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Long-term outcomes of Sweden's Contact Family Program for children

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ABSTRACT

Objectives: To assess the long-term impacts of Sweden's Contact Family Program (CFP) for children on participants' future outcome profiles, here conceptualized as combinations of outcomes related to mental health problems, public welfare receipt, illicit drug use, placement in out-of-home care, educational achievement, and offending.
Methods: We analyzed longitudinal register data on more than 950,000 children born 1980–90, including 6693 children who entered CFP at 2–5 years of age, with a follow-up until 2008. Children's outcome profiles were identified by latent class analysis. The average program impact was estimated by means of propensity score matching.
Results: Long-term outcomes for those who had received the intervention were not better than for matched peers who did not receive the intervention. Simulation-based sensitivity analyses indicate that some of our estimated negative treatment effects may be affected by unobserved factors related to program participation and outcomes. However, both selection and outcome effects must be extremely strong in order to generate notable positive effects of CFP participation.
Conclusions: The results did not find support for CFP effectiveness in reducing risks of compromised long-term development in children. Since the intervention reaches a high-risk group of children and is popular among users, volunteer families and professionals, the program should be reinforced with knowledge-based components that target known risk factors for child welfare recipients.

Introduction

A number of social interventions which put emphasis on role modeling and the importance of extra-familial adults have been advanced to improve the lives of disadvantaged children and youth (Hamilton & Hamilton, 2004). Sweden's Contact Family Program (CFP) is an example of such a program. The CFP has existed and been mandated in national child welfare legislation since 1982. Volunteer families are commissioned by child welfare authorities to provide respite care and informal social support to children (primarily with single mothers) who have a stressful and/or adverse social situation. The CFP has much, but not all, in common with respite or relief care programs in the UK (Triseliotis, Sellick, & Short, 1995), youth mentoring programs in the US (Rhodes & DuBois, 2008), and the Aunties and Uncles Co-operative Family Program in Australia (Wilkes, Beale, & Cole, 2006). CFP is much used by local authorities: roughly 4% of all Swedish children will at age 18 have experience of a contact family (Vinnerljung & Franzén, 2005).

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The CFP has substantial preventive aims, mainly to prevent placement in out-of-home care and deteriorating development for children in adverse family environments (Andersson, 1993). A host of small scale studies have affirmed that the program is popular, both among users, social workers and volunteers. Most users do not view the program as an instrument for control by child welfare authorities. A long row of qualitative small-sample studies have affirmed that there are large individual variations in the scope and nature of support given by contact families. Since national guidelines do not exist, and local guidelines tend to be sketchy or (mostly) non-existing, these results are not surprising. The CFP can in practice be anything from having a child staying overnight in the volunteer family one weekend/month, to formation of strong bonds between the child/mother and the volunteer family, resulting in the latter assuming supportive roles during the child's formative years resembling those of close relatives (Andersson & Bangura Arvidsson, 2001).

Generally, both national and international scholars have written favorably about the CFP (Andersson, 1993; Barth, 1991; Gould, 1988). But the program has never been evaluated, even if a wide definition of evaluation is used. Partly this is caused by the intervention being legally mandated (parents can apply for and have a formal right to receive the intervention). For legal reasons it is practically impossible to use a randomized design. Constructing relevant comparison groups for quasi-experimental studies is equally difficult, particularly since the intervention is delivered by local authorities in 290 municipalities, each with a high degree of financial and legal independence from the national government level.

However, we do know from national cohort studies that children who receive this intervention belong to a high-risk group for future adverse outcomes (e.g. suicidal behavior, illicit drug use, criminality and poor educational achievement) in late adolescence and young adult years (Hjern, Vinnerljung, & Lindblad, 2004; Vinnerljung, Berlin, & Hjern, 2010; Vinnerljung, Franzén, & Danielsson, 2007; Vinnerljung, Hjern, & Lindblad, 2006; Vinnerljung, Öman, & Gunnarson, 2005). Excess risks for CFP children basically match those of youth from long-term foster care (Vinnerljung, Franzén, Hjern, & Lindblad, 2010). In addition, one national register study reported considerably elevated risks for post intervention placement in out-of-home care, in comparison with children of mothers who had indications of addiction or serious mental health problems (high risk groups for out-of-home care; Franzén, Vinnerljung, & Hjern, 2008), but whose children did not receive the CFP-intervention (Vinnerljung & Franzén, 2005).

While evaluations of youth mentoring programs indicate positive impacts on participants' development in the short-term (Eby, Allen, Evans, Ng, & DuBois, 2008; Tolan, Henry, Schoeny, & Bass, 2008) we essentially do not know anything about the effects the Swedish CFP. Thus, after 30 years an evaluation is long overdue.

The objective of this study is to assess long-term impacts of CFP on participants' future outcome profiles, here conceptualized as combinations of outcomes related to mental health problems, welfare receipt, illicit drug use, placement in out-of-home care, educational achievement and offending. By using extensive longitudinal register data for more than 950,000 young Swedes, our analysis offers several innovative contributions over the existing research into the outcomes of social interventions aimed at improving young disadvantaged people's growth and development. Firstly, we address the long-term results on participants' outcome profiles, rather than on a variety of outcomes analyzed in isolation. This person-oriented approach (Bergman, Magnusson, & El-Khouri, 2003) seems fruitful since it is reasonable to expect that several of the addressed outcomes tend to go hand in hand. Secondly, we estimate program effects within a counterfactual approach based on matching on propensity scores. This approach reduces well-known biases related to comparing people where there does not exist a sound basis for comparison. Lastly, the analyses are based on a specified model for program assignment since the data allow for a rigorous control for background factors related to the social circumstances of the children's parents.

