UNIVERSITY OF COPENHAGEN
DEPARTMENT OF ECONOMICS

Essays in Economics of Child Protection

PhD Thesis

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Contents

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I have spent a lot of years studying economics from various perspec-

tives and the last 4 years with a focus on research in labour economics and child welfare. I am proud and honoured to have gained a real interest in and understanding of social science and research. I have studied the complexity of the social and economic world and understood the difficulty of reducing that complexity into useful and tangible results. Thankfully the learning journey is never over, and I look forward to many curious years ahead.

Petra Gram Cavalca, Copenhagen, July 2021

Dansk introduktion

Denne afhandling handler om udsatte børn og om hvordan vi, som samfund, forsøger at hjælpe dem til et godt liv på trods af en svær start.

Vi ved fra forskning, og måske også fra vores egne erfaringer, at vores opvækst påvirker os hele livet og kan have stor betydning for hvordan vi har det som voksne. Vi har som samfund besluttet os for at alle børn har ret til en god og tryg opvækst. Men hvad gør man, når et barn mistrives eller forældrene ikke i tilstrækkelig grad formår at tage vare om barnets behov? Ofte vil man forsøge at hjælpe familien med forskellige typer støtte og rådgivning, eller måske kommer barnet i en aflastningsfamilie en gang i mellem. Det kalder vi forebyggende foranstaltninger. I yderste tilfælde kan man, med eller uden forældrenes samtykke, anbringe barnet udenfor hjemmet. Beslutningen om at anbringe et barn udenfor hjemmet er kompleks, der er mange hensyn at tage og det kan være svært på forhånd at vide om et barn vil have gavn af den ene eller den anden indsats. I de sværeste tilfælde må barnet anbringes udenfor hjemmet. Det er en stor beslutning, der kan have vidtrækkende konsekvenser for både barnet og dets forældre. Derfor har vi også et ansvar for at gøre det så godt som overhovedet muligt. Desværre mangler der viden om hvilke indsatser der virker, for hvem og hvornår. Vi ved ganske enkelt utrolig lidt om hvordan vi bedst hjælper de mest udsatte børn i vores samfund.

I Danmark er knap 12,000 børn anbragt udenfor hjemmet hvert år. Det svarer til cirka 1 procent af alle 0-17-årige og denne andel er stabil over tid. Omkring 5 procent af alle danske børn oplever således at blive anbragt udenfor hjemmet i løbet af deres opvækst.

Når man anbringer et barn, er håbet at forbedre muligheden for et godt og selvstændigt liv på sigt. Desværre ved vi, at mange tidligere anbragte har det svært, men er det et udtryk for medfødte udfordringer, resultatet af det barnet har oplevet før anbringelsen, eller er det anbringelsen i sig selv der er traumatisk? Hvilken effekt har anbringelsen for barnets trivsel, sundhed og skolegang? Griber vi ind i tide, eller får økonomiske hensyn lov til at spille en for stor rolle i anbringelsesbeslutningerne? Og hvorfor er det svært at rekruttere nok kvalificerede plejefamilier?

Det er de spørgsmål jeg interesserer mig for i denne afhandling. Jeg håber at du også synes de er vigtige.

Afhandlingen består af tre kapitler i alt. Kapitlerne kan læses uafhængigt af hinanden og efterfølges af litteraturliste og bilag. Alle kapitler belyser fra forskellige vinkler, hvordan vi som samfund klarer opgaven med at hjælpe børn der ikke trives i eget hjem, og i hvor høj grad det lykkes os at hjælpe de anbragte børn med at finde deres vej i livet. Tak fordi du læser med.

Kapitel 1

med Mette Ejrnæs og Mette Gørtz

I kapitel 1 undersøger mine medforfattere og jeg hvordan børns sundhed, uddannelse og kriminalitet ændrer sig i tiden op til, og umiddelbart efter en anbringelse. Ved brug af detaljerede registerdata, suppleret med unikke survey data fra Københavns Kommune undersøger vi udviklingen i børnenes trivsel i tiden omkring anbringelsen.

I kapitlet viser vi at børn der bliver anbragt, allerede i årene op til anbringelsen er i en stærk negativ udvikling. Vi ser, at en stigende andel af børnene har kontakt med både det somatiske og psykiatriske hospitalsvæsen og har tiltagende skolefravær og kriminalitet i årene op til at de bliver anbragt for første gang. Der sker således vigtige forandringer i børnenes trivsel allerede inden anbringelsen, som nemt kan forveksles med effekter af anbringelsen, selv hvis man ikke har adgang til så detaljerede data som her.

Dernæst viser vi at anbringelsen giver anledning til en stigning i brugen af sundhedsydelser i form af besøg hos praktiserende læge og køb af receptpligtige lægemidler. Denne stigning ser dog ud til at være midlertidig. Samtidig sker der er fald i hospitalsindlæggelser. For nogle børn betyder anbringelsen at de helt forlader skolesystemet, men for de børn der bliver i skolen, forbedres deres fravær efter anbringelsen.

Studiet understreger de udfordringer der er i at måle den kausale effekt af anbringelser og peger på at det er nødvendigt at indsamle flere data hvis man fremadrettet ønsker at evaluere mere systematisk på anbringelsesområdet. Vi viser, at en vej frem er at indsamle mere information fra sagsbehandlingsprocessen, idet sagsbehandlernes vurdering ser ud til at indeholde den information vi mangler i de nuværende registre.

Kapitel 2

I kapitel 2 undersøger jeg en specifik del af anbringelsessystemet, nemlig plejeanbringelserne. Børn der anbringes uden for hjemmet kan anbringes i flere typer af anbringelser, størstedelen af anbringelser i Danmark er i dag i en plejefamilie. Desværre kan det mange steder være svært at rekruttere nok plejefamilier. Samtidig er der et ønske om at anbringe stadig flere børn i plejefamilier fremfor i institutionsanbringelser, hvilket yderligere øger efterspørgslen efter dygtige plejeforældre. Et vigtigt spørgsmål er derfor hvordan vi sikrer at vi kan rekruttere nok, dygtige plejeforældre fremadrettet?

Jeg opstiller en teoretisk model for plejefamiliernes arbejdsudbud, som viser, at familier der vælger at blive plejeforældre har lavere løn på det almindelige arbejdsmarked eller større indre motivation for plejeopgaven. Modellen viser, at der er et potentielt trade-off imellem kvantitet og kvalitet når man vælger hvor meget man skal betale plejeforældre. I den empiriske del af kapitlet undersøger jeg derfor hvilke faktorer der er vigtigst når en familie vælger om de skal blive plejeforældre eller ej. Især undersøger jeg om familiens økonomiske omstændigheder spiller en rolle for om de vælger at blive plejeforældre. Jeg undersøger ligeledes hvordan familiens indkomst og arbejdsmarkedsudbud ændrer sig når de vælger at blive plejeforældre.

Resultaterne viser at plejeforældre er veluddannede, ofte indenfor plejesektoren, med gennemsnitlig indkomst og god tilknytning til arbejdsmarkedet før de bliver plejeforældre. Der er ingen tegn på at plejefamilier vælger plejeopgaven af økonomiske årsager. En del plejeforældre vælger at forlade deres tidligere job når de bliver plejeforældre, hvilket naturligvis fører til et indkomsttab på det almindelige arbejdsmarked. Den økonomiske kompensation plejefamilierne modtager, ud over overførsler for direkte udgifter til plejebarnet, er dog mere end nok til at kompensere for den tabte indkomst og plejefamiliernes samlede husstandsindkomst stiger således med omkring 16 procent i gennemsnit når de bliver plejeforældre.

Kapitel 3

med Mette Ejrnæs og Mette Gørtz

I kapitel 3 undersøger vi hvilken rolle kommunernes økonomi spiller i anbringelsesbeslutninger. Mere specifikt undersøger vi om kommuner der har brugt en større andel af deres budget, anbringer færre børn end kommuner der ikke har brugt så stor en del af deres årsbudget. Vi analyserer også effekten af en stor budgetreform, der introducerede økonomiske sanktioner for kommuner der ikke overholder deres budgetter.

Resultaterne viser, at økonomi spiller en rolle for antallet af anbragte børn i kommunerne. Vi viser at kommuner der er økonomisk pressede i højere grad hjemtager børn, især når de fylder 18 år, og der er således færre der får tildelt efterværn. Samtidig er økonomisk pressede kommuner mere tilbøjelige til at vælge billigere anbringelsestyper. Effekterne er større efter budgetreformen, hvilket understøtter vores fortolkning af resultaterne.

Konklusionen er, at der kan være utilsigtede afledte effekter af at lægge et stort økonomisk pres på kommunerne og samtidig bede dem om at varetage et vigtigt, men ofte overset velfærdsområde, som anbringelsesområdet. Kapitlet viser, at det måske ikke er hensigtsmæssigt at delegere styringen af anbringelsesområdet til kommunalt niveau.

Efter at have læst alle tre kapitler i min afhandling håber jeg, at du har fået en bedre forståelse for kompleksiteten af anbringelsesområdet. Resultaterne der er præsenteret i afhandlingen viser blandt andet hvor svært det er at adskille grunden til anbringelsen (den svære opvækst) fra effekten af anbringelsen selv, og understreger derfor nødvendigheden af at tænke over, hvordan man kan indsamle data der faciliterer evalueringen af anbringelser. Det er helt centralt hvis man ønsker at forbedre de indsatser man tilbyder anbragte børn. Jeg har også vist, at organiseringen af anbringelsesområdet har betydning, både for rekrutteringen af plejeforældre, og for beslutningsprocessen omkring en anbringelse. Anbringelsesområdet har i mange år gået under radaren både forskningsmæssigt blandt økonomer og politisk. Jeg håber, at du som læser har fået et større og bredere indblik i det komplekse anbringelsessystem og hvordan vi som samfund forhåbentlig kan gøre det bedre i fremtiden for de børn, der har allermest brug for det.

Denne Ph.d. afhandling er en del af et større, interdisciplinært forskningsprojekt inddelt i tre underprojekter. Det første projekt dokumenterede beslutningsprocessen i anbringelsessager og mundede ud i en publikation målrettet nuværende og fremtidige praktikere (Ebsen et al., 2017). Det andet projekt er et kvalitativt studie af anbringelser i familiepleje, hvilket har affødt en række publikationer (Andersen and Bengtsson, 2019; Bengtsson and Karmsteen, 2020; Bengtsson and Luckow, 2020) og en Ph.d. afhandling i Sociologi (Luckow, 2019). Det tredje og sidste projekt er et kvantitativt studie af anbringelser, hvilket inkluderer denne Ph.d. afhandling.

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English introduction

This thesis focuses on at-risk children and how we, as a society, attempt to help them to a good life despite a difficult beginning.

We know from research and perhaps also from our own experiences, that our childhood affects us throughout life and can have a large impact on how we act as adults. As a society, we have decided that all children have the right to grow up in a good and safe environment. So what do we do, when a child is not thriving or the parents are not capable of taking care of the child's needs? The first resort is often to support the family through various interventions and counselling, or maybe to let the child stay weekends with another family. These are what we call preventive care interventions. In extreme cases, we can, with or without consent from the biological parents, place the child in out-of-home care. The decision to place a child in out-of-home care is complex. Many factors should be taken into consideration and it can be difficult to know if an intervention will benefit the child. In more severe cases, it can become necessary to place the child in out-of-home care. This is an important decision, which can have far-reaching consequences for the child and its parents. We are obliged to make the best decisions possible in these cases. Unfortunately, there is a lack of knowledge about which interventions are most effective, for whom and under which circumstances. We simply know very little about how best to help the most vulnerable children in our society.

In Denmark, around 12,000 children are in out-of-home care each year. This is equivalent to around 1 percent of all 0- to 17-year-old children and the placement rate is stable over time. Around 5 percent of all Danish children experience an out-of-home placement at some time during their childhood.

When we place a child in out-of-home care, we hope to improve the child's possibility for a good and independent adult life. Unfortunately, we know that many previously placed children have a difficult adult life. But can this difficulty be attributed to genetic disposition, a result of early-life experiences from before the out-of-home care placement, or is it indeed the placement itself that causes trauma? What effect does an out-of-home care placement have on the child's well-being, health, and schooling? Do we intervene in a timely manner, or do we allow economic concerns to influence placement decisions? Why is it a recurring problem to recruit enough qualified foster families?

These are the questions I will address in this thesis. I hope you find them as important as I do.

The thesis consists of three self-contained chapters. The chapters can be read independent of each other and each chapter is followed by a bibliography and an appendix. They each cast light on a different aspect of how we, as a society, manage the responsibility of helping children who do not thrive in their own home, and to what extent we succeed in helping these children find their way in life. Thank you for your interest.

Chapter 1

with Mette Ejrnæs og Mette Gørtz

In chapter 1 my co-authors and I investigate how children's health, education, and crime changes in the time leading up to, and immediately following a placement in out-of-home care. Using detailed administrative register data, supplemented by unique survey data from a large Danish municipality, we investigate the development of the children's well-being in the time around their first out-of-home care experience.

In the chapter we show that children placed in care are already undergoing a strong negative development in the years leading up to placement. The data show that an increasing share of the children are in contact with somatic and psychiatric care and that they have increasing absenteeism from school and crime rate. This confirms that there are important changes in child well-being already prior to the placement, which can easily be mistaken for a causal effect of the placement itself in the absence of highly detailed data.

We then go on to show how the placement gives rise to an increase in health care utilization in the form of visits to the general practitioner and prescription drug purchase. This increase seems to be temporary. At the same time there is a drop in hospitalizations. For some children, the out-of-home care placement makes them drop-out of school, but for those who stay enrolled absenteeism drops following the placement.

Our research highlights important challenges in measuring the causal effect of out-of-home care and shows how important it is to collect more data if we want to evaluate interventions more systematically going forward. We show that one way forward is to collect more data from the caseworker, since their evaluation of the child's situation seem to hold information that supplements current register data in important aspects.

Chapter 2

In chapter 2, I investigate a specific part of the child protection system; foster family care. Children placed in out-of-home care can be placed in various types of care, but in Denmark the majority is placed with a foster family. Unfortunately, many child protection systems have a hard time recruiting enough foster families. At the same time, there is a wish to place a larger fraction of children in foster families rather than institutional care, which increases the demand for qualified foster parents. An important question is how to ensure a sufficient supply of qualified foster families in the future?

I create a theoretical model of foster families' labor supply, which shows that families who decide to foster, have lower wages in the regular labor market, or greater intrinsic motivation for fostering. The model suggests that there is a potential trade-off between quality and quantity, when choosing how much to compensate families for fostering. In the empirical part of the chapter, I look into what factors predict selection into foster parenting in Denmark. In particular, I investigate whether economic circumstances play a role when a family chooses to foster. I also look at how the choice to become foster parents affect household earnings and labor market supply.

The results show that foster parents are well educated, often in care sector professions, with average earnings, and a strong attachment to the labor market prior to fostering. There is no sign that families choose fostering due to economic circumstances. A share of parents choose to leave their regular job when they begin fostering, which naturally leads to an earnings loss in the regular labor market. The current economic compensation, beyond transfers to cover direct living costs for the foster child, more than makes up for the earnings loss in the regular labor market, so the total household earnings increase by around 16 percent on average when a family starts fostering.

Chapter 3

with Mette Ejrnæs og Mette Gørtz

In chapter 3, we study the role of budgetary constraints on child protection decisions. More specifically, we investigate whether municipalities who have spent a larger share of their budget, place fewer children in out-of-home care in comparison to municipalities who did not spend as large a share of their yearly budget. We analyze the effect of a budget reform, which introduced economic sanctions for municipalities overrunning their budgets.

The results show that budget constraints does play a role for the number of children placed in out-of-home care in the municipalities. We show that municipalities who have spent a larger share of their budget are more likely to end out-of-home care for children, in particular when they turn 18-years old. This means that fewer children are allowed to continue in after care. At the same time, these municipalities are also more likely to choose cheaper types of placements. The effects are larger after the introduction of economic sanctions, supporting our interpretation of the results.

The conclusion is that there may be unintended side effects from enforcing strict budget adherence, while asking that municipalities administer such an important, but often overlooked, welfare service as child protection. The chapter shows that it may not be appropriate to delegate child protection services to the local level.

After reading all three chapters in my thesis, I hope that you have gained a better understanding of the complexity of child protection. The results presented in the thesis show how difficult it is to separate the reason for an out-of-home placement (a difficult childhood) from the effect of the placement itself, highlighting the need to consider how to collect data that facilitates the evaluation of child protection intervention. This is essential if we want to improve the interventions, we offer children in out-of-home care. I have also shown that it is important how we organize child protection services. When it comes to the recruitment of foster families as well as for the decision process around an out-of-home placement. Child protection services have in many years been overlooked, both in economic research and politically. I hope that you as a reader have gained a broader perspective on the complexity of child protection and how we, as a society, can improve the way in which we help the children in need.

