimes described as the ‘PLO’ or Public Law Outline stage (Ministry of Justice, 2021). This is the process by which the local authority decides whether to initiate proceedings to become involved in the child’s care. 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) having a child protection plan to being a looked-after child, with caring responsibility removed from the parent. The local authority’s concerns are usually about abuse or neglect. In 2021-22, there were 14,954 cases of local authorities making public law applications to initiate proceedings (Cafcass, 2022). Wood et al. (2022) conducted a UK survey which found FGCs were in widespread use, most 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. Every year there are perhaps thousands of FGCs in the UK, but the precise number is unknown.

The pre-proceedings stage can be terminated by a local authority if the parent(s) make positive changes or, for example, if the family agree to the children being looked after by alternative carers. Family court data for England and Wales shows it took 45 weeks on average for disposal (going from application to care order etc.) to be reached in 2021 (Ministry of Justice, 2022).

There are five stages to an FGC: referral, preparation, conference, implementation of the plan and review of the plan (Family Rights Group, n.d.a). The conference itself is a one-off intervention of around three hours in length which takes place in a neutral location rather than the family home or local authority (Early Intervention Foundation, 2018). There are two key features of an FGC that distinguish it from other interventions such as Family Network Meetings: that family groups are given private time to come-up with their own plan for the care of the child(ren), and that the co-ordinator of the FGC is independent (i.e. not the family’s social worker). There is no statutory requirement for an FGC; government guidance merely states local authorities ‘should consider’ FGCs (Department for Education, 2014). However, 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, “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). In response, the Government’s *Stable Homes, Built on Love* strategy (Department for Education, 2023) emphasises the importance of family-led decision-making and sets out plans to test the implementation of family group decision-making and family network support packages through the Families First for Children Pathfinder.

Under the Department for Education’s *Supporting Families: Investing In Practice* programme, a model of FGCs was delivered similar to that used in some other local authorities and offered by other providers (Taylor et al, 2020). This involves an independent coordinator and an allowance for private time for the family during the meeting. The charity *Daybreak* delivered training and support to local authorities, encouraging the use of core questions for discussion in FGC meetings, and adherence to time limits for steps of the process. The programme funder was What Works for Children’s Social Care.

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, UK, Canada and Sweden. Of the 33 studies they considered, 24 were assessed as having a high risk of bias. They concluded that the evidence base was of poor quality. Eight of the studies were randomised controlled trials (RCTs) but even among these, only one was judged to have a low level of bias. With this caveat in mind, none of the RCTs provided evidence that shared family decision making meetings reduced entry/re-entry to care or referrals/re-referrals for maltreatment, increased satisfaction, empowerment or reunification with families, compared to control services.

The broader conclusion from Nurmatov et al. (2020) was of a weak evidence base. Only the USA and the Netherlands had any RCT evidence and, as noted above, those studies mostly were assessed as having a high risk of bias. Within England, evidence on FGCs was based on non-experimental studies by Munro et al. (2017) and Mason et al. (2017). With more than 1,500 families randomised, our trial is the largest to date of FGC and so addresses the concern expressed by de Jong et al. (2015) around small sample size. It also acts as a demonstration of the viability of trialling FGC, another concern of de Jong et al. (2015). As such, it serves to support the position of Creemers et al. (2017) who argue not only that experiments are viable but that they should be the standard to establish the effectiveness of FGC.

The trial reported in this paper was conducted in order to provide high-quality evidence on the effectiveness of FGCs in England. The trial, plus a process evaluation and cost evaluation, was carried out in 21 local authorities which volunteered to join the programme on the basis of not previously offering FGCs during pre-proceedings (although some used FGCs at other stages). The 21 local authorities which 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.

The trial was designed to answer the following primary research question: does referring families for FGCs at pre-proceedings stage, relative to care-as-usual, change the likelihood that children are looked after, 12 months after the pre-proceedings letter?

METHODS

The study was designed as a parallel non-blinded randomised controlled trial with two arms: care-as-usual (the control group) or care-as-usual plus referral for an FGC (the treatment group). Families were randomised on a rolling basis with 1:1 assignment within each local authority. Families remained in their group whether or not they received a FGC (intention-to-treat design). Randomisation was achieved using an independent online platform involving a permuted block design, with blocks of different sizes, to ensure that randomisation outcomes were difficult to guess. Local authorities were asked to randomise families immediately after the Legal Gateway Panel, from September 2020 until the end of the reporting period on 31st May 2022. The protocol for the trial (Taylor et al., 2020) was registered at <https://osf.io/4dxeb/>.