Data and methods

This study uses comprehensive longitudinal register data. Sweden has a long tradition of national registers with high-quality data for health and socio-economic indicators, and for child welfare interventions. These registers are based on the individually unique 10-digit personal identification number (PIN) that follows every Swedish resident from birth (or time of immigration) to death. Different registers can be linked through the PIN-number. Also, members of the same birth family can be identified and linked through the Multi-Generation Register administered by Statistics Sweden. Our study utilizes data from several national registers, administrated by Statistics Sweden, the National Board of Health and Welfare, the National Agency for Education, and the National Council for Crime. The study was approved by the regional ethics committee in Stockholm.

Population and intervention

Our population consists of all children born in Sweden 1980–1990, recorded in the Medical Birth Register, who were alive at age 16. We excluded immigrant children (born outside Sweden) since we wanted to avoid well known links between language difficulties and educational achievement (one of our outcome measures). Immigrant children are also underrepresented among those that receive a Contact Family in early age (Andersson & Bangura Arvidsson, 2001). Furthermore, we excluded children with a record of emigration after birth, and all children who according to the Longitudinal Integration Database for Health Insurance and Social Studies (LISA-register) were receiving a disability pension at age 23. This is a strong indicator of lasting somatic or mental impairment that may have been present to some degree in early age, and may actually have been a cause for the intervention (even though normal procedure is that such support is administered by the health authorities). After these delimitations the effective population size was 954,848 children.

The treatment group consisted of all children born 1980–1990 that started a Contact Family intervention at age 2–5, but were not placed in out-of-home care at any time during those years ($n = 6693$; 0.7% of the population). The construction of the treatment group was dependent on two restrictions. Firstly, the intervention became a part of the legal framework and national individual based statistics first in 1982 so the 1980 birth cohort is the first one with interventions starting at age two that can be studied. Secondly, the latest follow-up data we had access to were from 2008. We set age 18 as a minimum age for inclusion in the follow-up. Subsequently, the birth year cohort born 1990 is the last one that could be included. The comparison group is drawn from the remaining part of the population ($n = 948{,}155$). All children are followed in the National registers from age 6 to 2008, in one register to 2009. Age at last year of follow-up is subsequently age 18/19–28/29. Follow-up time thus varied between 12 and 23 years.

The national register based information on the CFP is basically limited to duration. There is no information on the cause, content or intensity of the intervention, or any data describing the child or mother. Also, there is no information that enables us to identify the volunteer families. The mean and median duration of CFP participation in the treatment group was 949.32 (SD = 800.00) and 718 (p25 = 364, p75 = 1331) days respectively. This means that the median length was around 2 years and that 75% of the CFP children participated for at least 1 year. If the median program child lived with the contact family every second weekend, and had a longer stay during summer/winter holidays, this would suggest a median intensity of around 50 occasions.

Dependent variable: outcome profiles

A hallmark of the person-oriented approach is that variables in and of themselves have limited meaning. When we assume that the relationships among our addressed variables are not uniform across all the values that the variables may take, we can develop outcome profiles that describe individuals, not scores on the variables (Bergman & Trost, 2006; Bogat, Levendosky, & von Eye, 2005). We began by constructing six binary outcome variables from the available register data, all reflecting key adverse outcomes in a variety of important life areas.

Poor mental health (h). Indication of poor mental health was defined as having collected any prescribed psychotropic drugs in 2009 (neuroleptics: ATC-code NO5A; sleeping pills: NO5C; anxiety reducing pharmaceuticals: NO5B; anti-depressants: NO6A), according to the National Pharmacological Register.

Illicit drug use (D). A hospitalization with a drug abuse diagnosis or a conviction for a drug related offence after age 16 was considered an indication of illicit drug use. The outcome is based on a combination of information from the Hospital Discharge Register and the Register of Criminal Offences.

Extensive welfare recipiency (W). If more than 50% of disposable income at age 21 consisted of means-tested public welfare, this was considered as an indication of extensive welfare recipiency. Data were retrieved from the LISA-register.

Placement in out-of-home care (P). Placement in foster family or residential care at age 13–18, according to the Child Welfare Register.

Poor educational achievement (e). No grades (usually due to high rates of absconding), incomplete grades, or very low grades at age 15–16 are viewed as an indication of poor educational achievement. Incomplete grades were defined as having a grade missing in one of the core subjects (according to school legislation): Swedish, English or mathematics. Very low mean grades was defined as a mean average grade < (mean − 1 standard deviation), in other words belonging to the 1/6 in her/his peer group with the lowest school performance in the country. Data were retrieved from the National School Register.

Serious criminality (C). Serious criminality (C) was defined as having been sentenced to probation, prison or forensic psychiatric care (as opposed to fines, community service or a suspended sentence) according to the Register of Criminal Offences. All these sanctions are strong indications of either serious crimes or a criminal career in a young population as ours.

Drawing on model-based methods related to the person-oriented approach (Bergman et al., 2003), we applied categorical latent class analysis (LCA) using Latent GOLD 4.5 (Statistical Innovations Inc., Belmont, MA) to reduce the number of combinations (six binary variables yield $2^6 = 64$ possible alternatives) and thereby identifying relevant groupings of outcomes in our data that describes individuals rather than variables. A number of strategies are available when we are interested in determining the number of classes (Bauer, 2007; Henson, Reise, & Kim, 2007). Since we have a very large sample size ($N > 950{,}000$), which makes *P*-value-based significance testing less informative (Rothman, Greenland, & Lash, 2008), we have to seek alternative ways for determining the number of classes. Here, we utilize the Bayesian Information Criterion (BIC) rather than various *P*-value-based likelihood ratio tests as our primary tool for deciding on the number of classes. BIC weights both model fit and parsimony and tends to favor simpler models more than *P*-values do in very large datasets. Thereby it reduces the risk of over-fitting (Raftery, 1995). Moreover, the performance of the BIC in categorical LCA with unequal class sizes (which is reasonable to expect here as many of our addressed outcomes are rare events) increases as the sample size increases (Nylund, Asparouhov, & Muthén, 2007). However, we also looked at the reduction in L^2, bivariate residuals, and

Table 1
Latent class analysis: model fit statistics. N = 954,848.