This Ph.d. thesis is part of a larger interdisciplinary research project divided into three distinct subprojects. The first project was concerned with documenting the decision-making processes in child protection cases and resulted in a publication primarily aimed at future practitioners (Ebsen et al., 2017). The second project is a qualitative study of family foster care which resulted in a number of publications (Andersen and Bengtsson, 2019; Bengtsson and Karmsteen, 2020; Bengtsson and Luckow, 2020) as well as a Ph.d. thesis in Sociology (Luckow, 2019). The third and final project is a quantitative study of out-of-home care, in which this Ph.d. thesis is included.

The research group consists of Mette Ejrnæs (PI), Mette Gørtz and me (Department of Economics, University of Copenhagen), Tea T. Bengtsson and Stine Tankred Luckow (VIVE - The Danish National Center for Social Science Research) and Frank Ebsen and Idamarie Leth Svendsen (University College Copenhagen). The research has been carried out in collaboration with Tove Holmgaard Sørensen and Mette Larsen from the Municipality of Copenhagen, who have kindly provided data for the empirical analyses. The research project is funded by TrygFonden.

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Chapter 1

Health, education and crime of children in out-of-home care

Health, education and crime of children in out-of-home care

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Abstract

Children at risk of out-of-home care are among the most vulnerable in our societies, yet little is known about how out-of-home care affects child outcomes. This paper investigates the short-term impact of placing children in out-of-home care on child health, juvenile crime and schooling. We apply two different strategies. First, we adopt an event study approach using detailed full-population data to investigate the dynamic trajectory of child outcomes from 12 quarters before to 8 quarters after their first placement in out-of-home care. We show that there is a clear deterioration of outcomes related to mental health and juvenile crime in the last eight quarters before placement. When the child enters out-of-home care, we find a temporary increase in health care utilization, a decrease in hospitalizations and an improvement in schooling outcomes. Second, we explore the effect of placement for a small sub-sample of children using additional information we have collected from their caseworkers at the time these children were considered for out-of-home placement. We use the caseworkers' risk assessments of the children to match children placed in care to a control group of non-treated children with similar risk assessments. Using this data, we estimate the causal impact of the placement and find that, for the child at the margin, the effects of out-of-home placement are small but imprecisely measured. The paper contributes to the literature on the effects of out-of-home care for children's health, schooling and crime outcomes by documenting the contemporaneous changes in child outcomes around the time of placement.

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1 Introduction

Child neglect and maltreatment can have devastating consequences for later-life health and well-being (Doyle, 2007; Doyle, 2008, 2013; Currie and Widom, 2010; Paxson and Waldfogel, 2002; Currie and Tekin, 2012). Most Western countries have child protection programs to improve equity in conditions and secure basic rights and safety for at-risk children. These often include preventive measures and support to children and families. Removing children from their parents to place them in care outside their home is often seen as the last resort, and it is one of the most drastic and intrusive interventions into family life. In the US and in other Western countries such as Denmark, an average of around 1 percent of a cohort of children is placed in care, and 5-6 percent of children will experience some type of out-of-home placement before they turn 18 years old (Ejrnæs and Gørtz, 2017a; Turney and Wildeman, 2016). In addition to being a drastic measure, out-of-home placement is a very costly intervention (Cavalca, Ejrnæs, and Gørtz, 2021).

Children who receive child protective services are more likely to experience homelessness, delinquency, unemployment and chronic health conditions than other children later in life, and they have poorer educational outcomes (see for example Doyle and Aizer, 2018 and Lindquist and Santavirta, 2014). Yet the evidence on the causal effects of out-of-home care is sparse and results are somewhat mixed. The lack of causal evidence is related to the challenge of defining a relevant control group for children placed in out-of-home care. The ideal experiment for estimating the causal effect of out-of-home care would be to randomize children into care. Given the obvious ethical concerns related to such an experiment, a number of different approaches using observational data have been adopted.

The first study to credibly estimate the causal effect of out-of-home care adopts a two-stage instrumental variable approach using random assignment of cases to child protection investigators and judges with differential propensity to place children in care (Doyle, 2007). The author finds an increase in juvenile delinquency for the marginal child placed in foster care. Follow-up papers using the same identification strategy show an increase in adult delinquency (Doyle, 2008) and no effect on health outcomes (Doyle, 2013). Other studies using a similar identification strategy provide mixed evidence of the effect of the marginal out-of-home placement. Warburton et al. (2014) finds no effects on criminal behaviour, a reduction in the graduation rate, and an increase in welfare receipts for 16-18 year old Canadian boys having been placed in foster care. Baron and Gross (2020) exploit the quasi-random assignment of child welfare investigators in Michigan, and find positive effects of foster care on child outcomes, in particular schooling outcomes, such as increased attendance and improvement in test scores, as well as a decrease in grade retention. Bald et al. (2019) show, with data from Rhode Island, that girls being removed before the age of 6 had higher test scores and reduced grade repetition, while there was no detectable impacts for boys. Roberts (2019) finds, using data from South Carolina, that for children on the margin, a foster care placement leads to positive and substantial educational effects, which are stronger for children who are younger at the time of investigation.

Studies looking at the average treatment effect of placement rather than the local average treatment effect generally point to zero results. Berger et al. (2015) use a combination of OLS, DiD, fixed effects models, and matching to show that, when adjusting for selection bias, placement has little effect on children's cognitive skills or behavior problems. Lindquist and Santavirta (2014) find no statistically significant association between placement in foster care and criminality.

In this paper, we attempt to reconcile the mixed findings in the previous literature by adopting two new empirical approaches to study the effects of out-of-home care on child outcomes. In our first approach, we use an event study to trace out the dynamic trajectory of child outcomes around the time of their first out-of-home care placement. This allows us to investigate how child outcomes change in the time leading up to placement as well as the short-term impact of placement. Causal identification in an event study of the effect of placement relies on the assumption that for children who are placed in care, the exact timing of the first out-of-home placement is independent of child outcomes. As we will show in the empirical analyses, there are indications that this assumption is not fulfilled; therefore, we also apply an alternative approach. In our second approach, we use survey information collected on a sub-sample of children at risk. The sample contains children for whom an out-of-home placement is considered, and the survey collected caseworker's risk assessments for each individual child. We use these risk assessments to form a control group and estimate the causal effect of out-of-home care for the marginal child.

We exploit extraordinarily rich population-wide longitudinal data with quarterly observations from the Danish administrative registers. We study all children in Denmark placed in out-of-home care for the first time in the years between 2013 and 2016 and who were between 4 and 18 years old at the time of placement. The detailed administrative data allows us to identify around 7,000 children in care, their placement history and their outcomes on a quarterly basis. We observe quarterly number of visits to the general practitioner, share of children for whom prescription drugs are purchased and the share of children who are hospitalized in any given quarter, either to a somatic or psychiatric hospital. We look at the share of children enrolled in elementary school, and for the children enrolled in elementary school we look at the rate of absenteeism. For the older children we observe juvenile delinquency, measured as criminal charges. Our rich data allows us to follow the children closely in a period preceding (two to three years before) and following (two years after) their first out-of-home care placement.

The estimates from our event study show a deterioration in health, schooling outcomes and juvenile crime leading up to the first out-of-home placement. At the time of the placement, we see a sharp increase in hospitalization due to mental illness and an increase in criminal charges, indicating that the timing of placement could be endogenous and triggered by, for example, mental health problems. After the placement, there is a temporary increase in primary care utilization in the year following placement, followed by a stabilization. Hospitalizations decrease and school absenteeism improves for the average child in care, while there is no significant change in juvenile crime following placement.

To overcome the endogeneity problems in the event study, we adopt an alternative approach using additional data from the survey we collected on caseworker risk assessments of children. For every child case investigation in the municipality of Copenhagen over the period 2015 to 2016, caseworkers provided their assessment of individual child risks. This information, which is usually unobserved, is essential to form a valid control group. The survey data is linked with administrative registers, which allows us to evaluate the same set of outcomes as for the full-population analysis. We provide evidence that having access to a measure of caseworker risk assessment of the individual child enables us to match children placed in out-of-home care to a control group of children with very similar characteristics, but who were not placed in care following the survey, and to estimate the causal effects of out-of-home care. Despite the limited size of the survey and that the results are imprecisely estimated, the analysis provides further evidence on the causal effects of out-of-home care. Interestingly, for some of the child outcomes, we find very different results compared to the event study. In particular, we find no indication of causal effects of the placement on either hospitalization or absenteeism. We find some evidence for a small causal increase in visits to the GP.

We contribute to the literature on the causal effects of out-of-home placements on several dimensions. First, our paper is the first to use an event study approach to study child outcomes both before and after out-of-home care. Using high-quality quarterly longitudinal data on child outcomes, we are able to measure contemporaneous and short-run impacts on a broad spectrum of outcomes measuring health, schooling, and crime. Our rich register data, which provides information on the family backgrounds of the children allows us to show that, even using a control group matched on a rich set of observable characteristics, for example, on parental background and prior outcomes, the group of children being placed in care experienced a different development path in the years leading up to placement compared with children who are not placed in care. Second, we exploit a unique, but small, survey data set to show that having access to details about children considered for out-of-home care and a measure of caseworkers' risk assessments of each individual child allows us to match children placed in out-of-home care to a control group of children with very similar characteristics. This allows us to estimate the causal effects of out-of-home care placement using propensity score matching on caseworker risk assessment.

2 Institutional setting

The overall goal of Danish child protection laws is to support at-risk children in attaining "the same opportunity for personal development, health and an independent adult life as their peers".[1] The child protection responsibility lies with the municipalities, which can draw on a range of interventions from various preventive measures to placement in out-of-home care as the most drastic intervention. In principle, out-of-home care is intended to be a temporary arrangement, implying that reunification with the child's parents should be sought when possible. In practice, less than one third of children in out-of-home care return to their biological parents before age 18, and the rest "age out" of care at age 18 or over. Out-of-home placements are primarily for the 0-17 year-olds, but in some cases, the municipality extends the placement up to the age of 22. Around two thirds of children are placed in a foster family, while a third live in institutional care.[2] Note that in this paper we use the term foster children to refer to children in all types of out-of-home care. Many children transition from one type of care into another; on average, children experience 1.4 placements.

A report from daycare workers, school teachers, nurses, doctors, or a neighbor will instigate a municipal investigation into potential child neglect or abuse. Such an investigation takes a general view on the child's situation, investigating the child's behavior, development, health, school and family situation. The investigation draws on the assessment by relevant experts and professionals, and the parents and children aged 15 and above are also heard during the process. An investigation should be concluded within four months of being opened. As a result of the investigation, municipalities can either

[1] Law on Social Services, Ch. 11, Paragraph 46

[2] Own calculations on register data, see Data section for details.

conclude that there is no reason for intervention, or it can initiate preventive care measures or a placement. Survey evidence from Copenhagen (Ejrnæs and Gørtz, 2017a) suggests that the most common reasons for placing a child in out-of-home care are parental neglect (50 percent) and child externalizing behavior and social adjustment issues (33 percent). Less frequent reasons are violence or threats of violence (10 percent) or sexual abuse (2 percent). The municipal council (which consists of elected local politicians) is responsible for the decision to place a child in out-of-home care and parents can appeal such decisions to the National Social Appeals Board (Svendsen, 2017). In grave child abuse cases where a decision is made to acutely place a child into care (without parental consent), a child welfare investigation must be completed within two months of the placement.

An out-of-home placement can also be initiated following various preventive measures. On average, around 55 percent of all children in our register data sample are placed in out-of-home care without any previous preventive measures. Conditional on having received preventive measures before a placement in out-of-home care, the average duration from the first quarter of a preventive measure to the first placement is 9 quarters (or around two years).

3 Data

The analyses presented in the following sections are based on a main sample consisting of quarterly Danish administrative data for the full population of children in Denmark and a subsample of children in the municipality of Copenhagen for whom we collected additional survey information. We link information from several registers using anonymized personal identification numbers to obtain information on children's personal characteristics, child protective services, health care utilization, educational outcomes and criminal charges.

3.1 Main outcomes

We look at 7 main outcomes related to the child's health, education and criminal behavior.

We measure the *number of visits to general practitioner* as the sum of reported health services a child received in the given quarter. The number of visits is reported on a weekly basis and we cap the number of weekly services to +/- 5 visits (following Sundhedsdatastyrelsen, 2019). We measure the *share of children with a prescription drug purchase* by identifying children for whom at least one prescription drug was purchased in a given quarter. This includes all prescription medication sold at a pharmacy or other drug store, but does not include over-the-counter medication or medication given to the patient at the hospital. We do not measure the quantity of drugs purchased since this can be hard to compare across drug types. We measure *hospitalizations* as the child having at least one inpatient or outpatient admission to a hospital. We count the hospitalization in the quarter that the hospitalization was initiated, independent of the duration of the stay. The ICD-10 diagnosis codes distinguish between psychiatric and somatic hospitalizations.

We measure *enrollment* for children between and including ages 7 and 15, using information for all public and private elementary schools in Denmark. *Absenteeism* is measured conditional on enrollment as the percentage of total time the student has been absent from school as reported by the school, regardless of the reason for the absence.

Criminal charges are measured for children 15 years or older who are charged with a crime. A charge is counted in the quarter that the crime was committed as recorded in the criminal register.

3.2 Sample selection

Our main sample consists of all people in Denmark aged 0-24 years old in the period 2010-2018 who experienced their first placement in out-of-home care between ages 4 and 18 in the years 2013-2016. We observe children 12 quarters before their first out-of-home care placement and 8 quarters after, and the balanced sample consists of around 7,000 foster children. Note that we use the term foster children and children in out-of-home care interchangeably. For enrollment in elementary school, we restrict the sample to school-aged children (ages 7-15 throughout the period), and for absenteeism, the sample is restricted to the group of children who were enrolled in school throughout the period. For criminal charges, we restrict the sample to children above the age of criminal responsibility (ages 15 and above). See table 1 for descriptive statistics on the samples.

For comparison, we construct a matched control group consisting of children who were never placed in out-of-home care and is selected to match the foster care sample on age and gender, mother's characteristics and past outcomes measured in all quarters from 8 to 12 quarters prior to the event. The matching is done using propensity score matching (Leuven and Sianesi, 2003) on prior observable characteristics presented in table 2. The estimated propensity score is used to match non-foster children to foster children using the one-to-one nearest neighbor algorithm without replacement. The control

Table 1: Foster children at time of first placement, balanced samples

	Health sample		Enrollment sample		Absenteeism sample		Crime sample	
	mean	(sd)	mean	(sd)	mean	(sd)	mean	(sd)
Age	12.62	(4.0)	11.45	(1.4)	11.16	(1.4)	16.99	(0.1)
Girl	0.48	(0.5)	0.45	(0.5)	0.49	(0.5)	0.48	(0.5)
Placed in out-of-home care with no prior preventive care	0.56	(0.5)	0.54	(0.5)	0.52	(0.5)	0.51	(0.5)
Quarters from first preventive care to first out-of-home care placement	8.28	(5.7)	7.92	(5.8)	8.99	(6.2)	7.46	(5.2)
Placement ongoing	0.29	(0.5)	0.45	(0.5)	0.55	(0.5)	0.00	(0.0)
Legal action								
Placement with consent	0.84	(0.4)	0.83	(0.4)	0.79	(0.4)	0.89	(0.3)
Placement without consent	0.10	(0.3)	0.12	(0.3)	0.15	(0.4)	0.04	(0.2)
Urgent placement	0.03	(0.2)	0.04	(0.2)	0.04	(0.2)	0.01	(0.1)
Other	0.02	(0.2)	0.01	(0.1)	0.02	(0.1)	0.06	(0.2)
Placement Type								
Foster family care	0.29	(0.5)	0.39	(0.5)	0.51	(0.5)	0.07	(0.3)
Kinship care	0.07	(0.3)	0.08	(0.3)	0.11	(0.3)	0.02	(0.1)
Group home	0.20	(0.4)	0.15	(0.4)	0.08	(0.3)	0.20	(0.4)
Institutional care	0.30	(0.5)	0.33	(0.5)	0.30	(0.5)	0.30	(0.5)
Independent living	0.14	(0.3)	0.04	(0.2)	0.01	(0.1)	0.41	(0.5)
Length of placement								
Duration, years	1.34	(1.1)	2.05	(1.8)	2.11	(1.8)	0.61	(0.3)
Spell duration, years	1.71	(1.2)	2.98	(1.9)	3.02	(1.9)	0.67	(0.3)
Reason for placement								
Child risk/externalizing behavior	0.80	(0.4)	0.78	(0.4)	0.71	(0.5)	0.85	(0.4)
Child health concerns	0.35	(0.5)	0.38	(0.5)	0.37	(0.5)	0.35	(0.5)
Abuse/neglect of child	0.60	(0.5)	0.68	(0.5)	0.72	(0.5)	0.44	(0.5)
Adult risk/externalizing behavior	0.50	(0.5)	0.58	(0.5)	0.61	(0.5)	0.45	(0.5)
Other	0.28	(0.4)	0.24	(0.4)	0.27	(0.4)	0.33	(0.5)
Share of reasons due to child	0.51	(0.3)	0.47	(0.3)	0.42	(0.3)	0.60	(0.3)
Share of reason due to parents	0.49	(0.3)	0.53	(0.3)	0.58	(0.3)	0.40	(0.3)
At end of first placement								
Exit before age 18	0.36	(0.5)	0.55	(0.5)	0.50	(0.5)	0.12	(0.3)
New placement	0.17	(0.4)	0.26	(0.4)	0.31	(0.5)	0.04	(0.2)
Continued care after age 18	0.00	(0.1)	0.00	(0.1)	0.00	(0.0)	0.00	(0.0)
Age out	0.46	(0.5)	0.18	(0.4)	0.20	(0.4)	0.83	(0.4)
N	7,000		2,470		762		1,314	

group is assigned a random placebo event quarter. We match on the outcome trajectory in 8-12 quarters prior to the event since we observe a divergence in outcomes between children who are eventually placed in care and children never placed in care from 8 quarters prior to the placement (see appendix section A). Before 8 quarters the outcome levels between the two groups are different, but their trends are largely parallel. The matched control group is similar to the foster children on observable characteristics 8 quarters prior to the event. Since we still see differential pre-trends in the 8 quarters leading up to the event, we only use the matched control group for reference in the descriptive evidence, but they are not used to estimate the impact of placement in the main specification of the event study.