Ethics

This research meets standards outlined in the Coram research ethics policy, which adheres to the widely accepted Economic and Social Research Council's Research Ethics Framework and the Social Research Association's Ethical Guidelines. Following a recommendation from Coram's (anonymised) Research Ethics and Governance Advisory Group, Coram (anonymised) trustees approved the evaluation plan on 22 January 2020. FGCs delivered in the

research are voluntary, and families had the opportunity to opt out of their administrative data being used in the research after being notified with an information sheet sent with the pre-proceedings letter. A full discussion of ethical considerations including consent is reported in Taylor et al, 2020. Families randomised to the treatment group were free to decline the FGC offer and receive instead business-as-usual support.

Data collection

Most data was requested from local authorities via completion of a pre-populated data return in an Excel template, containing one row per child. The primary outcome – looked after status 12 months after the pre-proceedings letter – was obtained in this way, in addition to most secondary outcomes. These secondary outcomes included: whether the child’s living arrangement remained the same or changed, between the date of a court judgement (or date of letter stating the local authority would not pursue proceedings) and a date 6, 12 and 18 months later (sustainment of living arrangements); the amount of time children spent as looked after (versus not looked after) in the 6, 12 and 18 months after the pre-proceedings letter (time spent in care); and the likelihood that court proceedings went ahead by the end of the reporting period (court diversion). The sustainment of living arrangements outcome has been reported separately as an exploratory outcome in this article, as it differs in terms of the follow-up period (from the date of a court judgement or letter stating the local authority would not pursue proceedings) whereas the other outcomes are measured from date at which the pre-proceedings letter is issued.

One secondary outcome – ‘perceived inclusiveness’, which sought parents’ perceptions of how inclusively their local authority worked with them in planning their child or children’s care around eight weeks after randomisation – was instead collected directly from parents. At around 8 weeks post randomisation, the following text message was sent:

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.

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).