Model/number of classes	BIC	L^2	Reduction in L^2	Uncorrelated bivariate residuals?	Classification error
1	2044273.67	154264.28	0.00%	No	0.00%
2	1901191.28	11085.50	92.81%	No	3.33%
3	1893472.22	3270.06	97.98%	No	4.52%
4	1891498.44	1199.89	99.22%	No	6.80%
5	1890916.27	521.33	99.66%	No	6.48%
6	1890728.25	236.93	99.85%	No	8.95%
7[a]	1890635.26	47.55	99.97%	Yes	8.82%
8	1890714.81	30.72	99.98%	Yes	10.28%

BIC = Bayesian Information Criterion.
[a] The model with the smallest BIC and uncorrelated bivariate residuals is chosen as the best model.

the classification error rate to determine the best-fitting model (Magidson & Vermunt, 2001, 2004; Vermunt & Magidson, 2005).

The BIC suggests that a seven-class solution is a valid representation of groupings of outcomes in the data (Table 1, Model 7). The L^2 value indicates the maximum association that can be explained by any latent class model. Model 1 is thus a baseline-model against which the fit of alternative models can be judged. The reduction in L^2 for Model 7 supports that the BIC has identified a valid model as the seven-class solution represents a massive improvement. The bivariate residuals (BVR) assesses the extent to which the two-way associations between any pair of indicators are explained by the model. Analyses of the BVR for Models 4–6 show that these models were short in reproducing the associations between the indicators. This means that we need a more complex model to achieve a better fit. Based on the BIC and the fact that the BVR for Model 7 were not correlated, the seven-class solution is judged to represent data adequately. As the misclassification error rate for Models 4–8 is low (<10%) and virtually identical, it becomes of minimal concern for this study.

Fig. 1 shows the identified classes along with the conditional probabilities for each of the nominal outcome variables. The classes/profiles were labeled according to the levels of the conditional probabilities, and cases were assigned to classes using the modal assignment rule (Vermunt & Magidson, 2005). Around 81% (n = 775,342) of the individuals are found in a class termed No adverse outcomes. The conditional probabilities for the outcome variables are more or less zero (not shown in figure). As shown in Fig. 1, one class represents individuals who mainly had problems related to poor educational achievement (e). The size of this class is around 9% (n = 82,006). Another class characterizes people who first and foremost had problems related to poor mental health (h), and represents approximately 6% of the sample (n = 59,411). An additional class identified people who mainly had problems related to extensive welfare recipiency, placement in out-of-home care, and poor educational achievement (WPe). This class represents around 3% of the individuals (n = 26,678).

A major advantage with our massive sample size is that it becomes feasible to identify relatively small high-risk subgroups of the population. Thus, the remaining three latent classes identified more problem-burdened individuals. Around

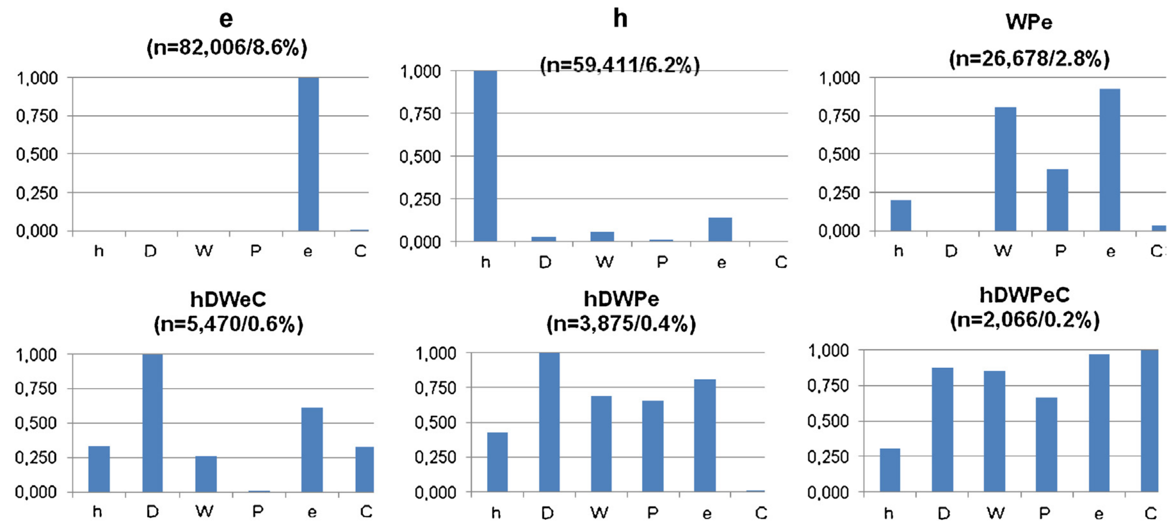

Fig. 1. Dependent variable: outcome profiles (no. of cases/relative class size within brackets).
Bars in each graph show the conditional probabilities for the six indicator variables across classes. Labeling of classes are based on the level of the conditional probabilities. e = poor educational achievement; h = poor mental health; WPe = extensive welfare recipiency/placement in out-of-home care/poor educational achievement; hDWeC = poor mental health/illicit drug use/extensive welfare recipiency/poor educational achievement/serious criminality; hDWPe = poor mental health/illicit drug use/extensive welfare recipiency/placement in out-of-home care/poor educational achievement; hDWPeC = poor mental health/illicit drug use/extensive welfare recipiency/placement in out-of-home care/poor educational achievement/serious criminality.