3.3 Descriptive evidence

Figure 1 shows descriptive evidence on the four health outcomes; the quarterly number of visits to a general practitioner, share of children with prescription drug purchase and share of children with a somatic or psychiatric hospital admission. The figure shows the average outcome for foster children and the matched group of non-foster children separately.

Table 2: Treatment vs matched control children, quarter before first placement

	Treatment mean	Control mean	Difference b	t
Age	12.65	11.95	0.70***	(10.7)
Girl	0.49	0.48	0.00	(0.2)
Preventive care	0.31	0.00	0.31***	(55.4)
Average outcome, measured 8 quarters prior to event (t-8)				
Number of GP visits	1.10	1.08	0.02	(0.5)
Somatic hospital contact	0.12	0.12	0.00	(0.2)
Psychiatric hospital contact	0.03	0.03	0.00	(1.7)
Prescription drug purchase	0.31	0.32	-0.01	(-0.9)
Enrolled in elementary school	0.96	0.99	-0.03***	(-8.3)
Absenteeism	0.12	0.10	0.01***	(4.4)
Criminal charge	0.12	0.07	0.05***	(4.1)
Mother's characteristics, measured at year of birth of the child				
Age	27.44	27.15	0.29**	(2.8)
Married/Registered partnership	0.33	0.30	0.02**	(2.9)
Highest completed elementary school	0.01	0.01	0.00	(0.1)
Highest completed secondary education	0.81	0.81	-0.00	(-0.4)
Highest completed tertiary education	0.08	0.07	0.01*	(2.4)
Unemployed	0.05	0.05	-0.00	(-0.5)
On early retirement benefits	0.03	0.03	0.00	(0.5)
On cash benefits	0.35	0.17	0.18***	(23.9)
Criminal charge	0.24	0.25	-0.00	(-0.4)
Psychiatric hospital contact	0.11	0.12	-0.00	(-0.5)
N	6,781	6,781	13,562	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 1a shows that foster children have an average of a little more than 1 visit per quarter to their general practitioner 12 quarters prior to their first out-of-home care placement. From 8 quarters prior to placement we see a clear divergence between foster and non-foster children in the time leading up to the event.[3] The figure also shows a sharp increase in the average number of GP visits at the time of entry into out-of-home care, with a subsequent drop in the number of visits, but the average remains much higher than before the placement in out-of-home care. When we look at prescription drug purchases, figure 1b shows a similar pattern. Prescription drugs are purchased for around 30 percent of children in each quarter in both groups 8 quarters prior to the event. In the last 6 quarters prior to the event there is a clear divergence in the groups, and a sharp increase in prescription drug purchases for the foster children at the time of their first placement, followed by a stabilization at around 45 percent. The lower two panels show the quarterly share of children who experience somatic or psychiatric hospitalization. Figure 1c shows a divergence between the two groups in the 8 quarters leading up the first placement and a small additional

[3]The decline in the outcome variable for the control group is likely to be caused by convergence to the mean.

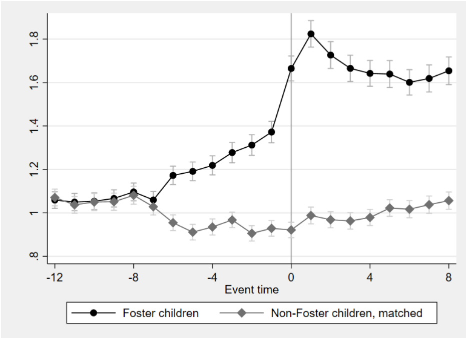
increase in somatic hospitalizations at the time of entry into out-of-home care. We see the same pattern, but much more pronounced, in the share with a psychiatric hospital contact shown in figure 1d. More than 8 percent of foster children have a psychiatric hospital visit in the quarter they are placed in out-of-home care, and the percentage stabilizes at around 6 percent following placement. This should be viewed relative to the less than 1 percent in the matched control group.

Figure 2 shows a significant decrease in the share of foster children who are enrolled in school from more than 95 percent two quarters before placement in out-of-home care to 90 percent two quarters after. The enrollment rate in the matched control group remains high at around 99 percent throughout the event period. There is a higher rate of absenteeism among foster children at around 10 percent absence on average in the years leading up to placement in care. This is followed by a clear drop in absence at the time of placement to an average of around 5 percent for those enrolled throughout the period.

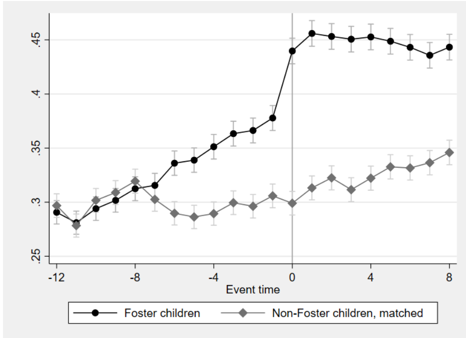
Figure 3 shows an increasing share of children with a criminal charge among foster children in the time before the event. After placement there is a small drop, and the share remains relatively stable at around 10 percent.

The descriptive evidence clearly documents why a traditional matching approach is insufficient to account for differences between foster children and non-foster children even when very detailed administrative data is available. It also highlights the importance of timing when matching on observable characteristics, since large changes in most outcomes occur shortly before out-of-home care placement, which could mistakenly be attributed to the placement itself if only less granular data were available. In the following, we drop the control group and only use foster children to estimate the impact of an out-of-home care placement relative to the quarter prior to placement. Given the substantial pre-trends in several outcomes, we are very careful when interpreting the results of the event study.

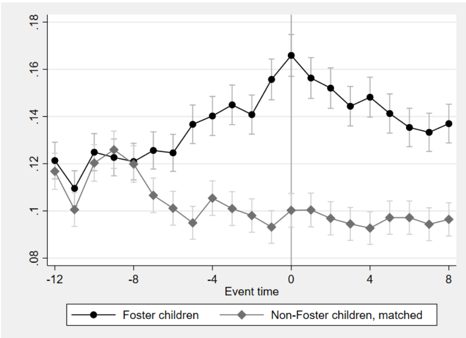
Figure 1: Health, quarterly average



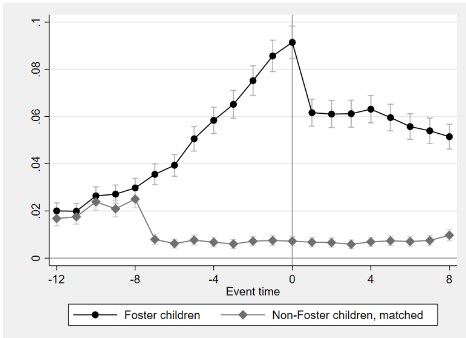
(a) Number of visits to general practitioner



(b) Share with prescription drug purchase



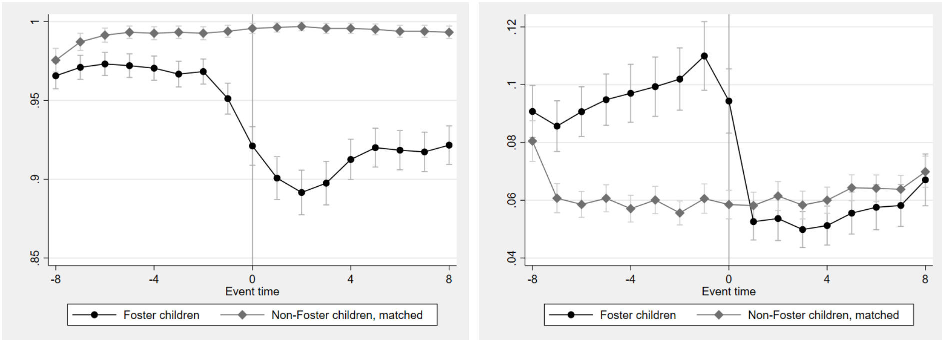
(c) Share with somatic hospitalization



(d) Share with psychiatric hospitalization

Note: The figure shows the average outcomes separately for foster children and the group of matched non-foster children in the balanced health sample (see table 2). The two groups are matched on observable characteristics measured prior to the event, i.e., in quarters t-12 to t-8.

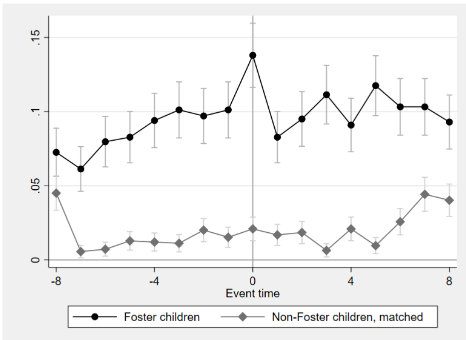
Figure 2: Schooling, quarterly average



(a) Enrollment (b) Absenteeism

Note: The figure shows the average outcomes separately for foster children and the group of matched non-foster children in the balanced schooling sample, consisting of school-age children (ages 7-15). The two groups are matched on observable characteristics measured prior to the event, i.e., in quarters t-12 to t-8.

Figure 3: Juvenile crime, quarterly average



(a) Criminal charges

Note: The figure shows the average outcome separately for foster children and matched non-foster children in the balanced crime sample, consisting of children above the age of criminal responsibility (age 15 and above). The two groups are matched on observable characteristics measured prior to the event, i.e., in quarters t-12 to t-8.

3.4 Survey among caseworkers

From January 2015 to June 2016, we conducted a survey of caseworkers in the municipality of Copenhagen. The survey was intended to provide insights into the "marginal child" by asking questions regarding the caseworkers' risk assessments for each individual child for each child protection decision.[4]

A municipal caseworker concerned about a child will open an investigation to determine whether there are grounds for intervention. Based on the investigation, the caseworker will present the case for the team of child protection caseworkers. At the meeting, the leader and the caseworkers discuss the case and decide whether to recommend an out-of-home placement. One exception for this procedure is if an acute out-of-home placement is needed. In this case, action is taken immediately.

The survey was designed to collect caseworker assessments of every case that was taken up at a meeting, irrespective of whether the child was placed in out-of-home care following the meeting or not. This means that by construction, the survey consists of children at the margin of a placement as all of the children in the survey sample were considered for placement.

For every case in which placement in out-of-home care was considered for a child in the 18-month period in 2015 to 2016, caseworkers were asked to respond to ten short questions. The caseworkers responded to the questionnaire online. The response rate was 89 percent on the distributed survey. Specifically, the questionnaire asked caseworkers to assess the strength of causes for placement for each case, and whether there had been unanimity or disagreement among caseworkers about that assessment. They were asked to assess how concerned (on a scale from 1 to 10) they were for the child, what signs of threats to the child's well-being they saw, how they made their overall evaluation of the concerns and risk factors that were threatening the child, their assessment of resources surrounding the child, and if applicable, which type of placement (foster family versus institutional care) they found most suitable for the child in question. This is information that is not available in the registers or in municipal administrative data, but which may determine whether the child is placed or not.

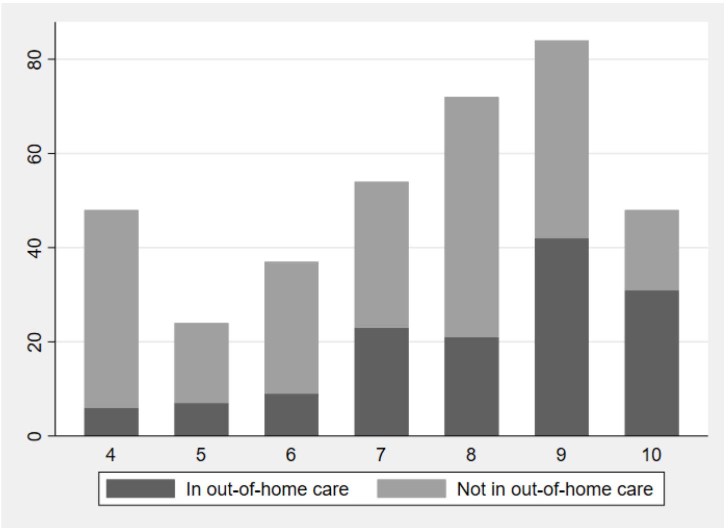
We will primarily use the caseworker's risk assessments, in particular their answer to the question: How do you assess the total burden of risk factors for the child on a scale from 1 to 10, where 1=high risk and 10=no risk.[5] We have reversed the scale to facilitate interpretation of the results. Figure

[4]See Ejrnæs and Gørtz, 2017b for a description of the main results of the survey.

[5]DK: Hvordan vurderer du den samlede belastning af bekymringer/risikofaktorer for barnet påen skala fra 1 til 10 hvor 1=høj belastning fra risikofaktorer og 10=ingen belastning fra risikofaktorer?

4 shows how the cases were distributed on the risk factor scale divided by those children who were subsequently placed in out-of-home care and those who were not. We have also experimented with using information from other questions, but as most of the questions are highly correlated it does not make a difference.

Figure 4: Distribution of risk assessment



Note: Due to the small number of observations at each risk assessment level, risk assessments of ≤ 4 are grouped at risk level 4.

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The final survey dataset contains information on 350 children of which a placement of 139 children was initiated after the case meeting. The survey responses were subsequently anonymized and linked at the individual level to the same type of register information on socio-economic characteristics that was used in the first set of analyses on the full-population register data. We refer to the children who are placed in care after the meeting as the treatment group and those who were not placed in care as the control group. Table 3, columns 1-2, shows descriptive statistics based on register data for all survey children. As shown in columns 3-4 in the table, there are still substantial differences between the two groups of children even when we only look at children who were considered for out-of-home placement. Importantly, children who were subsequently placed in care following the case meeting had a more severe risk assessment by the caseworkers on average than those who were not placed in care.

To improve the balance between the treatment and control group, we use propensity score matching to match the treatment and control groups on age, sex and the caseworkers' assessments. The matched survey sample consists of 262 children, of which 131 (50 percent) were placed in out-of-home care after the survey.

Table 3, columns 5-8, shows that once we match on age, gender and caseworker risk assessment, the matched survey children that were not placed in care were very similar to the survey children who were placed in out-of-home care.[6] Outcomes at the time of the survey are not significantly different and we see only small differences in mother's characteristics. It is striking how similar these children are when comparing observable characteristics. Taking into account the caseworker risk assessment allows us to identify what looks like a valid control group compared to just matching on a large number of characteristics observed in the registers for the full-population sample. This suggests that the caseworker assessment likely holds important additional information that is not readily observable even in comprehensive and detailed administrative data.