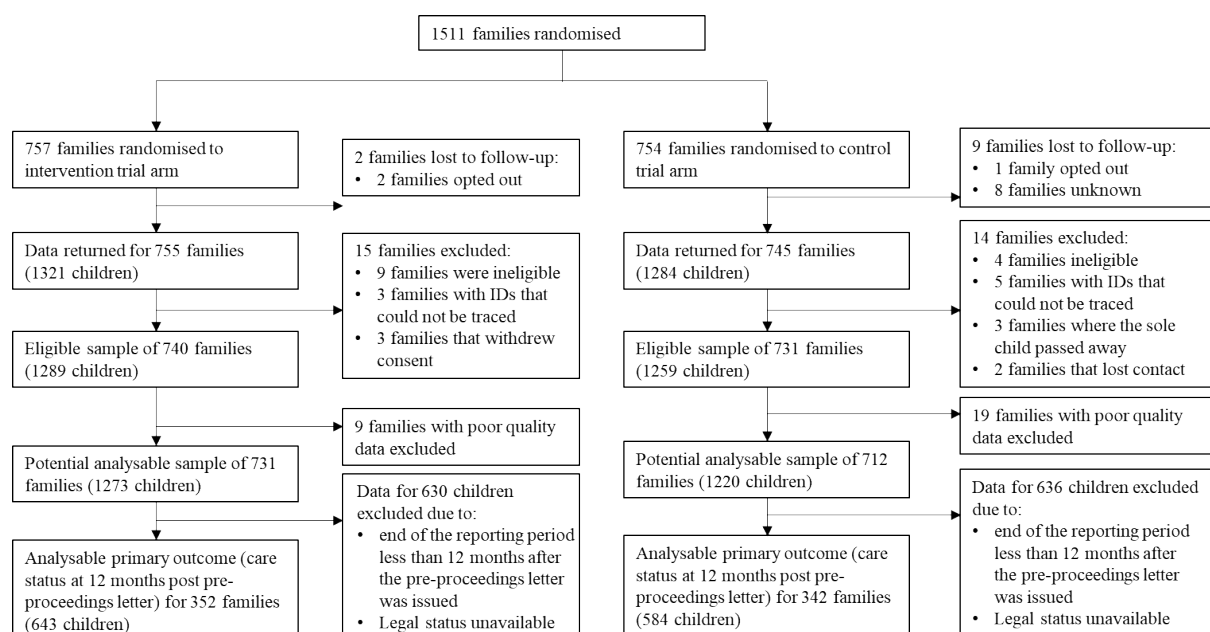
Eligibility

We instructed local authorities to include all families who began pre-proceedings in the trial, including, for example, families without good English, families with children of any age, families with members based abroad, and families where local authorities were seeking any kind or combination of court order(s). Emergency cases where local authorities take immediate action do not enter pre-proceedings and so were not eligible. Data were not available on families' socio-economic backgrounds or reasons for referral.

Data cleaning

Figure 1 shows the participant flow through the study. Overall, 1,511 families were randomised. Local authorities carried out randomisation for an average of 13 months (58 weeks), ranging from 12 to 19 months (50 to 82 weeks). Local authorities returned data for 1,500 (99%) randomised families. Of these, 57 children from 29 families were excluded from the data analysis: 3 families opted out; 13 families were ineligible; 8 family IDs could not be traced; 3 families contained children who passed away over the course of the programme; and contact was lost with 2 families; and one child who had been adopted into another family several years prior. The total eligible sample was 1,471 families, containing 2,548 children.

FIGURE 1. CONSORT DIAGRAM SHOWING PARTICIPANT FLOW THROUGH STUDY BY TRIAL ARM



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 the start was delayed, so the period of intake to the trial was 22% shorter; 14 months rather than the planned 18 months. Finally, fewer local authorities took part (three local authorities dropping out from the original 24). Together these might have been expected to approximately halve the sample, and indeed 1,511 families were randomised compared with an expected 3,300.

Local authority data were cleaned, pooled and then merged with data from Daybreak, which provided further information about the FGCs that were delivered. Where data did not meet the validation criteria listed in Taylor et al. (2023), 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, these were cleaned in accordance with a list of assumptions (see Supplementary Materials).

The majority of trial outcomes were at agreed points in time; 6, 12, and 18-months post-pre-proceedings letter. Since there was a fixed reporting period of 31st May 2022, the delay to starting randomisation (due to Covid) introduced a considerable difference between the number of families randomised into the study and the number with observable outcome data at these time points. For example, in order for children to have observable legal statuses at 12-months post-pre-proceedings letter, they must have been randomised/entered pre-proceedings in the first nine months of the trial (at least 12 months before the end of the reporting period). This reduced the effective sample size but does not bias estimates since it affects intervention and control arms equally.

Of more consequence is the effect on the statistical power of the trial. The target sample size in the protocol of 6,000 children in 3,300 families was predicted to yield a minimum detectable effect size (MDES) of 0.095. However, the final sample size for the primary outcome (N=1,227 children) allowed for an MDES of 0.206. Given these impacts on sample size, this article does not report outcomes at 18-month follow-up, which were considerably smaller than intended across all outcomes (care status n=312, sustainment of outcome n=23, and time spent in care n=401).

Data analysis

The data analysis was carried out using R software (R Core Team, 2018). Descriptive statistics were calculated for the characteristics of the full sample and of those analysed. We checked the characteristics of the intervention and control groups on each variable for balance across intervention and control groups.

Impacts were estimated on an intention to treat (ITT) basis using regression analysis for each of the outcomes; logistic regression for categorical outcomes (care status, court diversion, sustainment of living arrangements) and linear regression for continuous outcomes (time spent in care and perceived inclusiveness). All regressions included local authority fixed effects and reported clustered standard errors to account for the nesting of children within families (see intraclass correlation coefficients reported in Table 1). Variables that were unbalanced across trial arms were also included in all regression models.

Table 1. ICCs for children within families for key outcomes

Outcome at 12 months	Number of children	Number of families	ICC
Care status	1,227	694	0.90
Court diversion	1,251	754	1.00
Sustainment of living arrangement	401	245	0.73
Time spent in care	1,433	810	0.88

The marginal standardisation approach outlined by Naimi and Whitcomb (2020) and their accompanying code was used to calculate adjusted risk ratios. Glass' delta effect sizes were reported for continuous outcomes. Bias-corrected and accelerated (BCa) 95% confidence intervals were obtained using bootstrapping.