0.6% ($n = 5470$) of the sample had problems related to poor mental health, illicit drug use, extensive welfare recipiency, poor educational achievement, and serious criminality (hDWeC). Another class discerns people who had problems related to poor mental health, illicit drug use, placement in out-of-home care, extensive welfare recipiency, and poor educational achievement (hDPWe). This class represents around 0.4% ($n = 3875$) of the sample. The final class is an extended version of the previous ones since it also includes persons who had indications of placement in out-of-home care and serious criminality (hDPWeC). In absolute numbers, this last class represents around 2000 people which correspond to approximately 0.2% of the sample.

Estimating the average program impact

Since the assignment to CFP is not random, we applied propensity score matching (PSM) to find a suitable control group of non-participants (Guo & Fraser, 2009). PSM constructs a statistical comparison group that is based on a model of the probability of program participation using observed characteristics. Participants are then matched on the basis of this probability (or propensity score) to non-participants. The average treatment effect on the treated (ATT) is then calculated as the mean difference in outcomes across these groups (Deheija & Wahba, 2002). The propensity scores are not known but have to be estimated by some standard probability model. Here we used a binary logit regression model which included observed covariates that jointly affect program assignment and outcomes (e.g. parental circumstances related to educational attainment, civil status, mental health, substance use and criminality, see Table 2). All PSM-analyses were performed using the 'psmatch2' module (Leuven & Sianesi, 2003) in Stata 12/MP-version (StataCorp LP, College Station, TX).

Most observed covariates indicating parental conditions are based on data from when population members were 17 years of age, and thereby violating the assumption of using pre-treatment characteristics in the program assignment equation (Caliendo & Kopeinig, 2008). However, the utilized covariates described in Table 2 may be deemed as sufficient proxies for pre-treatment parental circumstances. Regarding parental educational attainment, for example, we know that having a child lowers educational participation (Henz, 2001). This implies that parenthood is negatively associated with further educational enrolment. Indications of parental substance abuse, mental health problems and criminality are collected from the entire observation period, from the birth of the individual child to 2008. Mental health problems and substance abuse often result in hospitalizations several years after the condition is manifested. The standard procedure in Swedish health care services to persons with mental health or addiction problems is out-patient treatment. In other words, it is likely that the register indications also tell us something about the environmental conditions in the birth home. This way of reasoning is also valid for criminality where probation or prison in the Swedish court system often follows after a long line of less repressive sentences. We are aware of casting a wide net with an extended observation time for the variables related to parental psychopathology and thereby being short on precision. These variables also constitute crude indications of possible genetically related risk factors (Cloninger, Sigvardsson, Bohman, & von Knorring, 1982; Kendler et al., 2012; Sigvardsson, Bohman, & Cloninger, 1996). This is also the reason why we used indications for both the mother and the father in these variables.

Regrettably, available register data do not include information about reasons for CFP participation. However, earlier research has shown that causes for program participation in this age group (2–5 years) are almost exclusively parent related (Andersson & Bangura Arvidsson, 2001). It would of course have been preferable to have robust information about child related risk factors that most likely have affected long-term developmental outcomes, e.g. early experience of abuse and neglect. We also lack information about the voluntary families, which limits the precision of our analyses.

It seems safe to assume that program participation in itself could not affect the utilized covariates. Given the nature and frequency of the intervention, it makes little or no sense that children's participation in the CFP should influence, for example, their parents' civil status or substance abuse.

There are several different methods of matching individuals in the treatment group with individuals in the comparison group. The four most widely used methods are 'Nearest neighbor matching', 'Radius matching', 'Kernel matching' and 'Stratification matching' (Becker & Ichino, 2002). On the one hand, Nearest neighbor and Radius generate the best quality matches, because they search for appropriate matches in a more restricted area of propensity scores. On the other hand, Kernel and Stratification produce the best quantity matches, because they use more control units. To ensure that the results were not sensitive to the choice of matching algorithm, all methods were applied. The overall results were robust regardless of the method used. Therefore, we report the results from Nearest neighbor (one-to-one) matching.

Underlying assumptions and conditions

As shown in Table 2, CFP-children constitute a highly selected group. Compared to their unmatched peers, their parents were (among other things) far more likely to be single, have a lower level of education, be out of work, have disability pension, to live on public welfare and have indications of mental health problems, illicit drug use, and serious criminality. However, compared to their matched peers, these differences were virtually zero. This means that our PSM-analysis reported below has constructed a valid control group and that the balancing property is sufficiently satisfied.

The validity of PSM also rests on other assumptions. A key one is that of conditional independence (CIA), meaning that no selection on unobservables will bias our estimated impact of CFP participation and outcomes. After having reported ATT estimates, we will explore this assumption by systematically examining how our results may change as this assumption is

Table 2
Descriptive statistics. $N = 954{,}848$. CFP, $n = 6693$; unmatched controls, $n = 948{,}155$; matched controls (one-to-one matching), $n = 6693$.