Table 3: Survey treatment vs control children, survey quarter

	Unmatched				Matched			
	Treatment mean	Control mean	Difference b	t	Treatment mean	Control mean	Difference b	t
Matching variables								
Age at time of survey	13.56	10.43	3.14***	(5.2)	13.37	13.05	0.31	(0.5)
Girl	0.55	0.48	0.07	(1.2)	0.52	0.51	0.01	(0.1)
Caseworker risk assessment	8.11	6.77	1.34***	(6.2)	7.99	7.60	0.39	(1.9)
Outcome variables								
Number of GP visits	1.24	1.38	-0.15	(-0.7)	1.21	1.31	-0.09	(-0.4)
Somatic hospital contact	0.17	0.17	0.01	(0.2)	0.17	0.18	-0.01	(-0.2)
Psychiatric hospital contact	0.10	0.02	0.08**	(3.1)	0.09	0.04	0.05	(1.8)
Prescription drug purchase	0.34	0.29	0.04	(0.9)	0.32	0.32	0.00	(0.0)
Enrolled in elementary school	0.81	0.80	0.01	(0.2)	0.81	0.76	0.04	(0.6)
Absenteeism	0.20	0.15	0.06	(1.1)	0.20	0.17	0.03	(0.6)
Criminal charge	0.11	0.11	-0.00	(-0.0)	0.11	0.13	-0.01	(-0.2)
Mother's characteristics								
Age	28.86	28.19	0.66	(1.1)	28.93	28.13	0.80	(1.2)
Married/Registered partnership	0.33	0.37	-0.04	(-0.7)	0.33	0.45	-0.12*	(-2.0)
Highest completed elementary school	0.03	0.07	-0.04	(-1.6)	0.03	0.10	-0.07*	(-2.3)
Highest completed secondary education	0.69	0.64	0.06	(1.1)	0.69	0.63	0.06	(1.0)
Highest completed tertiary education	0.08	0.16	-0.08*	(-2.3)	0.08	0.14	-0.06	(-1.6)
Unemployed	0.04	0.06	-0.01	(-0.6)	0.04	0.05	-0.02	(-0.6)
On early retirement benefits	0.02	0.02	0.00	(0.2)	0.02	0.01	0.02	(1.0)
On cash benefits	0.39	0.38	0.00	(0.1)	0.40	0.42	-0.02	(-0.4)
Criminal charge	0.29	0.29	-0.00	(-0.0)	0.29	0.24	0.05	(0.8)
Psychiatric hospital contact	0.16	0.24	-0.08	(-1.8)	0.16	0.15	0.01	(0.2)
N	139	211	350		131	131	262	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

[6] The number of observations is lower, since not all observations could be matched satisfactorily in the propensity score matching procedure.

4 Empirical strategy

To structure our discussion of the impact of a placement, we use the framework of Freyaldenhoven, Hansen, and Shapiro (2019). Let z_{is} be an indicator for the placement of child i in out-of-home care in period s. We assume that the decision about placement of a child is initiated if the risk of the child's safety and health exceeds a threshold. Let η_{is} be a measure of the child's general risk and η^* the threshold. The indicator for placement is given by $z_{is} = 1(\exists s^* \geq s : \eta_{is} \geq \eta^*)$, such that the child i is in out-of-home care in period s if $z_{is} = 1$. We now consider the outcome Y_{is}, e.g., the health or school outcome for the child, and assume that it is affected not only by the placement decision but also by the underlying risk to the child:

$$Y_{is} = \beta z_{is} + \gamma \eta_{is} + \alpha_i + \epsilon_{is}. \tag{1}$$

We assume strict exogeneity of the placement with respect to ϵ_{is}. α_i is an individual fixed effect, which we can account for given the panel structure of our data. In this model z_{is} is endogenous if $\gamma \neq 0$ and η is unobserved. If η_{is} is observed, we can solve the endogeneity problem by controlling for η_{is}.

4.1 Event study

In the event study framework, we introduce an additional time variable: the event time t, which is defined such that $t = 0$ in the first period that the child is placed in out-of-home care ($\min s : z_{is} = 1$). We index all periods relative to that period. In the baseline specification, we consider a balanced panel of children placed in care from 12 quarters prior to their first out-of-home placement to 8 quarters after. We study the evolution of a set of child health and schooling outcomes across event time, focusing on high-frequency outcomes that are available on a quarterly basis.

If η_{is} is unobserved, we would expect to see a trend in the outcome variable prior to the first placement (see Freyaldenhoven, Hansen, and Shapiro, 2019). A large spike or dip in the outcome variable around the time of the first placement may also suggest that the outcome in itself can trigger a placement, e.g., charges of crime or hospitalization. Therefore, it is also important to consider the dynamics of the outcome leading up to the placement. More information about this process may also allow us to determine which outcomes could potentially impact the decision about a placement.

We estimate the level change in the average outcome for foster children relative to the quarter before placement, controlling for age and quarter fixed effects. We model the outcome of interest, Y_{iqt}, for individual i, in quarter q, at event time t for child i in a non-parametric event study as follows:

$$Y_{iqt} = \sum_{s \neq -1} \beta_s \cdot \mathbf{I}[s = t] + \delta_{iq} + \gamma_q + \alpha_i + \eta_{iqt} + \epsilon_{iqt}. \tag{2}$$

The first term on the right hand side is the full set of event time dummies, where $t = -1$ is left out so the remaining event time coefficients represent the difference in outcomes with respect to the quarter prior to placement. The model includes age dummies (δ) and quarter dummies (γ) to control for underlying age and time effects. Without controlling for age and time, the model would simply yield the level change in the mean outcome for the group relative to $t - 1$. The three sets of dummies are all identified in the model due to variation in the age and the calendar time at which a child experiences the first placement. In some of the specifications we allow for an individual fixed effect (α).

For a causal interpretation of the estimates from the non-parametric event study, the timing of first placement in out-of-home care should be uncorrelated with the outcome conditional on being placed in care, and on time and age effects. In addition to the non-parametric event-study, we estimate a parametric version of the model following Dobkin et al. (2018), where we allow for a linear pre-trend in event time t such that

$$Y_{iqt} = \sum_{s > -1} \beta'_s \cdot \mathbf{I}[s = t] + \delta t \cdot + \delta_{iq} + \gamma'_q + \alpha'_i + \eta_{iqt} + \epsilon'_{iqt}. \tag{3}$$

The event time coefficients (first term on the right hand side) now identify the post-event effect relative to the linear pre-trend. We assume that the pre-trend would have continued in the post-period in the absence of the event. In other words, if the children had not been placed in care, we assume outcomes would have evolved as predicted by the linear pre-trend. Although this is a strong simplification, it may be a relatively good short-run approximation and yield a conservative estimate of the effects of foster care on child outcomes. For a causal interpretation of the parametric event study coefficients, we need the timing of first placement to be uncorrelated with deviations from the linear trend conditional on being placed in care, and on time and age effects. We also have to assume that no third factor that is correlated with the outcome variable occurs at the same time as the event.

In the main graphs we will present the non-parametric event time coefficients as estimated in model 2 with the linear trend estimated from the parametric event model 3. This allows us to visually evaluate the fit of the linear pre-trend to the non-parametric changes in outcomes in the periods prior to the event and to evaluate the magnitude and statistical significance of the post-event coefficient estimates.

To test the robustness of the main results from the event study, we estimate the impact of foster care on child outcomes using three alternative approaches presented in detail in appendix B. First, we estimate model (2) with a control group and individual fixed effects, where we use children that are placed in out-of-home care 8 quarters later as a control group for the treatment group following Fadlon and Nielsen (2019) (see appendix B.1). Second, we estimate model (2) with individual fixed effects, restricting two pre-periods to zero to identify all parameters (see appendix B.2). Third, we estimate model (2) with the matched control group and unit fixed effects (the matched control group is the one described in section 3, see also appendix B.3).

4.2 Survey matching

In the second empirical approach, we use children who were considered for out-of-home care and who had a similar caseworker risk assessment as a control group for the children placed in out-of-home care. The control group contains children who were considered for out-of-home care but where caseworkers decided not to place the child in care. This implies that we limit our attention to children with a high η, who are all on the margin to be placed. In addition, we have collected a new measure of child risk (η_{is}), which is usually unobserved. We use this variable to form a control group using propensity score matching. The idea behind our approach is to compare children who are placed in out-of-home care with children who are not but who have the same caseworker assessed propensity for a placement.

The survey data provides information on the caseworker risk assessment for children on the margin of placement. This allows us to identify the causal impact of an out-of-home care placement. With a slight modification of the notation, we consider the event time to be t time since caseworkers were considering a placement and the event time measures the quarters since the case meeting. The causal effect of placement measured 8 quarters after the caseworker meeting is identified by

$$E(Y^1_{iq8} - Y^0_{iq8} | p(\eta_{iq0}, x_{iq0})). \tag{4}$$

We construct the control group by propensity score matching with respect to sex, age (denoted by x) and case worker risk assessment (η), where p denotes the propensity score function. We use the estimated propensity score in a one-to-one nearest neighbor matching algorithm to match children considered for, but not placed in care to children placed in out-of-home care after the case assessment meeting. The effect we identify is mainly determined from the children who are at the margin of being placed in out-of-home care, since both the treatment and the control children were considered for placement.

To account for the fixed individual effect, we also use an alternative estimation method where we measure the changes in outcomes between period t=0 and t=8: $\Delta Y^z_{iq8} = Y^z_{iq8} - Y^z_{iq0}$ for $z_{iq0} = z$

$$E(\Delta Y^1_{iq8} - \Delta Y^0_{iq8} | p(\eta_{iq0}, x_{iq0})). \tag{5}$$

5 Results

We will present two sets of results corresponding to the two empirical approaches. In the first set of results, we examine the evolution of child outcomes in the years around their first out-of-home care placement in an event study. The event study coefficients measure the level change in outcomes for children relative to the period before placement, when controlling for time and age fixed effects. This set of results sheds light on the average impact of placement in out-of-home care on child outcomes. In the second set of results, we apply the caseworkers' assessment to form a control group of children who are not placed in care. By comparing the children who are placed with a control group, we obtain an estimate of the impact of the placement.

5.1 Event Study

Figure 5 shows the estimated change in health outcomes relative to the quarter before placement for children. The point estimates are from the non-parametric model 2 and the linear pre-trend estimated from the parametric model 3. This allows us to graphically inspect the change in outcomes across event time relative to the linear pre-trend.

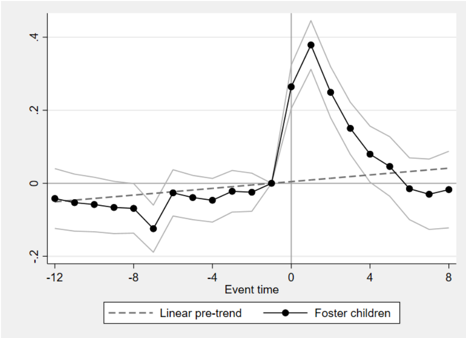
The estimated impact in 5a shows a large and statistically significant upwards jump in number of visits to the general practitioner at the time of entry into out-of-home care. The increase seems to be only temporary. A year after the placement began, the number of visits to a general practitioner, conditional on age and calendar time effects, has decreased again to the pre-event level. We would expect to see a temporary increase if children are, e.g., taken for a routine health visit at the time of placement. A similar change is, however, also reflected in Figure 5b, which shows a significant increase in prescription drug purchases for children at the time of placement, followed by a drop in the years following placement in care. Changes in these health outcomes may reflect either changes in health care utilization behaviour or changes in the true underlying health of the child.

The rich register data on health care use allows us to dig deeper into specific types of drugs and diagnoses that are prevalent for this group of children. Details can be found in the Appendix section C. Prescription drug purchases by drug type are shown in figure C.5. On the one hand, the significant increase in the purchase of asthma medication indicates that the increase in drug purchases following placement in care is likely to, at least partly, be driven by utilization effects. On the other hand, the share of children using ADHD medication, benzodiazepines and related drugs gradually decreases in the years following out-of-home care placement. This development possibly reflects changes in underlying health.

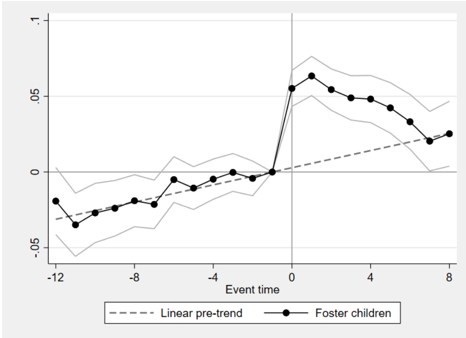
Figure 5c shows a gradual decrease in somatic hospitalizations following placement in out-of-home care. When we turn to psychiatric hospitalizations, we see a strong increase in the two years leading up to placement in out-of-home care, followed by a large and continued decrease in psychiatric hospitalizations after placement in care, see Figure 5d. The strong pre-trend indicates that psychiatric hospitalization may in itself be a decisive factor in initiating an out-of-home placement, and we are cautious with any causal interpretation of the subsequent drop in hospitalizations.

A closer look at hospitalizations by diagnosis groups suggests that the decrease is at least partly driven by a decrease in injury related hospitalizations (see Figure C.1). We interpret this as evidence that the decrease in somatic hospitalizations, at least to some degree, does reflect improvements in the underlying health of the children, perhaps mediated by a safer environment or potentially less risk-seeking behavior, although it is impossible to rule out that utilization effects may play a role.

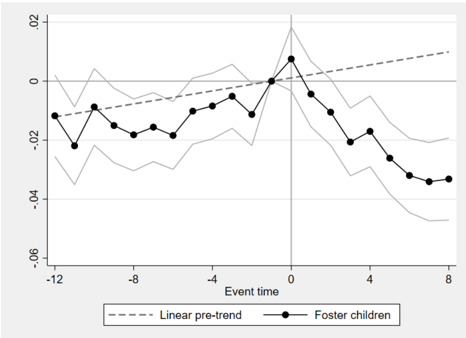
Figure 5: Health



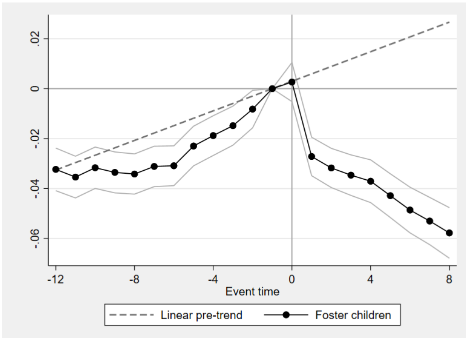
(a) Number of visits to general practitioner



(b) Share with prescription drug purchase



(c) Share with somatic hospitalization

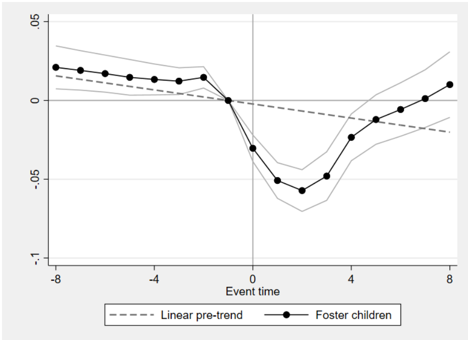


(d) Share with psychiatric hospitalization

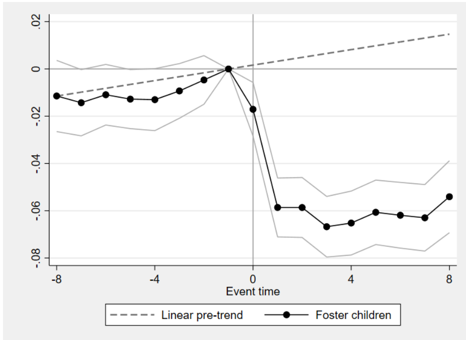
Note: The figure shows the estimated event coefficients from model 2 and the pre-trend from model 3, estimated for the balanced health sample of children in care.

Figure 6 shows that the share of children who are enrolled in elementary school decreases around the time of placement, but recovers within a year from the time of first placement. Absenteeism conditional on enrollment, on the other hand, decreases significantly at the time of placement and remains low throughout the observation period. We interpret the changes in schooling outcomes as reflecting a generally positive impact of the placement in out-of-home care. There is no statistically significant change in criminal charges across event time as shown in Figure 7.