Secondary outcomes (except for perceived inclusiveness, which is excluded due to a low text message response rate) were subject to Hochberg multiple testing adjustments.

FINDINGS

Participant characteristics

We analysed data on 2,548 children in 1,471 families in 21 local authorities. 740 families (50.3%; 1,289 children) were allocated to the intervention group and 731 to the control group (49.7%; 1,259 children). The number of children per family ranged from 1 to 8, with a mean of 1.7 children per family (SD=1.2). This is comparable to the general population; the 2018 Annual Population Survey suggests 1.8 children per household in England.

At the end of the reporting period (31st May 2022) the children and young people were aged a mean average of 6.8 years (SD = 5.4), ranging from 0 to 19 years and 8 children (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.

FGC characteristics

FGCs were delivered to 376 families containing 694 children in the eligible sample. The majority (n=351; 93.4%) were delivered to families in the intervention arm. The number of family member attendees is known for 308 FGCs and ranged between 1 and 15 with a mean of 6.0 (SD=2.6), increasing to a mean of 8.3 (SD=3.0; range = 1-19) including professionals. At least one child was reported to attend 34.1% of FGCs. Where known, 41% (n=146) of FGCs were delivered virtually, 40% (n=144) were delivered in-person, and 19% (n=70) were delivered in a hybrid format. Among all family network attendees, 52.8% attended in-person and 47.2% by phone or video call.

We observed a higher number of family network member attendees for FGCs that were delivered in a hybrid format compared to other formats (see Table 2). The trial was not designed to compare the impact of in-person versus hybrid formats, and 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). We also observed that 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. There were some differences in outcomes (see Taylor et al, 2023), which may favour in-person and hybrid formats over virtual FGCs and merit further exploration in future research.

TABLE 2. FGC ATTENDANCE BY DELIVERY TYPE

FGC delivery	Mean number of family network attendees	Standard deviation
In-person	5.6	2.5
Virtual	5.9	2.4
Hybrid	7.3	2.6

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%). In most cases, they were delivered by facilitators who were employees of the local authority (n=269, 71.5%) but some were delivered by workers for independent providers of FGCs (n=85, 22.6%), by employees on zero hours contracts with the local authority (n=3, 0.8%), and in some cases the status of the facilitator was unknown (n=19, 5.1%). Review meetings took place for just under half of the FGCs (n=155, 41.2%) but this may be an underestimate as not all data on FGC characteristics could be matched with cases in our dataset.

Balance checks

Balance checks for each outcome (care status, sustainment of living arrangement, time spent in care, and court diversion) showed that trial arms were equivalent across all baseline characteristics except for care history (whether the child's mother had previously had a child or children taken into care). This variable was therefore included as a predictor in the estimated regressions.

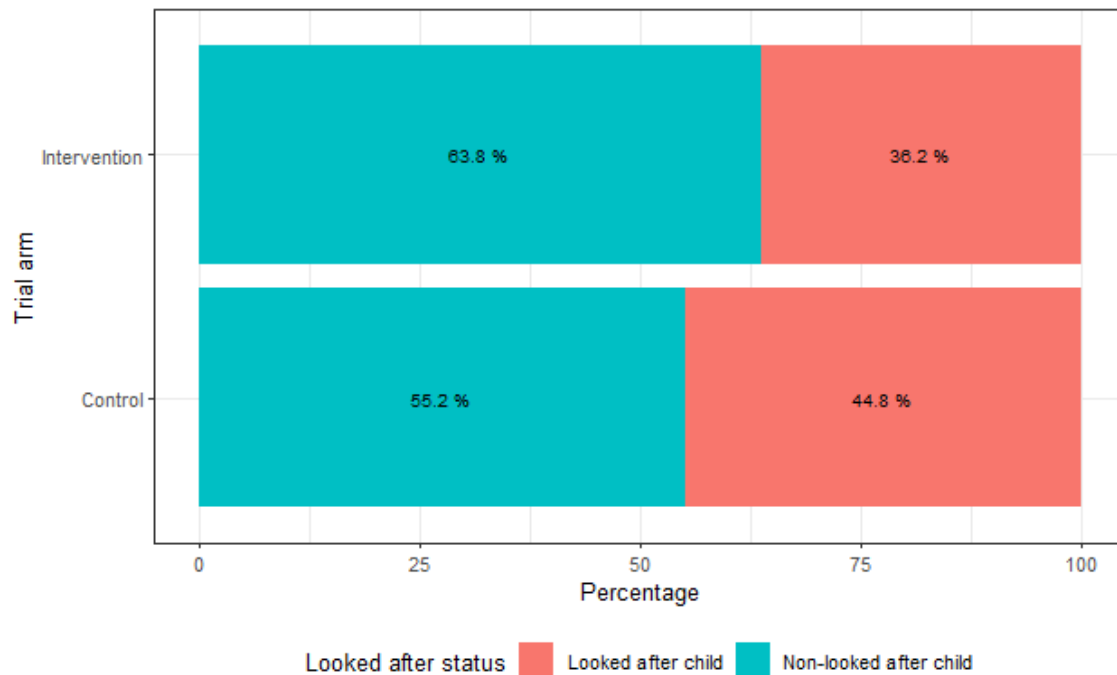
Impact on primary outcome: Care status 12 months post pre-proceedings letter