Covariate	Definition	Range	Effect on program participation, OR (95% CI)	Sample	Mean CFP	Mean control
Child characteristics						
Sex[a]	Boy	0–1	1.04 (0.99–1.09)	Unmatched	0.524	0.516
				Matched	0.524	0.522
Birth year[a]	Year of birth	1980–1990	1.08 (1.07–1.09)	Unmatched	1985.9	1985.2
				Matched	1985.9	1985.9
Parental/household characteristics						
Employment[b]	Mother employed when child was age 17	0–1	0.59 (0.56–0.63)	Unmatched	0.563	0.861
				Matched	0.563	0.558
Poverty[b]	Public welfare >50% of mother's income when child was age 17	0–1	1.76 (1.63–1.91)	Unmatched	0.171	0.027
				Matched	0.171	0.163
Teenage parent[c]	Mother teenager at the birth of her first child	0–1	1.17 (1.07–1.28)	Unmatched	0.095	0.032
				Matched	0.095	0.090
Domicile[d]	Town (at age 17)	0–1	1.06 (1.00–1.11)	Unmatched	0.429	0.430
				Matched	0.429	0.431
	Rural (at age 17)	0–1	1.10 (1.02–1.18)	Unmatched	0.178	0.184
				Matched	0.178	0.172
Country of birth[d]	Nordic country, mother	0–1	1.23 (1.11–1.35)	Unmatched	0.073	0.043
				Matched	0.073	0.073
	Other European country, mother	0–1	0.69 (0.60–0.80)	Unmatched	0.030	0.032
				Matched	0.030	0.030
	Non-European country, mother	0–1	1.22 (1.07–1.39)	Unmatched	0.041	0.025
				Matched	0.041	0.044
Civil status[b]	Single mother when child was age 17	0–1	7.04 (6.68–7.43)	Unmatched	0.585	0.103
				Matched	0.585	0.592
Educational attainment[b]	Mother Secondary education when child was age 17	0–1	0.92 (0.87–0.98)	Unmatched	0.560	0.510
				Matched	0.560	0.558
	Mother Post-secondary education when child was age 17	0–1	0.67 (0.61–0.72)	Unmatched	0.163	0.340
				Matched	0.163	0.158
Illicit drug use[e,f]	Substance abuse, mother[g]	0–1	1.22 (1.11–1.34)	Unmatched	0.150	0.024
				Matched	0.150	0.134
	Substance abuse, father[g]	0–1	1.36 (1.27–1.46)	Unmatched	0.412	0.113
				Matched	0.412	0.419
Health	Mother had Disability pension when child was age 17	0–1	1.83 (1.71–1.96)	Unmatched	0.283	0.085
				Matched	0.283	0.287
Mental health[e]	Poor mental health, mother[h]	0–1	1.59 (1.48–1.71)	Unmatched	0.183	0.041
				Matched	0.183	0.168
	Poor mental health, father[h]	0–1	1.19 (1.08–1.32)	Unmatched	0.078	0.022
				Matched	0.078	0.069
Criminality[f]	Serious criminality, mother[i]	0–1	1.38 (1.24–1.53)	Unmatched	0.104	0.013
				Matched	0.104	0.090
	Serious criminality, father[i]	0–1	2.43 (2.27–2.61)	Unmatched	0.436	0.092
				Matched	0.436	0.436

[a] Medical Birth Register.
[b] LISA-register.
[c] Multi-Generation Register.
[d] The Total Population Register.
[e] Hospital Discharge Register.
[f] Register of Criminal Offences.
[g] At least one hospitalization with a substance abuse diagnosis according to standardized ICD-codes or at least one conviction related to substance abuse.
[h] At least one hospitalization with a psychiatric diagnosis according to standardized ICD-codes.
[i] At least one conviction that resulted in a sentence to probation, prison, or forensic psychiatric care (as opposed to fines, community service or a suspended sentence).

weakened in specific ways. More specifically, we apply a simulation-based sensitivity analysis proposed by Ichino, Mealli, and Nannicini (2008) in which we derive point estimates of the ATT under different possible scenarios of deviation from the CIA. This analysis is organized as follows. We start by examining the effects of "calibrated" confounders, i.e. confounders which are similar to the empirical distribution of important binary covariates. This simulation exercise reveals the extent to which the ATT estimates are robust to deviations from the CIA induced by the impossibility of observing factors similar to

Table 3
Long-term outcomes of CFP participation on outcome profiles.

Profile	Sample	Mean CFP	Mean controls	Risk difference (95% CI)	Risk ratio (95% CI)
No adverse outcome	Unmatched (crude)	0.520	0.814	−0.294 (−0.304 to −0.285)	0.64 (0.65–0.62)
	Matched (ATT)	0.520	0.635	−0.115 (−0.136 to −0.094)	0.82 (0.84–0.80)
e	Unmatched (crude)	0.150	0.085	0.064 (0.058–0.071)	1.75 (1.66–1.86)
	Matched (ATT)	0.150	0.138	0.012 (−0.003 to 0.027)	1.09 (1.00–1.18)
h	Unmatched (crude)	0.075	0.062	0.013 (0.007–0.019)	1.21 (1.11–1.32)
	Matched (ATT)	0.075	0.070	0.006 (−0.005 to 0.017)	1.08 (0.96–1.22)
WPe	Unmatched (crude)	0.190	0.027	0.164 (0.160–0.168)	7.10 (6.76–7.46)
	Matched (ATT)	0.190	0.118	0.072 (0.057–0.087)	1.61 (1.48–1.75)
hDWeC	Unmatched (crude)	0.017	0.006	0.012 (0.010–0.013)	3.04 (2.54–3.65)
	Matched (ATT)	0.017	0.012	0.005 (0.000–0.010)	1.42 (1.07–1.88)
hDWPe	Unmatched (crude)	0.029	0.004	0.025 (0.024–0.027)	7.47 (6.50–8.58)
	Matched (ATT)	0.029	0.018	0.011 (0.004–0.017)	1.58 (1.26–1.97)
hDWPeC	Unmatched (crude)	0.019	0.002	0.017 (0.016–0.018)	9.28 (7.81–11.02)
	Matched (ATT)	0.019	0.007	0.012 (0.007–0.017)	2.67 (1.92–3.73)

ATT = average treatment effect on the treated.

the observed covariates. After that we will search for a "killer" confounder, i.e. a confounder which will drive the estimated ATT toward zero, and then assess the plausibility of such particular configuration (Nannicini, 2007).