Figure 6: Schooling



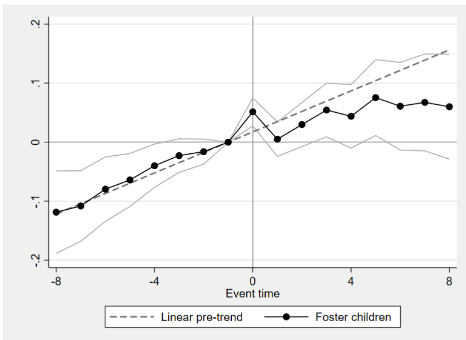
(a) Enrollment



(b) Absenteeism

Note: The figure shows the estimated event coefficients from model 2 and the pre-trend from model 3, estimated for the balanced school sample of children in care. Absenteeism is conditional on enrollment throughout the event period.

Figure 7: Juvenile crime



(a) Criminal charges

Note: The figure shows the estimated event coefficients from model 2 and the pre-trend from model 3, estimated for the balanced crime sample of children in care.

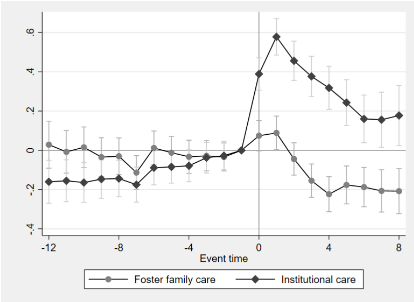
Robustness of results

We perform three additional estimations to test the robustness of the event study results, see appendix B. The first robustness check uses later-placed children as the control group, following the design used in Fadlon and Nielsen (2019). The second robustness check uses an event study design, but with individual fixed effects instead of modelling a trend. And the third robustness check uses an event study design combined with propensity score matching. All three alternative estimation approaches yield results that are very similar to the main results. One exception is the estimates for schooling outcomes in the specification with unit fixed effects presented in B.2. The results from this specification show no temporary drop in enrollment in the year following placement in out-of-home care, but instead shows a gradual and sustained increase in enrollment following the event. The decrease in absenteeism following placement in out-of-home care is estimated to be larger at the time of placement compared with the main estimates, and to continue to decrease in the years following the event. In this sense, the main results may represent conservative estimates of the positive impact of out-of-home care on schooling outcomes.

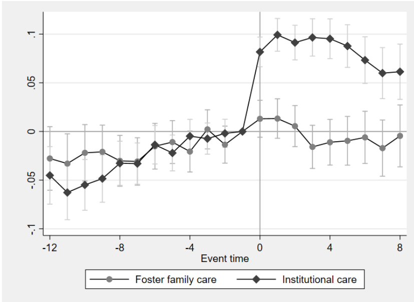
Heterogeneous effects

To check for heterogeneous effects, we split the sample according to type of care, gender and age. When splitting the sample into children who are placed in foster families and children in institutional care we see remarkable differences, as shown in figure 8. For children in foster families, we do not see the same deterioration in health and school outcomes prior to placement. For most of the outcomes there are no pre-trends, with the exception of juvenile crime. Moreover, we see clear improvements in health and schooling outcomes after the placement, since both the rate of hospitalization, the number of GP visits and absenteeism fall. On the other hand, children in institutional care follow the same development as shown in the main graphs, but with even larger changes. Although it is tempting to try to draw causal inference on the effect of foster families versus institutional care from the comparison of the outcomes of the two types of care, however, it is important to remember that children placed in institutional care are different to children placed in family foster care. They are older and often have more complex needs.

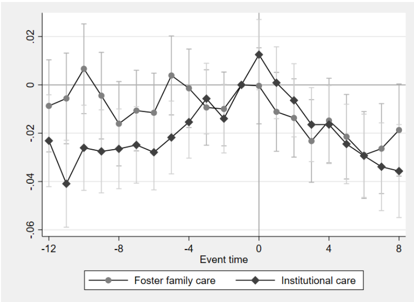
Figure 8: Heterogeneity by type of care



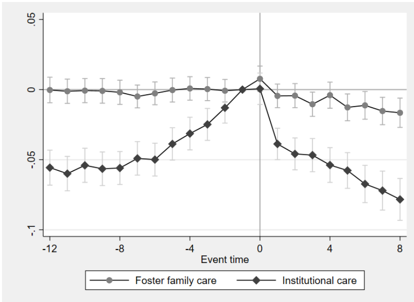
(a) GP visits



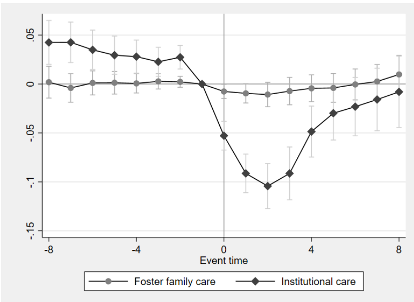
(b) Prescription drug purchase



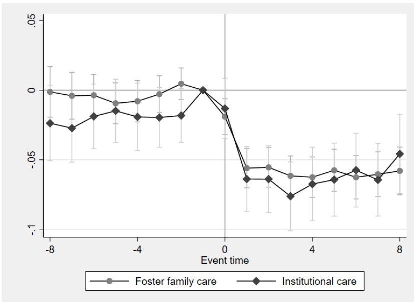
(c) Somatic hospitalization



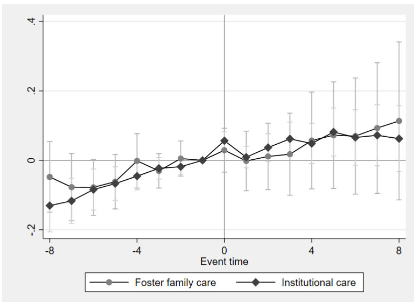
(d) Psychiatric hospitalization



(e) Enrollment



(f) Absenteeism



(g) Criminal charges

Note: The graphs show the level change in outcomes relative to period t=-1 separately for children placed in institutional care and family foster care.

Results by age and sex are shown in Appendix section D. When splitting the sample according to age in Figure D.2, we see that the effects on health are mainly driven by the oldest children (aged 13-16). For school outcomes, there are only minor differences across age.

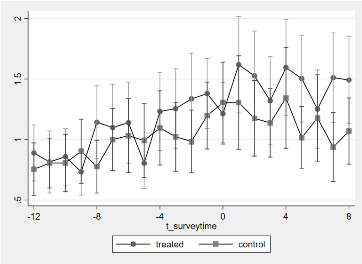
When comparing the effects across sex, see Figure D.1, we find that the health effects are stronger for girls, while the effects on juvenile crime are stronger for boys.

5.2 Survey matching

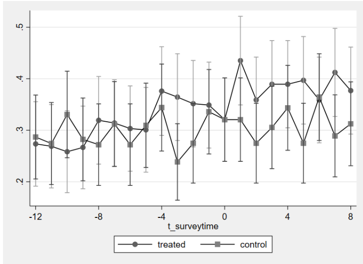
In this section, we present results for the local average treatment effect of placement using our matching strategy on the survey data collected for this project. We compare the outcomes of children who were placed in care with a matched control group of children who were not placed, but had similar risk levels as assessed by the caseworker.

Figure 9 shows event graphs on health and school outcomes for the two groups. The event time $t = 0$ refers to the quarter in which the decision about placement was taken. All the event graphs show that the two groups are not significantly different neither before nor after the placement, partly due to small sample size. When taking a closer look at the outcomes, we find a weak tendency fort visits to a GP and drug purchases to increase after the placement relative to the control group and that the enrollment rate is decreasing for the children in care relative to the control group.

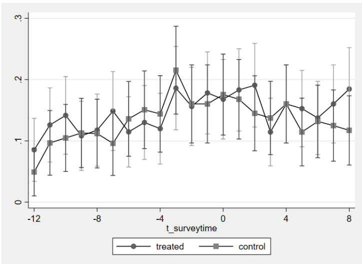
Figure 9: Main outcomes, quarterly averages



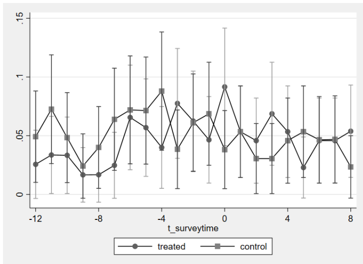
(a) Number of visits to general practitioner



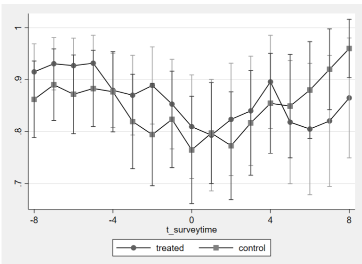
(b) Share with prescription drug purchase



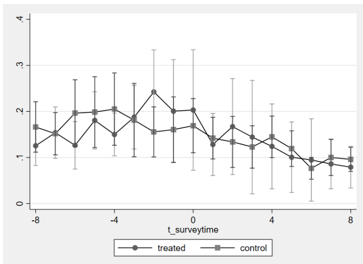
(c) Share with somatic hospitalization



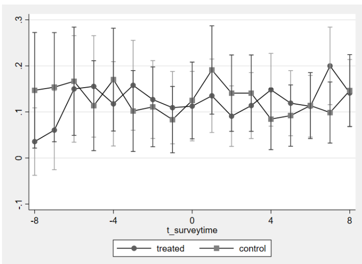
(d) Share with psychiatric hospitalization



(e) Enrollment



(f) Absenteeism



(g) Criminal charges

Note: The figure shows the average outcomes separately for children placed in care after the survey and children who were not placed in care after the survey. The two groups are matched on age, gender and caseworker risk assessment. The sample is unbalanced across event time.

To further investigate the effect of placement, we look at child outcomes 8 quarters after the placement decision was taken. The results are shown in the top part of table 4. We do not see any significant differences between the children placed in out-of-home care and those who are not placed in care. The point estimates indicate an increase in GP visits, hospital contacts and prescription drug purchases and a decrease in enrollment, absenteeism and criminal charges. None of these differences are statistically significant.

In Appendix section E, we show additional regression analyses where we pooled all observations after the care decision is taken. In the regression framework, we control for background characteristics (see Table E.3). We find largely the same effects, and now the increase in GP visits is statistically significant.

Finally, we take individual fixed effects into account by looking at the average change in outcomes between periods t=0 and t=8 and compare the two groups, see table 4. The last column shows the difference in differences between the two groups over time. We see largely similar results, with a few exceptions. The difference between the number of GP visits is now a little larger and marginally statistically significant. The difference in psychiatric hospitalizations has switched sign from positive to negative, but remains insignificant. The difference in school enrollment and absenteeism between the treatment and control groups has increased, but it remains insignificant, and the same is true for the change in criminal charges. When we do the same exercise, but pool all observations from $t = 1, .., 8$, we get the same results, namely, the only significant effect of placement is found in the number of visits to a GP (see Table E.4).

Generally, our results using survey data show no significant short-run effects of the placement on health and school outcomes. The only marginally significant effect is on the number of GP visits, which on average, increase by around half a visit after a placement. Where the results for visits to a GP and partly also for enrollment are in line with the results of the event study, the results on hospitalization and absenteeism are inconsistent with the event study. The decline in hospitalization and absenteeism found in the event study is unlikely to be a causal effect of the out-of-home placement as the control group based on survey data displays a similar decline.

Table 4: Survey treatment vs control children, 8 quarters after survey

	Treatment mean	Control mean	Difference b	t
Average outcome in t=8				
Number of GP visits	1.49	1.07	0.42	(1.8)
Somatic hospital contact	0.18	0.12	0.07	(1.5)
Psychiatric hospital contact	0.05	0.02	0.03	(1.3)
Prescription drug purchase	0.38	0.31	0.06	(1.1)
Enrolled in elementary school	0.86	0.96	-0.10	(-1.6)
Absenteeism	0.08	0.10	-0.02	(-0.7)
Criminal charge	0.14	0.15	-0.01	(-0.1)
Average change in outcome from t=0 to t=8				
Δ Number of GP visits	0.28	-0.25	0.53*	(2.0)
Δ Somatic hospital contact	0.02	-0.06	0.08	(1.2)
Δ Psychiatric hospital contact	-0.04	-0.02	-0.02	(-0.6)
Δ Prescription drug purchase	0.06	-0.02	0.08	(1.2)
Δ Enrolled in elementary school	0.00	0.14	-0.14	(-1.4)
Δ Absenteeism	-0.04	0.02	-0.06	(-1.2)
Δ Criminal charge	-0.01	0.02	-0.03	(-0.4)
N	130	128	258	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: The top rows of the table show average outcomes 8 quarters after the survey for all survey children observed in that quarter. The bottom rows show the average change between the survey quarter and 8 quarters after the survey. For this reason, the bottom rows are conditional on observing an outcome for each child in both quarters and results may differ with respect to the unbalanced sample.

6 Conclusion

This paper examines the development in child outcomes around the time of a placement in out-of-home care.

The first part of our analysis uses a unique data set with administrative register data for all children experiencing a placement in out-of-home care in the period 2013 to 2016 in Denmark. The analysis points to a remarkable development in child outcomes up to two years *before* as well as *after* a placement in out-of-home care. Estimation results indicate a deterioration in health and an increase in juvenile crime in the period leading up to the placement, while the effect is less clear for school outcomes. This suggests that health issues and juvenile crime can trigger a placement.

After a placement, the picture is mixed. Some child outcomes improve following a placement. We find that, on average, the probability of hospitalization decreases after placement, while visits to the GP and prescription drug purchases remain at a high level. These effects may partly be driven by an increase in health utilization (for example an increase in asthma medication) and partly by improvements in underlying health (for example a reduction in hospitalization due to accidents). Turning to school outcomes, we see two opposite effects: a decrease in enrollment and a reduction of absenteeism for those who are enrolled.

In the second part of the empirical analysis, we use a survey data set that we collected on caseworker assessments of risk levels for a small sample of children who were all considered for out-of-home placement in Copenhagen during 2015-16. The purpose of this survey was to collect information about a group of children that shared quite similar circumstances in the sense that they were all investigated for the purpose of deciding whether to place them in out-of-home care or not. Some where subsequently placed in out-of-home care, others were not, but all could be thought of as being a "marginal child" on the verge of a placement. Due to the limited sample size of the survey, the results are generally imprecisely estimated. However, the results point to important insights into the causal effects. The increase in GP visits after placement found in the event study are likely to be a causal effect. Likewise, we find an indication that the fall in school enrollment for children in care could be a causal effect driven by children in institutional care. In contrast, the decline in hospitalization and absenteeism after the placement that was seen in the event study are unlikely to be a causal effect of the placement. The fact that we see a similar decline for children who are not placed indicates that it is not the placement in itself that causes the decline. However, it is important to note first, that the marginal children who are not in out-of-home care will receive other types of support and caseworkers will follow these children closely. Second, these results are effects on the marginal child and may not hold for all children in out-of-home care.

Our paper makes several important contributions. First, while a number of recent papers (Doyle, 2007; Doyle, 2008, 2013; Doyle and Aizer, 2018; Warburton et al., 2014; Baron and Gross, 2020; Bald et al., 2019) were able to identify the causal effects of placing the "marginal" child in foster care, our results based on an event study are related to placement of the average child. Our unique longitudinal data allows us to document the development in key outcomes of the children quarter by quarter. This description of the process leading up to placement as well as after placement clearly illustrates that the group of children being placed in out-of-home care had experienced a deterioration in their situation over several years before the decision to place them in care was taken. This underlines the difficulty in defining a relevant control group for this group of children and illustrates why ordinary propensity score matching on socioeconomic characteristics as well as previous child outcomes is generally not successful in identifying causal effects for placements in care.

Second, we contribute by exploiting a survey that we conducted to better understand the assessments and decisions that caseworkers have to make. Two features of this survey are important. First, the survey is designed to only consider "marginal" children, for whom a placement is considered. Second, when including caseworker risk assessments, which are usually unobserved, as an additional variable in our propensity score matching, the treatment and control groups become much more similar in terms of socioeconomic characteristics. Hence, including measures such as caseworker risk assessment improves the potential success of matching methods to identify causal effects by providing information that allows us to compare children who are similar on parameters that are essential in the placement decision.

Our analysis thus suggests that having better data on child characteristics, such as an assessment of, e.g., risk factors, would substantially improve policy makers options for evaluating public programs such as out-of-home care. Our paper provides a suggestion for a simple yet effective questionnaire that collects the essential information. This would furthermore contribute to improving the design of treatments for a very vulnerable group in society. Moreover, collecting such data systematically would assist caseworkers in finding the appropriate balance between out-of-home placement versus preventive measures for the individual child.