1,227 children in 694 families were included in this analysis as the pre-proceedings letter was issued at least 12 months before the end of the reporting period (31 May 2022) and a legal status was provided. The logistic regression fitted to this data showed that trial arm was a significant predictor of care status at 12 months ($p=0.03$). The full results of the regression analysis are shown in Supplementary Material 2.

The adjusted risk ratio from this model (taking account of imbalance across trial arms) 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 to children in the control trial arm. According to this adjusted model, 36.2% of children in the intervention arm were looked-after at 12-months compared to 44.8% of children in the control arm, an impact of -8.6 percentage points. This outcome, as well as secondary outcomes at 6 and 12 months are presented in Table 3.

FIGURE 2. ADJUSTED PERCENTAGES OF LOOKED-AFTER AND NON-LOOKED-AFTER CHILDREN AT 12 MONTHS POST-PRE-PROCEEDINGS LETTER BY TRIAL ARM



Impact on secondary outcomes

TABLE 3. IMPACTS ON OUTCOMES INCLUDING HOCHBERG-ADJUSTED P VALUES

Outcomes	N children	Intervention	Control	Estimated impact	P-value	Hochberg- adjusted p- value	Risk ratio / Glass' delta	95% CI
Primary outcome								
Care status at 12 months (looked-after)	1,227	36.2%	44.8%	-8.6 ppts.	0.03	N/A	0.81	0.79-0.83
Secondary outcomes								
Care status at 6 months (looked-after)	2,209	33.1%	42.0%	-8.7 ppts.	<0.01	0.022	0.79	0.78-0.80
Time in care at 6 months	2,398	39 days	49 days	-10 days	0.01	0.056	0.19	0.11-0.28
Time in care at 12 months	1,433	92 days	110 days	-18 days	0.06	0.36	0.20	0.09-0.31
Court diversion (proceedings issued)	1,251	59.2%	71.7%	-12.5 ppts.	<0.01	<0.01	0.82	0.81-0.84
Exploratory outcomes								
Sustainment of outcome at 6 months (change)	835	11.4%	16.5%	-5.1 ppts.	0.12	0.48	0.69	0.64-0.73
Sustainment of outcome at 12 months (change)	401	18.8%	19.1%	-0.2 ppts.	0.97	0.97	1.05	0.92-1.22

Table notes: 18-month outcomes are not reported in this table as the sample sizes were too small e.g. to calculate confidence intervals around risk ratios, and confidence intervals crossing 0.

Risk ratio for binary outcomes, Glass' delta for continuous outcome (time spent in care).

Differences between intervention and control proportions, and estimated impacts are due to rounding.

In this article, sustainment of outcome is treated as an exploratory outcome as it was observed over a different follow-up period (from the date of a court judgement or date of letter stating the local authority would not pursue proceedings) than all other reported outcomes (from the date of the pre-proceedings letter).

The >1 risk ratio and the negative estimated impact is likely due to the small sample size and adjusting for local authority and care history. The risk ratio for the unadjusted model is 0.91.

Care status at 6 months post pre-proceedings letter

Trial arm was a significant predictor of care status at 6 months when adjusted for multiple tests (adjusted $p=0.02$, $n=2,209$), with children in the intervention trial arm 8.7 percentage points less likely to be looked after at 6 months post-pre-proceedings letter compared to children in the control arm.