We also have to assume that a region of common support exists. This implies, among other things, that the distribution of propensity scores of treated and controls have to overlap so we can find for each treated a sufficient number of controls with similar propensity score value. In our case, both the treated and the comparison group are spread around the whole region of the common support (not shown to save space). Finally, the stable unit treatment value assumption should hold. This means that an individual's outcome only depends on his or her own participation and not on the treatment status of others. In our case, this assumption is likely to be valid since the intervention is provided on an individual basis, and it is rare that a contact family hosts more than one child (Andersson & Bangura Arvidsson, 2001).

Results

We estimated the effects of CFP participation on future outcome profiles. The ATT is the difference between the average outcome profile rate of participants and of their matched non-participant peers. For reasons of transparency, we start by presenting crude/unmatched differences in outcome profiles between treatment and control group. These naïve baseline estimates should be used to assess how our matching strategy has worked. After that we present the adjusted/matched differences. To facilitate interpretation, we discuss the ATT expressed as risk ratios rather than as risk differences (Table 3). Separate analyses of boys and girls did not alter the results more than marginally (not shown in tables). To ensure that our results were not driven by variations in follow-up time, we sequentially excluded the older birth cohorts from the analyses. These analyses did not change the overall results either (not shown in tables).

Compared to unmatched peers, CFP children had a 36% lower chance of having no adverse outcomes (RR = 0.64). Regarding the problem-burdened classes, CFP children were more likely to be found in all such outcome profiles. For example, CFP children had a 75% elevated risk of having poor educational achievement (e). The CFP children also had a 21% elevated risk of having poor mental health (h). The most notable crude excess risks, however, were associated with the more problem-burdened profiles. CFP children had a three-fold excess risk of having poor mental health, illicit drug use, extensive welfare recipiency, poor educational achievement and serious criminality (hDWeC). The likelihood of belonging to the class related to extensive welfare recipiency, placement in out-of-home-care and poor educational achievement (WPe), and the class related to poor mental health, illicit drug use, placement in out-of-home care, extensive welfare recipiency and poor educational achievement (hDPWe) was even greater: around a seven-fold elevated risk for both classes. Similar sizeable crude excess risks were also associated with the most problem-burdened profile. CFP children had a nine-fold elevated risk of having poor mental health, illicit drug use, placement in out-of-home care, extensive welfare recipiency, poor educational achievement and serious criminality (hDPWeC).

So far, we have compared people where it does not exist a sound basis for comparison. Therefore, the excess risks reported above are biased upwards and they should accordingly not be used to evaluate the effectiveness of the program. When comparing the CFP children with matched peers, excess risks were – not at all surprisingly – reduced considerably. The adjusted risk for having no adverse outcomes was around 18% lower for CFP children (RR 0.82). The adjusted risk for poor educational achievement class (e) was more or less zero (RR = 1.09). Similar results were also found for having poor mental health (h) class (RR = 1.08). Nevertheless, CFP children were still more likely to have simultaneous problems (WPe, hDWeC, hDWPe). Depending on outcome profile, risk ratios vary between 1.42 and 1.61. Moreover, there was still a notable

Table 4
Simulation-based sensitivity analyses.

	Fraction U = 1 by treatment/outcome (T/Y)				Outcome effect (OR)	Selection effect (OR)	ATT
	T = 1, Y = 1	T = 1, Y = 0	T = 0, Y = 1	T = 0, Y = 0			Risk difference (95% CI)
No adverse outcome							
No confounder (baseline)	0.00	0.00	0.00	0.00	–	–	−0.115 (−0.136 to −0.094)
Confounder (U) like:							
Teenage mother	0.07	0.12	0.03	0.06	0.40	2.67	−0.108 (−0.124 to −0.092)
Paternal criminality	0.38	0.49	0.07	0.18	0.36	6.56	−0.050 (−0.066 to −0.034)
Single mother	0.58	0.60	0.09	0.18	0.44	11.17	−0.037 (−0.055 to −0.019)
e							
No confounder (baseline)	0.00	0.00	0.00	0.00	–	–	0.012 (−0.003 to 0.027)
Confounder (U) like:							
Teenage mother	0.11	0.09	0.06	0.03	2.13	3.16	0.009 (−0.001 to 0.019)
Paternal criminality	0.46	0.43	0.16	0.09	2.06	7.63	−0.016 (−0.028 to −0.004)
Single mother	0.61	0.58	0.16	0.10	1.71	12.62	−0.018 (−0.030 to −0.006)
h							
No confounder (baseline)	0.00	0.00	0.00	0.00	–	–	0.006 (−0.005 to 0.017)
Confounder (U) like:							
Teenage mother	0.08	0.10	0.04	0.03	1.10	3.20	0.007 (−0.001 to 0.015)
Paternal criminality	0.38	0.44	0.11	0.09	1.19	7.88	0.003 (−0.005 to 0.011)
Single mother	0.59	0.58	0.13	0.10	1.30	12.92	−0.003 (−0.013 to 0.007)
WPe							
No confounder (baseline)	0.00	0.00	0.00	0.00	–	–	0.072 (0.057–0.087)
Confounder (U) like:							
Teenage mother	0.12	0.09	0.11	0.03	4.06	2.69	0.072 (0.061–0.081)
Paternal criminality	0.52	0.41	0.34	0.09	5.57	6.31	0.019 (0.007–0.031)
Single mother	0.59	0.58	0.31	0.10	4.15	10.79	0.006 (−0.008 to 0.020)
hDWeC							
No confounder (baseline)	0.00	0.00	0.00	0.00	–	–	0.005 (0.000 to 0.010)
Confounder (U) like:							
Teenage mother	0.15	0.09	0.08	0.03	2.65	3.16	0.004 (0.000 to 0.008)
Paternal criminality	0.57	0.43	0.25	0.09	3.32	7.69	−0.001 (−0.005 to 0.003)
Single mother	0.64	0.58	0.25	0.10	2.94	12.78	−0.004 (−0.008 to 0.000)
hDWPe							
No confounder (baseline)	0.00	0.00	0.00	0.00	–	–	0.011 (0.004–0.017)
Confounder (U) like:							
Teenage mother	0.13	0.09	0.10	0.03	3.56	3.11	0.008 (0.002–0.014)
Paternal criminality	0.60	0.43	0.39	0.09	6.63	7.60	−0.008 (−0.014 to −0.002)
Single mother	0.58	0.59	0.35	0.10	4.95	12.50	−0.009 (−0.015 to −0.003)
hDWPeC							
No confounder (baseline)	0.00	0.00	0.00	0.00	–	–	0.012 (0.007 to 0.017)
Confounder (U) like:							
Teenage mother	0.20	0.09	0.14	0.03	4.82	3.07	0.008 (0.004–0.012)
Paternal criminality	0.68	0.43	0.46	0.09	8.72	7.72	−0.002 (−0.007 to 0.001)
Single mother	0.59	0.59	0.39	0.10	5.83	12.63	−0.003 (−0.009 to 0.003)