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Appendix

A Children in care compared to all children not in care

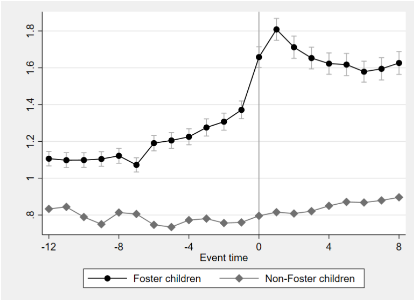
Here we present graphs showing descriptive difference between our sample of children in care as compared to all children who are not in care in our sample, matched only on gender and age. Graphs show the quarterly average of the two groups across event time. Non in care children are assigned a placebo event time.

Table A.1: Foster children vs non-foster children, quarter before first placement

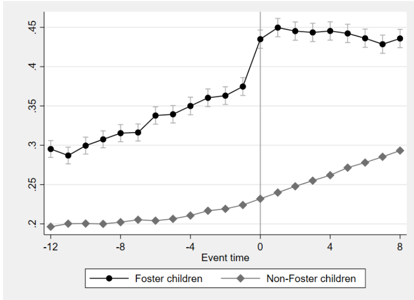
	Foster children	Non-foster children	Difference	
	mean	mean	b	t
Age	12.37	11.88	0.49***	(10.4)
Girl	0.48	0.49	-0.01	(-0.9)
Preventive care	0.31	0.00	0.31***	(302.4)
Number of GP visits	1.37	0.76	0.61***	(36.7)
Somatic hospital contact	0.15	0.07	0.08***	(24.7)
Psychiatric hospital contact	0.08	0.00	0.08***	(88.5)
Prescription drug purchase	0.37	0.22	0.15***	(29.6)
Criminal charge	0.14	0.01	0.14***	(67.1)
Enrolled in elementary school	0.92	0.99	-0.08***	(-55.6)
Absenteeism	0.17	0.05	0.12***	(75.5)
Mother's characteristics, measured at year of birth of the child				
Age	27.40	29.39	-1.99***	(-33.3)
Married/Registered partnership	0.32	0.50	-0.17***	(-28.6)
Highest completed elementary school	0.01	0.01	0.01***	(4.3)
Highest completed secondary education	0.81	0.60	0.21***	(35.6)
Highest completed tertiary education	0.08	0.34	-0.26***	(-45.5)
Employed	0.29	0.65	-0.36***	(-61.5)
Self-employed	0.01	0.02	-0.01***	(-6.0)
Unemployment benefits	0.05	0.02	0.03***	(16.8)
Education or health benefits	0.16	0.17	-0.01	(-1.5)
Early retirement benefits	0.03	0.00	0.03***	(33.7)
Retirement benefits	0.00	0.00	0.00**	(2.9)
On cash benefits	0.35	0.07	0.28***	(84.9)
Other	0.03	0.02	0.01***	(4.4)
On cash benefits	0.35	0.07	0.28***	(84.9)
Criminal charge	0.25	0.12	0.13***	(31.9)
Psychiatric hospital contact	0.12	0.03	0.09***	(42.2)
N	7,000	204,212	211,212	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

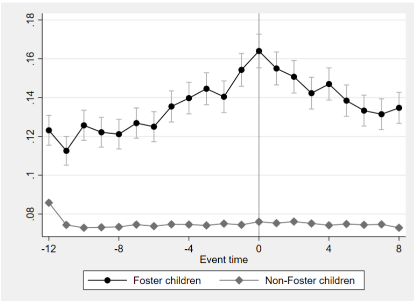
Figure A.1: Health, quarterly average



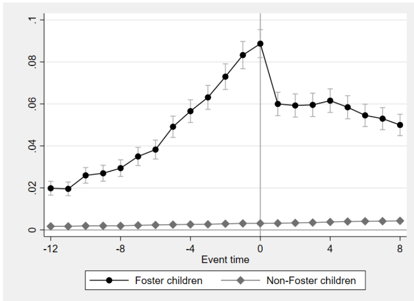
(a) Number of visits to general practitioner



(b) Share with prescription drug purchase



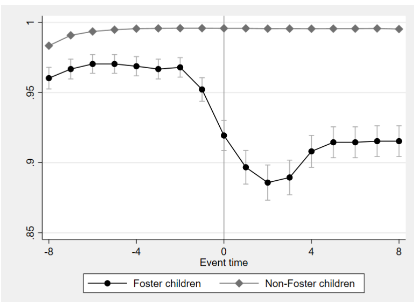
(c) Share with somatic hospitalization



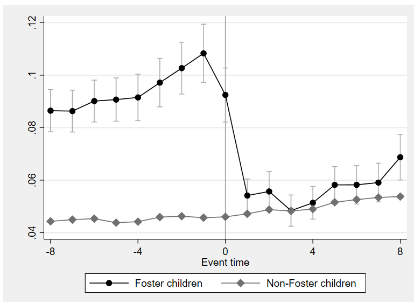
(d) Share with psychiatric hospitalization

Note: The figure shows the average outcomes separately for foster children and non-foster children matched only on age and gender.

Figure A.2: Schooling, quarterly average



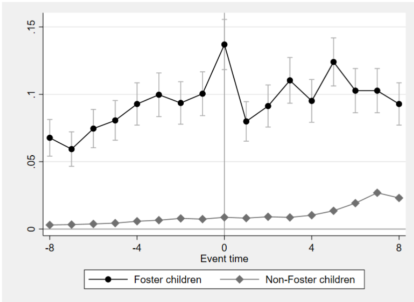
(a) Enrollment



(b) Absenteeism

Note: The figure shows the average outcomes separately for foster children and non-foster children matched only on age and gender, for school-age children (age 7-15).

Figure A.3: Juvenile crime, quarterly average



(a) Criminal charges

Note: The figure shows the average outcome separately for foster children and non-foster children matched only on age and gender, for children above the age of criminal responsibility (age 15 and above).

B Alternative estimation methods

Here we present 3 alternative event study estimation methods. The first alternative method is an estimation of the non-parametric event model using children placed in care at a later time as a control group. Adding the control group allows us to include unit fixed effects. The second alternative method is an estimation of the non-parametric event model on an unbalanced sample of children in care with unit fixed effects. In order to identify unit fixed effects without a control group, we must omit an additional pre-period dummy. The third alternative approach is an estimation of the non-parametric event model using the matched control group. Adding a control group to the estimation allows us to include unit fixed effects.

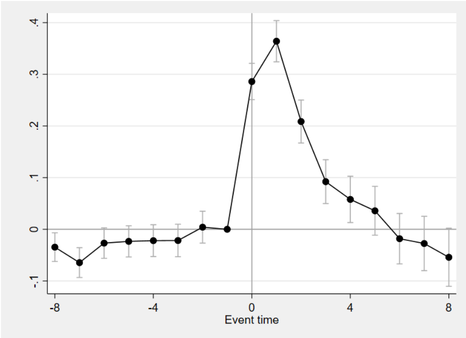
B.1 Event study with children placed in care 8 quarters later as control group

Table B.1: Foster children vs children placed in care 8 quarters later as control group, quarter before event

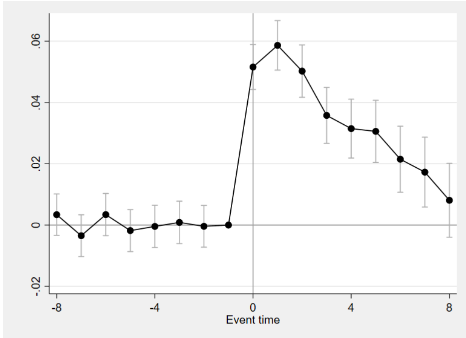
	Foster children	Non-foster children	Difference	
	mean	mean	b	t
Age	11.40	9.45	1.95***	(33.5)
Girl	0.47	0.48	-0.01	(-1.7)
Preventive care	0.28	0.13	0.15***	(29.7)
Number of GP visits	1.42	1.09	0.33***	(13.1)
Somatic hospital contact	0.17	0.12	0.05***	(11.2)
Psychiatric hospital contact	0.07	0.02	0.05***	(18.4)
Prescription drug purchase	0.36	0.28	0.07***	(12.4)
Criminal charge	0.14	1.00	-0.86***	(-18.1)
Enrolled in elementary school	0.91	0.96	-0.06***	(-15.3)
Absenteeism	0.16	0.10	0.07***	(19.2)
Mother's characteristics, measured at year of birth of the child				
Age	27.17	27.59	-0.42***	(-5.4)
Married/Registered partnership	0.30	0.33	-0.03***	(-5.5)
Highest completed elementary school	0.01	0.02	-0.00	(-1.7)
Highest completed secondary education	0.78	0.79	-0.02**	(-2.9)
Highest completed tertiary education	0.07	0.09	-0.02***	(-5.0)
Employed	0.25	0.30	-0.05***	(-8.9)
Self-employed	0.01	0.01	-0.00	(-0.7)
Unemployment benefits	0.05	0.05	0.01***	(3.3)
Education or health benefits	0.15	0.15	-0.01	(-1.3)
Early retirement benefits	0.03	0.03	0.00	(1.7)
Retirement benefits	0.00	0.00	-0.00	(-0.5)
On cash benefits	0.36	0.35	0.01	(1.5)
Other	0.04	0.03	0.00	(0.4)
On cash benefits	0.36	0.35	0.01	(1.5)
Criminal charge	0.27	0.26	0.01	(1.4)
Psychiatric hospital contact	0.14	0.14	0.01	(1.3)
N	18,590	9,888	28,478	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

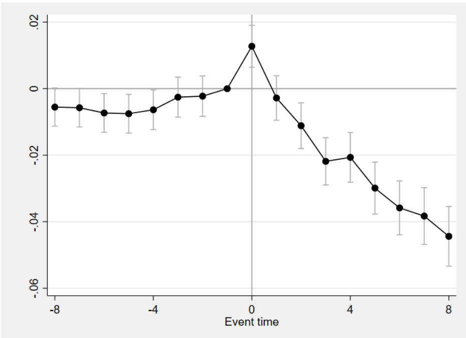
Figure B.1: Health



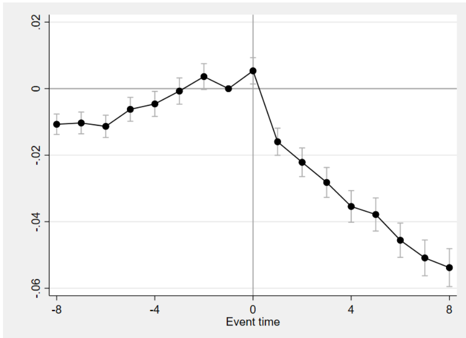
(a) Number of visits to general practitioner



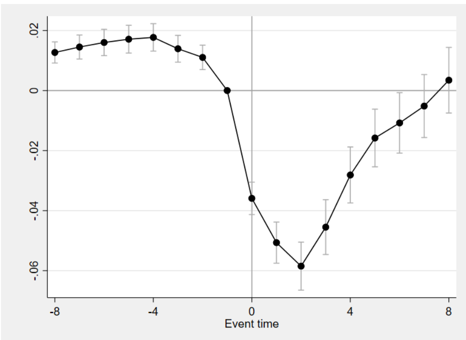
(b) Share with prescription drug purchase



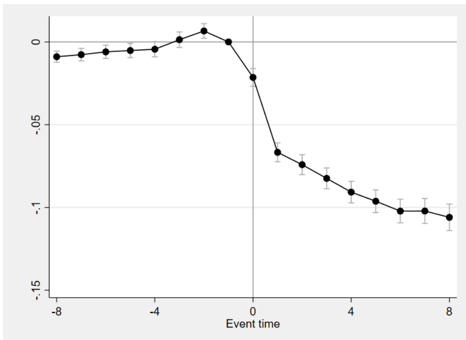
(c) Share with somatic hospitalization



(d) Share with psychiatric hospitalization



(e) Enrollment

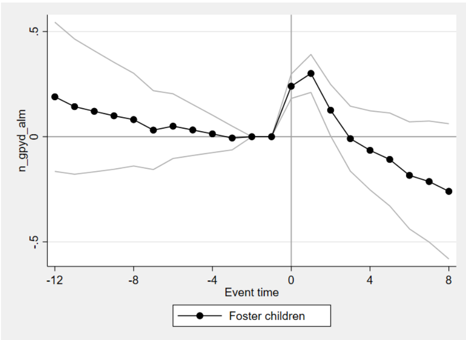


(f) Absenteeism

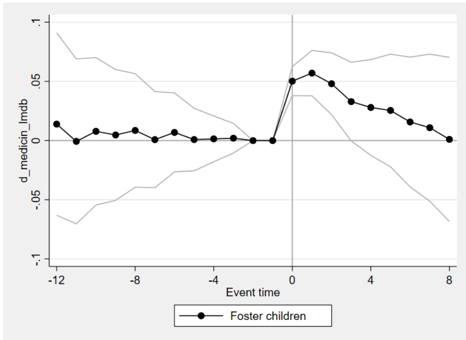
Note: The figure shows estimated event coefficients from model 2 with the group of children placed in care at a later (calendar) time as control group and unit fixed effects. The sample is unbalanced.

B.2 Event study with unit fixed effects

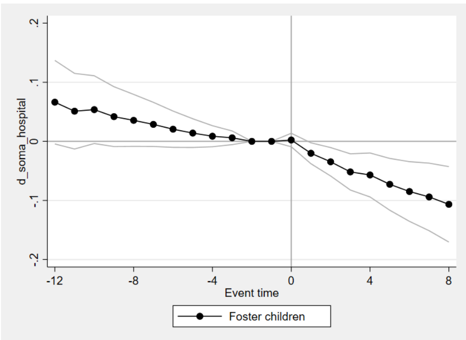
Figure B.2: Event coefficients, with unit fixed effects (no control group)



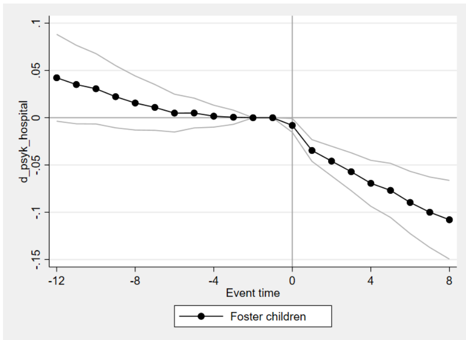
(a) Number of visits to general practitioner



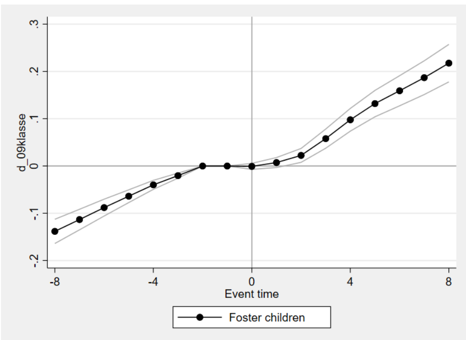
(b) Share with prescription drug purchase



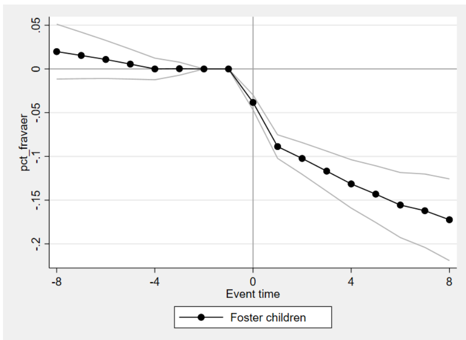
(c) Share with somatic hospitalization



(d) Share with psychiatric hospitalization



(e) Enrollment

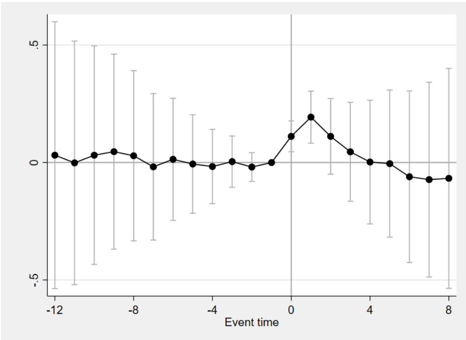


(f) Absenteeism

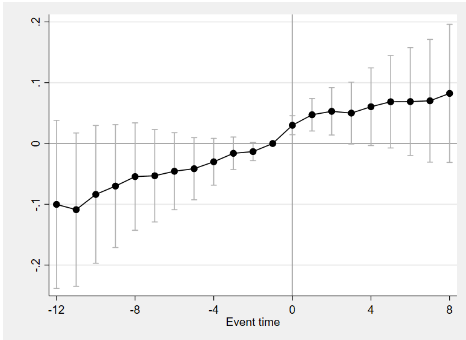
Note: The figure shows estimated event coefficients from model 2 with unit fixed effects and where pre-periods -1 and -2 are restricted to zero. The sample is unbalanced.