Time spent in care at 6 and 12 months post pre-proceedings letter

The 6-month analysis was based on the 2,398 children who had a pre-proceedings letter issued by 30 November 2021 (6 months before the end of the reporting period). Over the six months since randomisation, those in the treatment group spent on average 10 fewer days in care than those in the control group (adjusted $p=0.056$). This is an effect size of 0.19 (0.11-0.28).

We had sufficient data for analysis of time spent in care between pre-proceedings letter and a date 12 months later for 1,433 children, comprising 56% of the total eligible sample. 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%). The regression results showed children randomised to the intervention arm to spend 18 fewer days in care than those in the control arm, although this was not statistically significant at conventional levels ($p=0.36$).

Sustainment of living arrangements at 6 and 12 months post pre-proceedings letter

We had information about the living arrangement of 1,173 children at the date of either the step-down letter (letter informing families that care proceedings will not be issued) or the date when care proceedings were issued. This reduced to $n=401$ children by 12 months after this date, in 245 families in 20 local authorities. A logistic regression was fitted to the data. Possibly due to the small size of the sample, trial arm was found not to be a significant predictor of sustainment of living arrangements at 12 months (adjusted $p=0.97$).

Trial arm also did not significantly predict sustainment of living arrangement at 6 months (adjusted $p=0.48$; $n=835$).

Court diversion by the end of the reporting period

We had sufficient data for analysis of court diversion for 1,251 children (in 754 families), comprising 49% of the total eligible sample. A logistic regression was fitted to the data and

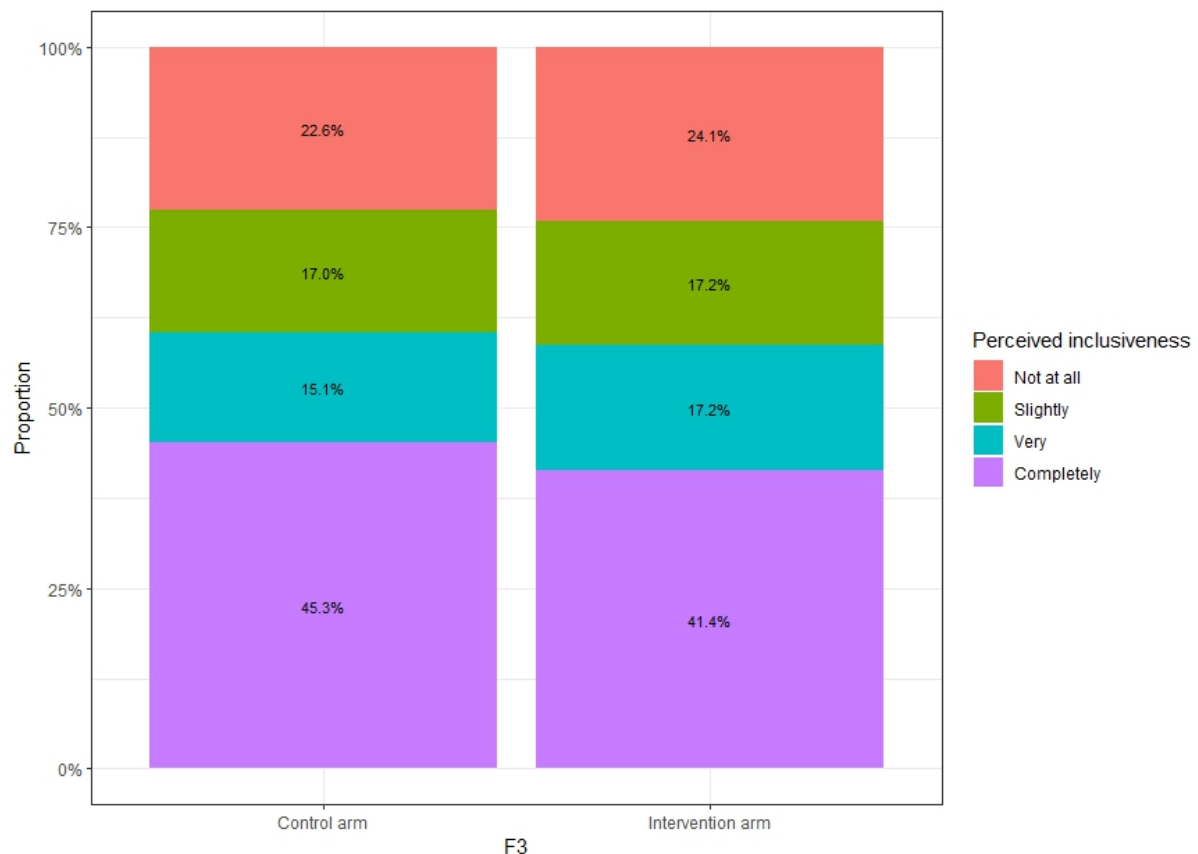
showed that trial arm was a significant predictor of court diversion (adjusted $p < 0.01$). The adjusted risk ratio was 0.82 (95% CI 0.81-0.84), meaning children in the intervention trial arm were 0.82 times less likely to have proceedings issued compared to children in the control trial arm. The adjusted difference in the proportion of children with proceedings issued or stepped down was 12.5 percentage points.