excess risks of CFP children to have all adverse outcomes simultaneously (hDWPeC, RR = 2.67). However, the underlying risks are low. The risk difference for this class is very small, around 1% point (RD = 0.012).

Simulation-based sensitivity analyses

None of the estimated treatment effects suggest that the outcomes for CFP children were more likely to be better than for those matched peers who did not receive the intervention. At best, our analyses suggest a null result. However, it is plausible that we have underestimated the effects of CFP participation due to unobserved characteristics related to parental circumstances. To assess if our estimated average program effects are robust to possible deviations from the assumption of conditional independence (unobserved factors do not affect program participation and outcomes), we utilized the simulation-based 'sensatt' program for Stata (for details, see Nannicini, 2007).

We successively examined how our matching estimates were altered when we simulated the effect of a fictive confounder while still were controlling for all the observed relevant covariates (Table 4). Firstly, we simulated a confounder which mimicked one of our modest indicators of program assignment, Teenage mother (see Table 2). Secondly, we simulated a confounder which copied one of our more potent indicators of program assignment; Paternal criminality. Thirdly, we simulated a confounder which imitated our by far strongest indicator of program participation: Single mother. Lastly, we simulated the effect of a "killer" confounder, i.e. a confounder that will drive our results toward sizeable positive effects of CFP participation.

Regardless of outcome profile, the simulated effects of the first confounder were virtually identical with our baseline ATT. The second and third analyses, in which we simulated the effects of stronger confounders, indicated that some of our results are slightly sensitive to potential deviations from the conditional independence assumption (see Table 4). Regarding the outcome profile related to welfare recipiency, placement in out-of-home care and poor educational achievement (WPe) and No adverse outcome, the simulated ATT is driven toward zero. Moreover, the simulated ATT for the outcome profile related to poor educational achievement (e) now suggests a marginally positive impact of CFP. A similar minor positive effect was also found for one of the more problem-burdened profiles (hDWPe). But simulations of a "killer" confounder (not shown in table) suggest that we only can expect substantial positive effects of CFP participation when the confounder is associated with exceptionally large selection and outcome effects (Odds ratio, OR > 20).

The results from the simulation exercises do not necessarily mean that a bias actually exists (Ichino et al., 2008). The majority of our estimated (negative) treatment effects were small and thus potentially more sensitive to a hypothetical bias than larger negative effects would be. However, even though most of our simulated confounders were associated with quite large selection and/or outcome effects, the majority of simulated ATT were still close to the baseline estimates. Only when a confounder was simulated so that it displayed an exceedingly large selection and outcome effect was the ATT driven toward notable positive effects. But the presence among unobservable factors of a confounder with similar characteristics can be considered less plausible in the present setting, where the set of observed variables is quite rich. Taken in conjunction, the simulations suggest essentially that the baseline ATT estimates are robust.

Discussion

This is the first attempt to evaluate the Swedish preventive Contact Family Program (CFP) since it started as a legally mandated intervention 30 years ago. We used an extensive national cohort sample, information from a host of national registers to construct outcome measures and to identify confounders, propensity score matching to construct a comparison group, and person-oriented statistical analyses to estimate outcomes. In spite of the program's wide-spread popularity among users, professionals, policy makers, and members of the social research community (national and international), we found no positive long-term preventive effects of the program. Within the limits of the design of this study, the procedures to construct a comparison group, and the specific long-term outcome profiles examined, the analyses indicate null-effects of the program for all outcome profiles. Regrettably, our results do not support the common assumption among Scandinavian policymakers and professionals that CFP is an effective prevention program – if we use long-term sustainable developmental effects and reduced risk of placement in out-of-home care as outcome measures. Although our analyses are based on imperfect observational data which (among other things) lack potentially important information about the causes of CFP participation, results from extensive sensitivity analyses did not threaten this conclusion. Only a fictive confounder, extremely strongly related to both program participation and outcomes on the scale of OR > 20, would change the main results substantially.