B.3 Event study with matched control group

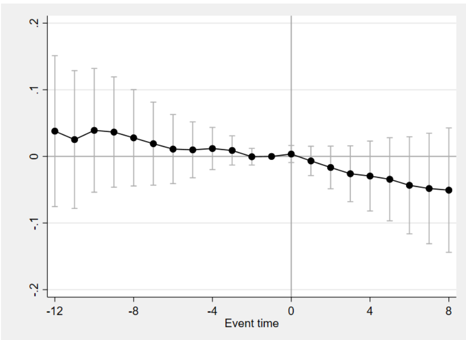
Figure B.3: Event coefficients, matched control group with unit fixed effects



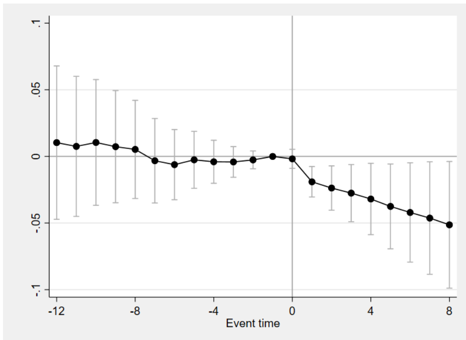
(a) Number of visits to general practitioner



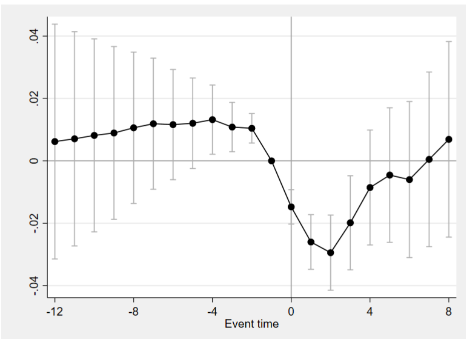
(b) Share with prescription drug purchase



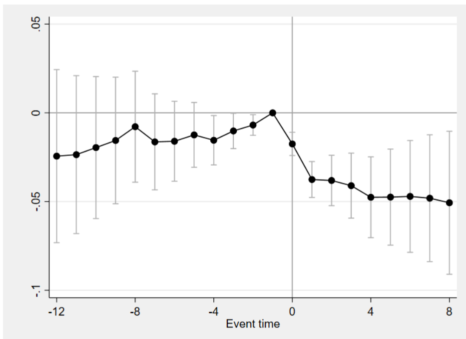
(c) Share with somatic hospitalization



(d) Share with psychiatric hospitalization



(e) Enrollment



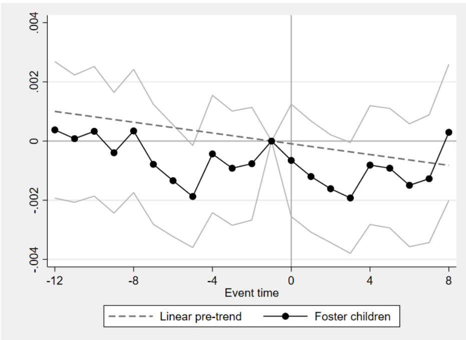
(f) Absenteeism

Note: The figure shows estimated event coefficients from model 2 with the matched children not in care as control group and unit fixed effects. The sample is unbalanced.

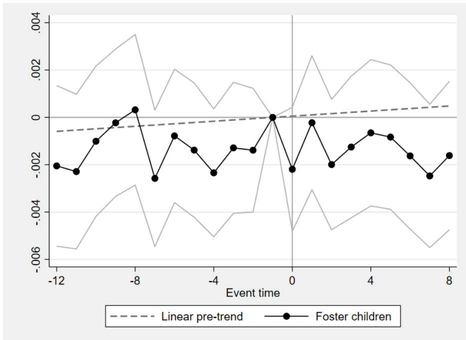
C Supplementary health evidence

Here we present supplementary evidence on health outcomes. For somatic hospitalizations we show results by 4 of the major diagnosis groups. The diagnosis groups are defined on the basis of the ICD10 diagnosis code and the specific ICD10 codes belonging to each group can be found in the notes to the figure. For psychiatric hospitalizations we show 3 of the largest diagnosis groups. For prescription drug purchases we show results for 4 of the largest drug groups.

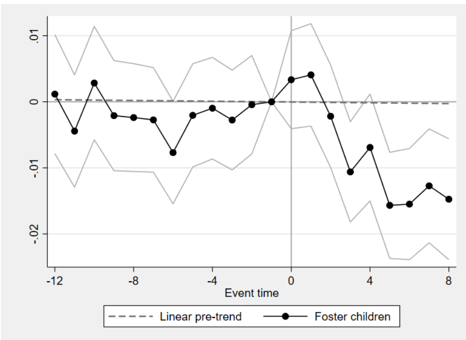
Figure C.1: Somatic hospitalizations by diagnosis group



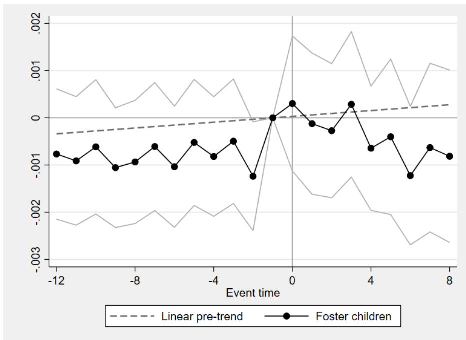
(a) Infections



(b) Respiratory diseases



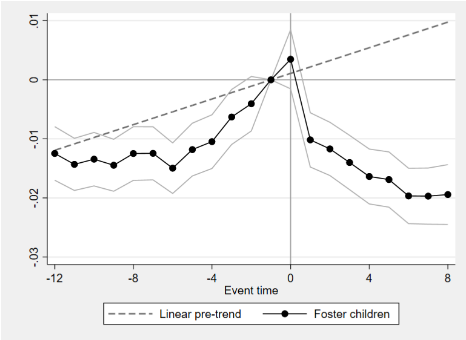
(c) Injury



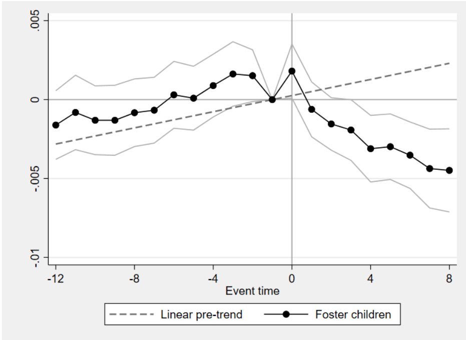
(d) Poisonings

Notes: ICD10 diagnosis codes in each of the diagnosis groups. Infections and parasites: A00-B99 , Respiratory diseases: J00-J99, Injuries: S00-T14, Poisonings: T15-T98.

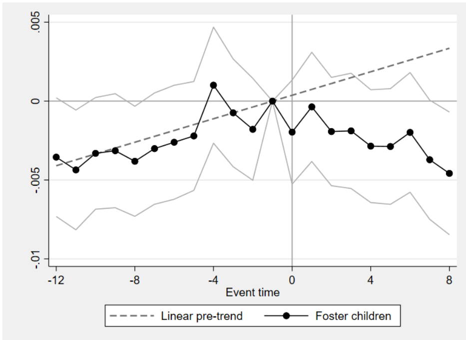
Figure C.2: Psychiatric hospitalizations by diagnosis group



(a) Reaction to severe stress, and adjustment disorders



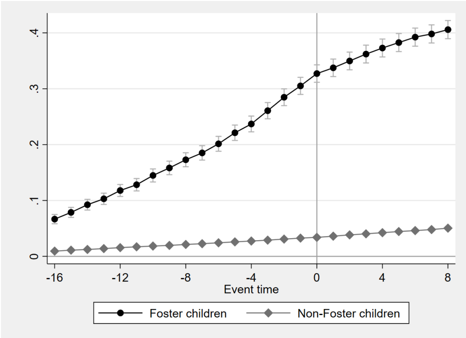
(b) Eating disorders



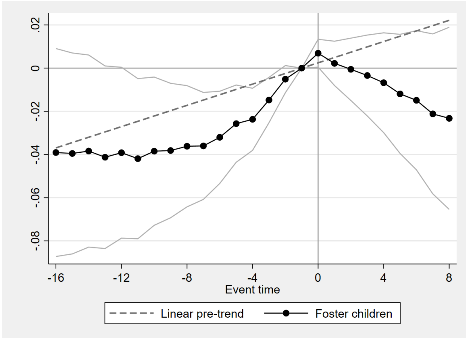
(c) Hyperkinetic disorders

Note: The diagnoses are categorized according to the ICD10 classification for mental and behavioral disorders. Here we show group F43 (Reaction to severe stress, and adjustment disorders), group F50 (Eating disorders) and group 90 (Hyperkinetic disorders).

Figure C.3: Cumulative psychiatric diagnoses



(a) Averages



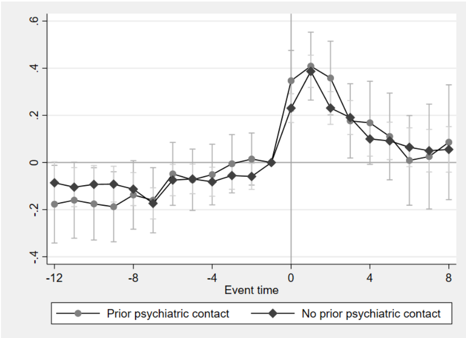
(b) Event coefficients

Table C.1: Survey treatment vs control children, no psychiatric hospitalization in t=0

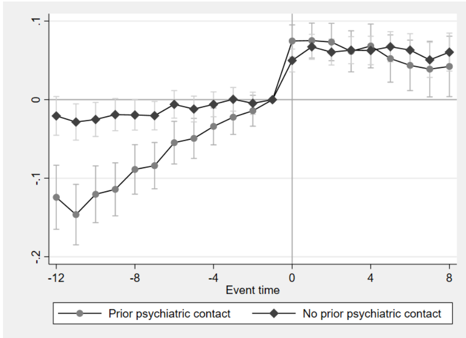
	Treatment mean	Control mean	Difference b	t
Average outcome in t=8				
Number of GP visits	1.33	1.01	0.32	(1.5)
Somatic hospital contact	0.17	0.12	0.05	(1.0)
Psychiatric hospital contact	0.04	0.02	0.02	(0.8)
Prescription drug purchase	0.35	0.30	0.05	(0.8)
Enrolled in elementary school	0.89	0.96	-0.07	(-1.2)
Absenteeism	0.08	0.10	-0.02	(-0.7)
Criminal charge	0.12	0.16	-0.03	(-0.6)
Average change in outcome from t=0 to t=8				
Δ Number of GP visits	0.23	-0.34	0.57*	(2.2)
Δ Somatic hospital contact	0.01	-0.06	0.07	(1.0)
Δ Psychiatric hospital contact	0.04	0.02	0.02	(0.8)
Δ Prescription drug purchase	0.07	-0.02	0.09	(1.4)
Δ Enrolled in elementary school	0.03	0.14	-0.11	(-1.2)
Δ Absenteeism	-0.04	0.02	-0.06	(-1.2)
Δ Criminal charge	-0.05	0.02	-0.07	(-0.9)
N	118	123	241	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

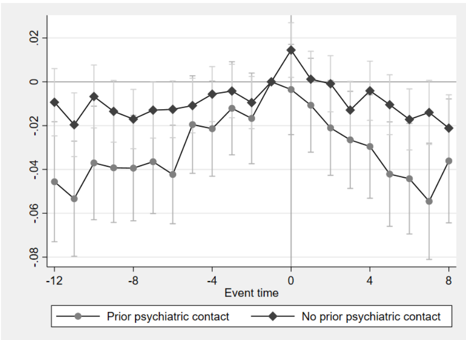
Figure C.4: Changes in outcomes for children with/without previous psychiatric diagnoses



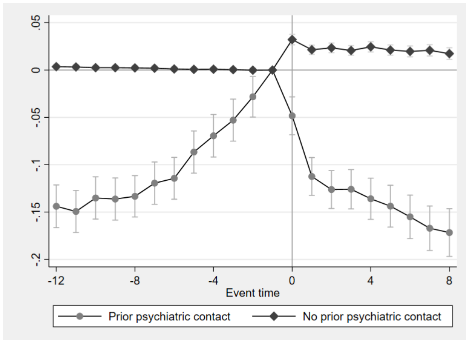
(a) Number of GP visits



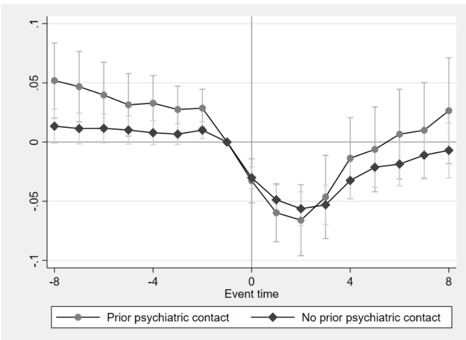
(b) Prescription drug purchases



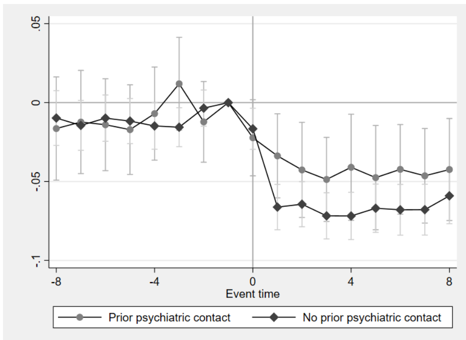
(c) Somatic hospitalizations



(d) Psychiatric hospitalizations

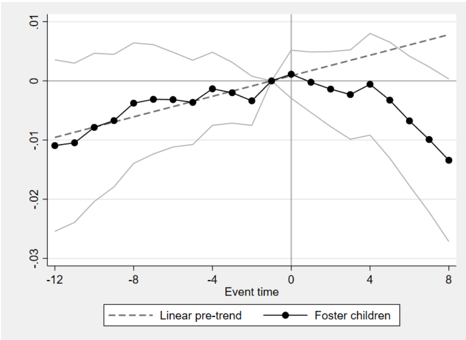


(e) Enrollment

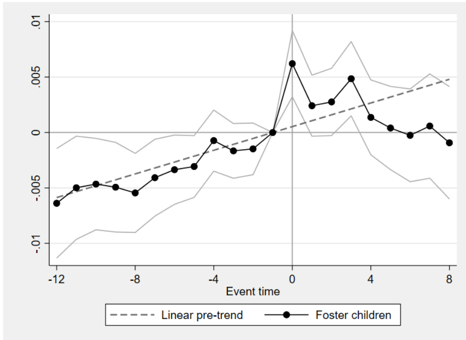


(f) Absenteeism

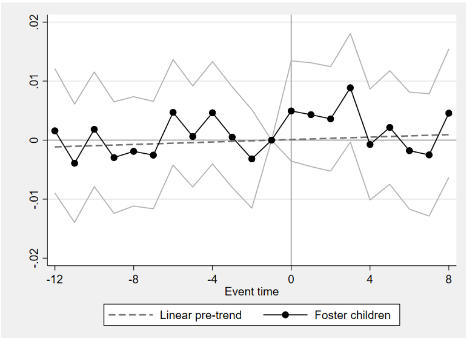
Figure C.5: Prescription drug purchases by drug group



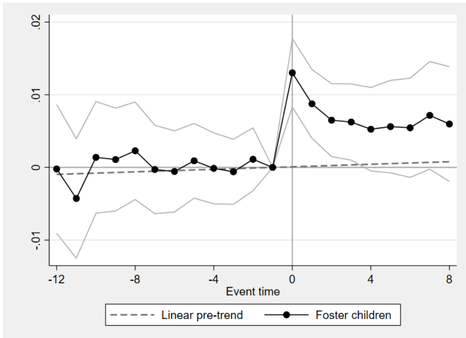
(a) ADHD treatment



(b) Benzodiazepines and related drugs



(c) Antibiotics

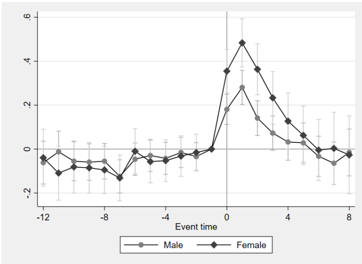


(d) Astma

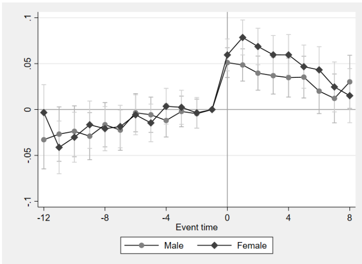
Notes: The drugs are categorized according to groups defined by the Danish Health Data Authorities (Sundhedsdatastyrelsen). The groups are defined based on the WHO ATC codes. The specific ATC codes can be found on medstat.dk. Asthma medication includes ATC codes R03. Antibiotics include ATC codes J01. ADHD medication includes ATC codes CO2AC01, N06BA01, N06BA04, N06BA09, N06BA12. Benzodiazepines and related drugs include ATC codes N05BA, N05CD, N03AE, N05CF.