Perceived inclusiveness

We were able to match the text messages sent to the main dataset of eligible participants for 889 families (or 1,580 children), to explore differences in the characteristics between families who responded to texts 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. Children whose parents responded to the text were more likely to have a mother who had previously had a child or children removed from her care ($p < 0.01$).

The response rate to texts was too low for a reliable breakdown across the intervention and control groups. In our trial protocol we specified that we would not include perceived inclusiveness as an outcome if the overall response rate was less than 50% (Taylor et al., 2023). We have therefore summarised our findings descriptively but have not reported any significance testing. We received analysable 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. The majority of parents in both trial arms (59% overall) felt either 'completely' or 'very' involved in the planning of their child(rens)'s care.

FIGURE 3. RESPONSES TO TEXT MESSAGE BY TRIAL ARM AT FAMILY LEVEL



Compliance analysis

The ITT results capture the impact of being in the treatment arm. This is distinct from the impact of FGC itself since many families in the treatment arm did not receive an FGC. On the other hand, some (a much smaller proportion) of those in the control arm did receive an FGC. If we assume that the estimated impact of being in the treatment arm arises purely from FGC participation, we can use compliance analysis to estimate the impact of FGC participation itself.

Of those families randomised to the intervention trial arm, 522 (71%) were referred to the FGC service and 351 (67% of those referred) went on to receive an FGC. Slightly more than half (389, 53%) of the families who were randomised to the intervention trial arm did not receive an FGC. Most often, this was because the family declined or refused the offer (n=226, 58%). Among families in the control arm, 25 (3%) received an FGC. The reason for this is unknown or unclear in 12 cases (48%). For 9 families (36%), local authorities explained that a referral was made despite randomisation to the control condition. In 4 cases (16%), the family requested an FGC.

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 of FGC participation (see Table 4). For our primary outcome, the probability of becoming looked after was estimated to be 16 percentage points lower for compliers ($p < 0.01$). By comparison, the ITT was a reduction of 9 percentage points. For the probability of care proceedings being issued, the CACE was a reduction of 28 percentage points ($p < 0.01$), compared to the ITT reduction of 13 percentage points. For time spent in care, the CACE suggested a reduction of 36 days ($p = 0.07$) compared to the ITT reduction of 18 days. For sustainment of living arrangements, the CACE was not statistically significant ($p = 0.96$), consistent with the ITT.

TABLE 4. CACE ANALYSIS FOR KEY OUTCOMES

Outcome	CACE coefficient	CACE p value	Number of observations
Care status at 12 months	-0.16	<0.01	1,227
Sustainment of living arrangements at 12 months	0.01	0.96	401
Time spent in care at 12 months	-36.45	0.07	1,433
Court diversion by end of reporting period	-0.28	<0.01	1,251

DISCUSSION

FGCs are non-statutory and only one of the interventions and approaches local authorities can use during pre-proceedings. This study builds on the previous evidence base on the use of FGCs in England and internationally. Munro et al., 2017 evaluated pre-proceedings stage FGCs in Southwark and Wiltshire by the charity Daybreak and found promising findings on outcomes, but did not have a robust comparison group. This study provide the most robust yet evidence in favour of the continued use of FGCs at pre-proceedings stage for families in England. The systematic review of studies comparing family group meetings to control group services by Nurmatov et al. (2020) found an evidence

base of poor quality, with few robust comparison groups, but some positive, moderate, and mixed findings, for example in the largest previous FGC RCT cited, by Perry et al. (2013) in the USA, with 677 families involved. This study is distinctive in that the systematic review did not find a reduction in entry to care internationally.