Our results principally confirm previous variable-oriented analyses of the same data where different multiple regression methods for examining single measures of outcome were used (e.g. placement in out-of-home care after intervention, school achievement, indications of mental health problems, and about ten other outcome indicators; Vinnerljung, Brännström, & Hjern, 2011). The previous findings were in essence identical to results reported in this study, but tended to yield slightly stronger negative treatment effect. As noted above, a primary aim of the CFP is to reduce risks of placements in out-of-home care. In earlier analyses assessing this particular outcome isolated from other measures, the results pointed to a substantially increased risk for the CFP-group compared to peers with similar background that had not received the intervention (Vinnerljung et al., 2011). But in this person-oriented study, we see that out-of-home care entries during adolescence rarely appear outside profiles with other indicators of adverse outcome (WPe, hDWPe, and hDWPeC: see Fig. 1). This approach does not indicate any clear negative effects of CFP for these classes, rather a null-result. By adopting a person-oriented approach, in which homogeneity in outcomes is not assumed, we have by means of LCA identified important sub-groups with various patterns of comorbidities. Focusing on outcome profiles rather than on various outcomes analyzed in isolation may also reduce a key problem in evidence-based decision making, namely translating average group results to individuals (Aas & Alexanderson, 2012). Since outcome profiles describe individuals rather than scores on variables, case-decisions (especially for people with complex problems) may be facilitated and use of research evidence among practitioners may thereby increase. To the extent that similar conclusions can be drawn from the results of applying both variable- and person-oriented approaches, the findings are considerably strengthened (Bergman & Trost, 2006). All in all, the results in this study and the results from previous variable-oriented analyses of the same data set point robustly in the same direction: there are no indications of positive long-term preventive effects.

How to understand these results? We know from several studies that the intervention often is used for families with substantial psychosocial problems (Andersson, 1992; Andersson & Bangura Arvidsson, 2001). In a study from Stockholm (the capital), the large majority of children in the CFP came from such backgrounds (Sundell, Humlesjö, & Carlsson, 1994). It seems probable that the intervention in many cases is used as a last resort for children from seriously adverse rearing environments, often with the direct goal to increase local authorities' monitoring of the conditions in the family (Andersson, 1992). The elevated risk of placement in out-of-home care, as found in previous analyses (Vinnerljung et al., 2011), seems a logical consequence of this background picture. The lack of improved long-term developmental outcomes, as reported in this study, should probably also be viewed in this perspective. The intervention was not strong enough for many children who remained in adverse family environments. But judging from other intervention research, it also seems probable that the basic assumptions underlying the intervention – that scheduled access to a supportive "normal" family outside the birth home will lead to reduced risks of deteriorating development – were ill founded. Instead we know from decades of intervention research that successful programs are usually based on identification of variable risk factors, are far more intensive and structured, and contain components that are successful in reducing the influence of these risk factors (e.g. Farrington & Welsh, 2007; Ferrer-Wreder, Stattin, Cass Lorente, Tubman, & Adamson, 2004).

A recent series of national cohort studies suggest that school failure seems to be a powerful mediator – and a determinant – for child welfare clients' long-term development (Berlin, Vinnerljung, & Hjern, 2011; Vinnerljung, Berlin, et al., 2010; Vinnerljung & Hjern, 2011). However, low cognitive ability does not seem to be the decisive factor. Early conduct problems is generally a strong predictor for long-term outcomes, and for school performance (Fergusson, Horwood, & Ridder, 2005a, 2005b). But the linkage between school failure and conduct problems is a two-way street. Conduct problems can lead to school failure, but school failure can also cause both conduct problems and mental health problems (Gustafsson et al., 2010). Thus, a more decisive strategy for the CFP that includes systematic targeting of well-known risk factors – e.g. poor school performance – could produce more beneficial results. This approach could start early by teaching pre-school children to read and to do basic mathematics (primary school starts at age 7 in Sweden). Literacy and numeracy skills, at time of entry into primary school, are the strongest predictors of future school success that we know of so far, even for children with early behavioral problems. These factors are more potent than parental education (Duncan et al., 2007).

So, instead of avoiding or terminating the CFP, we propose using the program's two favorable starting points for more knowledge-informed strategies. Firstly, these children constitute a high-risk group that should be targeted with preventive services. As earlier mentioned, a host of cohort studies has shown high risks for compromised long-term development. The CFP reaches the right children. Secondly, it is an intervention that is in demand by the users, and popular among volunteers and professionals. The reasonable way forward seems to keep the intervention, but equip it with components that in theory have risk-reducing effects. Early literacy and numeracy training for younger children, and substantial efforts to promote good school achievements for older children is one logical way to go. More structured behavioral interventions could possibly also be incorporated into the program, targeting both birth parents and volunteer families (Price, Chamberlain, Landsverk, & Reid, 2009). This type of reinforced CFP should be staged in trials, and evaluated.

An alternative would be to discard long-term ambitions and focus on short-term results. The large majority of CFP children have single mothers, and most of the birth fathers have indications of substance abuse and/or criminality (Vinnerljung et al., 2011). In other words, the intervention does reach a very vulnerable group of mothers. Qualitative studies suggest that the CFP makes life easier for these mothers (Andersson & Bangura Arvidsson, 2001). That in itself could, for sound reasons, be considered good enough. However, such a change in practice ambitions would require transparency from professionals toward policy makers that are responsible for allocating funds to family intervention services.

Conclusions

The results did not find support for CFP effectiveness in reducing risks of compromised long-term development in children and out-of-home care placements. Since the preconditions seem favorable to build on – the intervention reaches a high-risk group of children and is popular among users, volunteer families and professionals – it would be premature to simply terminate the program. Instead, we recommend that the program is reinforced with knowledge-based components that target known risk factors for child welfare recipients, for example poor school performance. These efforts should be explored in trials with high standard evaluation designs.

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