Our findings imply that local authorities should continue the use of FGCs at pre-proceedings stage where services already exist; and consider their introduction in other areas. The main relevance of the study is to commissioning decisions, not decision making in individual families. 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.

Becoming looked-after does not necessarily mean a child is happier or safer, but it is an important status in itself, and an important influence on many other child outcomes. Being looked-after is also expensive, and local authorities are obliged to seek value for money. Analysis of the costs and benefits to local authorities finds FGCs, used in this way, are likely to be cost effective (Taylor et al., 2023). The estimated average net benefit per child to a local authority in 2021 prices by referring the family of a child entering pre-proceedings for an FGC was £960 (£6,202 benefits minus £5,242 costs) per child in the first year, giving a benefit–cost ratio of 1.18:1. There may be other financial benefits to public services such as courts.

Like so many evaluations of programmes introduced since March 2020, it is difficult to say how generalisable our findings are to practice in general. 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 being a rare occurrence, as expected. FGCs are intended to be distinct 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 character and may look and feel more similar to business as usual. So larger effects sizes might be expected with entirely in-person meetings.

Some in the sector expressed initial concerns about randomising half of families not to be offered an FGC. In fact, such 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 upon them: FGCs are voluntary. These features reassured those with doubts about RCTs, an evaluation design in widespread use in education and other sectors but less common in children's services. Local authorities generally abided by the trial protocol.

This study demonstrated the feasibility of further RCTs in the sector. We obtained administrative data from all 21 local authorities on almost all randomised families. But, despite the clear definition and importance of looked-after status, going to court, and who a child lives with, the data we received was often difficult for officials to extract, and much of what we received was patchy and incomplete. Although speculative, the small number of children available for analysis may help explain our finding of no difference in whether or not living arrangements changed. These data issues would prove a major limitation to any study other than a RCT where any noise and error is likely to have been equal across both trial arms.

Limitations

Covid-19 brought a period of rapid change and learning for local authorities as they delivered FGC via hybrid and remote methods. While these 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, complicating interpretation of these findings. The intention-to-treat design meant we included a mixture of FGC types in our analysis, which may have had different impacts or impacts of different sizes. In this regard local authorities adopted different practices, reducing the consistency of the model evaluated. However, there was generally high fidelity and high adherence to good standards of practice (where known, the FGC facilitator was almost always trained to the standards set by Daybreak (n=339, 99%). While the study is limited in that it could not explore how effectiveness varies with quality or nature (in-person vs hybrid) of FGC implementation, its strength is in capturing the impact of real-world FGC delivery.

Covid also slowed court proceedings, complicating our interpretation of child outcomes. For example, in the administrative data from local authorities, we saw around 10 times more

Interim Care Orders than Care Orders. We saw a snapshot of child outcomes at a moment in time, during an on-going process, rather than the final picture for many cases.

The challenges presented by running a trial during Covid also impacted the sample size, as outlined above. The delayed start to the trial meant that 18-month outcomes were only available for small numbers of participants (those randomised early in the trial) and could not be relied upon as robust estimates of longer-term impact. Further research is needed to understand the extent to which the impacts observed in this trial are sustained.

There were instances of non-compliance with the trial protocol, such as social workers who did not refer intervention group families for an FGC, or who referred control group families to the FGC service. The latter were relatively few in number; only 6% of control group families received an FGC.

Conclusion

The findings of this trial, the first RCT of FGCs at pre-proceedings in the UK, provide evidence in favour of Family Group Conferences as a way to reduce the chances of children going into care. Among children in families entering pre-proceedings, 36.2% of those referred for FGCs were in care 12 months later compared to 44.8% of those not referred. This difference was apparent by the 6-month point. Being referred for FGC also appeared to make care proceedings less likely (59% compared with 72%). However, there was no apparent effect on stability of a child's living arrangement, although this may be due to the reduced sample size for this outcome. Future research should seek to confirm these findings, follow up outcomes for longer, and explore differences between groups of families and types of FGC (in-person, virtual and hybrid)..

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