D Heterogeneity by sex and age

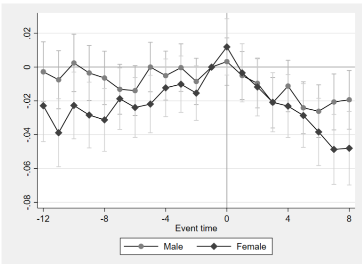
Figure D.1: Heterogeneity by sex



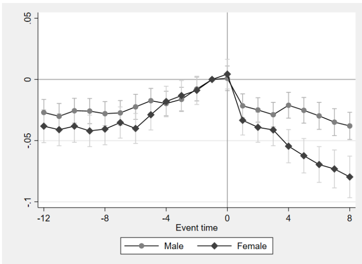
(a) GP visits



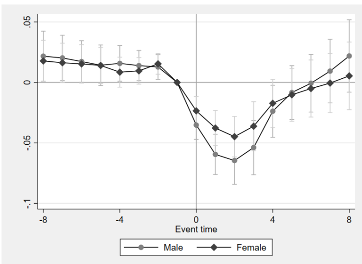
(b) Prescription drug purchase



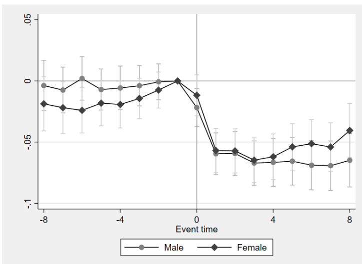
(c) Somatic hospitalization



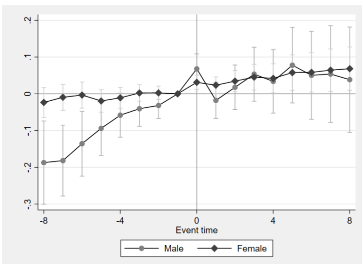
(d) Psychiatric hospitalization



(e) Enrollment



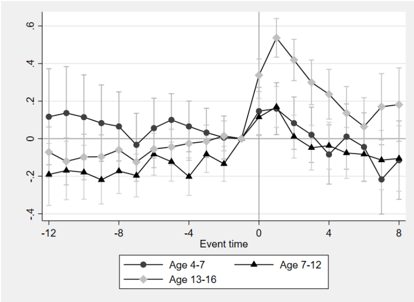
(f) Absenteeism



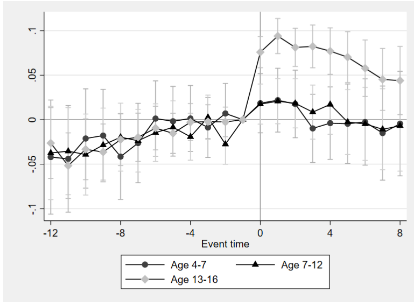
(g) Criminal charges

Note: The figures show the level changes in outcomes relative to period t=-1 separately for boys and girls.

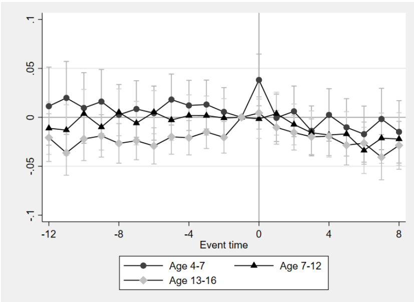
Figure D.2: Heterogeneity by age



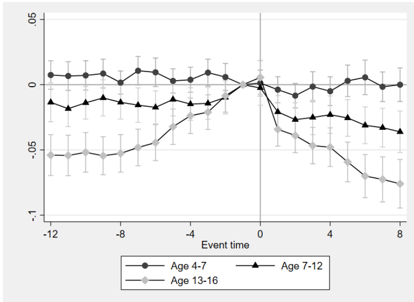
(a) GP visits



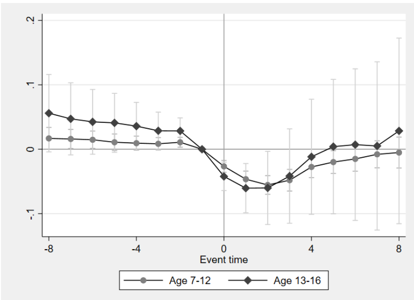
(b) Prescription drug purchase



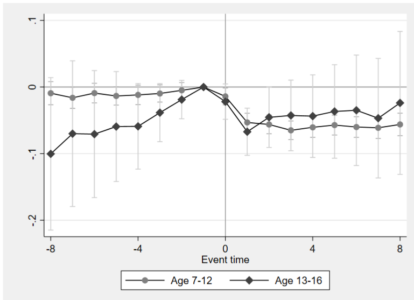
(c) Somatic hospitalization



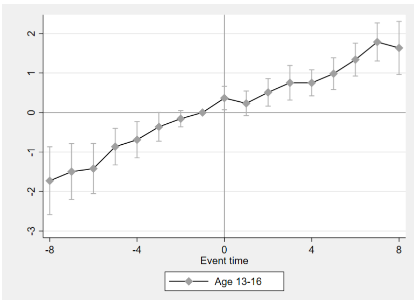
(d) Psychiatric hospitalization



(e) Enrollment



(f) Absenteeism

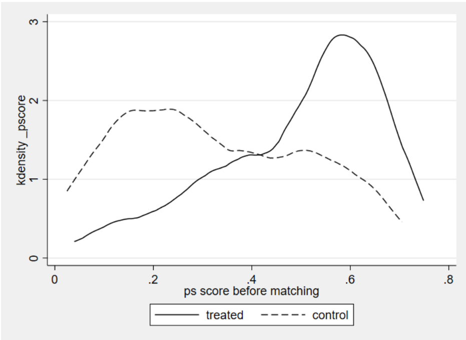


(g) Criminal charges

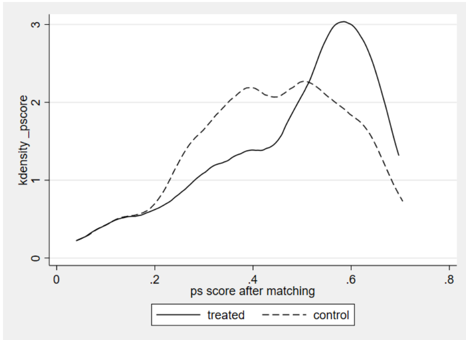
Note: The figures show the level changes in outcomes relative to period t=-1 separately by age group at the time of placement in out-of-home care.

E Supplementary survey evidence

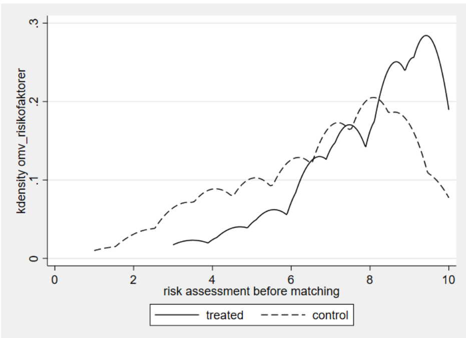
Figure E.1: Common Support



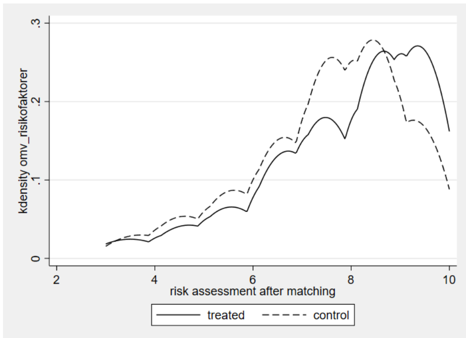
(a) Propensity score distribution, before matching



(b) Propensity score distribution, after matching



(c) Risk assessment distribution, before matching



(d) Risk assessment distribution, after matching

Note: The figure shows kernel density plots of the sample distributions before and after matching. The upper two panels show the estimated propensity score and the lower two panels the caseworker risk assessment. The matching procedure imposes a common support restriction on the propensity score and any observations not on common support are dropped from the matched sample.

Table E.1: Survey regression estimates, post-periods pooled, unmatched sample

	(1) Number of GP visits	(2) Somatic hospital contact	(3) Psychiatric hospital contact	(4) Prescription drug purchase	(5) Enrolled in elementary school	(6) Absenteeism	(7) Criminal charge
Placed in out-of-home care	0.270	0.0101	0.00460	0.0690	-0.0338	-0.00743	-0.00845
	(1.94)	(0.47)	(0.28)	(1.90)	(-0.59)	(-0.31)	(-0.26)
Age at time of survey	0.0251*	0.000901	0.00464***	0.0159***	-0.0165*	0.0111***	-0.000510
	(2.06)	(0.42)	(3.58)	(5.00)	(-2.13)	(3.65)	(-0.05)
Girl	0.415***	-0.0126	0.00941	0.0925**	0.0725	-0.0248	-0.110***
	(3.53)	(-0.68)	(0.70)	(3.04)	(1.84)	(-1.38)	(-3.57)
Caseworker risk assessment	0.0236	0.00971	0.00462	0.0135	-0.00841	0.00408	0.0223**
	(0.81)	(1.82)	(1.35)	(1.88)	(-0.70)	(1.04)	(3.05)
Mother's characteristics measured at year of birth of the child							
Age	-0.0276*	-0.00247	-0.000959	-0.00655*	0.00392	-0.00152	-0.000640
	(-2.41)	(-1.34)	(-0.81)	(-2.33)	(0.88)	(-0.92)	(-0.21)
Married/Registered partnership	-0.205	-0.0284	-0.0185	-0.0824*	0.0735	0.0175	0.00434
	(-1.55)	(-1.44)	(-1.23)	(-2.39)	(1.72)	(0.86)	(0.14)
Highest completed elementary school	0.201	0.0102	0.00867	0.0990	-0.125	-0.00660	-0.0475
	(0.90)	(0.21)	(0.42)	(1.27)	(-1.39)	(-0.18)	(-0.55)
Highest completed secondary education	0.478**	0.0237	0.0201	0.171***	0.0322	-0.0262	-0.0362
	(2.84)	(0.85)	(1.47)	(3.70)	(0.48)	(-1.05)	(-0.90)
Highest completed tertiary education	0.956***	0.0244	0.0763**	0.248***	-0.0683	-0.0345	-0.0889
	(4.12)	(0.69)	(2.85)	(4.40)	(-0.66)	(-0.93)	(-1.77)
Unemployed	-0.0238	-0.0217	0.0415	-0.0427	0.104*	0.0737*	-0.0168
	(-0.08)	(-0.57)	(0.85)	(-0.56)	(2.22)	(2.12)	(-0.51)
On early retirement	1.133*	0.0458	0.00783	0.249*	0	0	-0.0874*
	(2.49)	(1.06)	(0.42)	(2.36)	(.)	(.)	(-2.29)
On cash benefits	-0.268*	-0.00174	0.00353	-0.0742*	0.0331	-0.00578	0.0511
	(-2.06)	(-0.08)	(0.28)	(-2.23)	(0.74)	(-0.34)	(1.47)
Criminal charge	0.0342	-0.00566	0.0181	0.00955	0.0180	0.0370*	0.0221
	(0.23)	(-0.26)	(1.11)	(0.26)	(0.38)	(2.29)	(0.48)
Psychiatric hospital contact	0.0363	-0.0215	-0.0111	-0.0175	-0.114	-0.00157	-0.0377
	(0.23)	(-0.85)	(-1.11)	(-0.42)	(-1.74)	(-0.09)	(-0.49)
Constant	0.975**	0.135	-0.0502	0.0512	0.936***	0.0241	0.0637
	(2.64)	(1.88)	(-1.09)	(0.50)	(5.30)	(0.41)	(0.33)
Observations	2786	2786	2786	2786	1051	613	1402

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E.2: Survey regression estimates, change with respect to t=0, unmatched sample

	(1) Δ Number of GP visits	(2) Δ Somatic hospital contact	(3) Δ Psychiatric hospital contact	(4) Δ Prescription drug purchase	(5) Δ Enrolled in elementary school	(6) Δ Absenteeism	(7) Δ Criminal charge
Placed in out-of-home care	0.488*	0.0199	-0.0561*	0.0881	-0.103	-0.0106	-0.00222
	(2.50)	(0.46)	(-2.11)	(1.95)	(-1.62)	(-0.18)	(-0.04)
Constant	-0.240	-0.0316	0.00836	-0.0304	0.0713*	0.00811	0.00539
	(-1.75)	(-1.24)	(0.61)	(-1.10)	(2.15)	(0.60)	(0.14)
Observations	2786	2786	2786	2786	948	459	1188

t statistics in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E.3: Survey regression estimates, post-periods pooled, matched sample

	(1) Number of GP visits	(2) Somatic hospital contact	(3) Psychiatric hospital contact	(4) Prescription drug purchase	(5) Enrolled in elementary school	(6) Absenteeism	(7) Criminal charge
Placed in out-of-home care	0.333*	0.0158	0.00503	0.0727	-0.0239	-0.00562	-0.00707
	(2.29)	(0.71)	(0.30)	(1.91)	(-0.44)	(-0.23)	(-0.21)
Age at time of survey	0.0550***	0.00449	0.00443**	0.0208***	-0.0181*	0.0164***	-0.00104
	(3.45)	(1.43)	(2.92)	(5.04)	(-2.07)	(3.67)	(-0.09)
Girl	0.612***	-0.00481	0.0167	0.135***	0.0741	-0.0488	-0.112***
	(4.31)	(-0.22)	(1.05)	(3.63)	(1.53)	(-1.91)	(-3.41)
Caseworker risk assessment	0.0450	0.0163**	0.00932**	0.0150	0.00832	0.00558	0.0231*
	(1.17)	(2.63)	(2.73)	(1.47)	(0.43)	(0.72)	(2.40)
Mother's characteristics measured at year of birth of the child							
Age	-0.0330**	-0.00345	-0.000677	-0.00875**	0.00257	-0.000489	-0.000609
	(-2.63)	(-1.66)	(-0.49)	(-2.85)	(0.49)	(-0.22)	(-0.18)
Married/Registered partnership	-0.340*	-0.0326	-0.0282	-0.0925*	0.0768	0.0339	0.00176
	(-2.21)	(-1.40)	(-1.64)	(-2.21)	(1.49)	(1.15)	(0.05)
Highest completed elementary school	0.413	0.0487	0.00679	0.130	-0.110	0.0106	-0.0354
	(1.73)	(0.97)	(0.32)	(1.54)	(-1.09)	(0.26)	(-0.40)
Highest completed secondary education	0.590**	0.0467	0.0173	0.218***	0.0503	-0.0112	-0.0227
	(3.07)	(1.56)	(1.10)	(4.19)	(0.61)	(-0.35)	(-0.52)
Highest completed tertiary education	1.273***	0.0476	0.0937*	0.334***	-0.151	0.0464	-0.0757
	(4.34)	(1.15)	(2.42)	(4.75)	(-1.20)	(0.71)	(-1.38)
Unemployed	-0.122	-0.0120	0.0195	-0.0154	0.120*	0.0864	-0.0599*
	(-0.34)	(-0.23)	(0.45)	(-0.16)	(2.08)	(1.86)	(-2.16)
On early retirement	0.460	0.103*	0.0129	0.263*	0	0	-0.0861*
	(1.25)	(2.33)	(0.54)	(2.22)	(.)	(.)	(-2.06)
On cash benefits	-0.316*	-0.0148	0.0129	-0.0951*	0.0561	0.00813	0.0534
	(-2.08)	(-0.60)	(0.85)	(-2.44)	(1.19)	(0.37)	(1.41)
Criminal charge	0.0326	-0.00630	0.00403	-0.00896	0.0177	0.0378	0.00942
	(0.18)	(-0.24)	(0.23)	(-0.19)	(0.31)	(1.67)	(0.20)
Psychiatric hospital contact	-0.0282	-0.00488	-0.0138	-0.0191	-0.102	0.0151	-0.0132
	(-0.15)	(-0.15)	(-1.00)	(-0.36)	(-1.26)	(0.71)	(-0.15)
Constant	0.398	0.0367	-0.0941	-0.00812	0.815**	-0.0922	0.0584
	(0.83)	(0.41)	(-1.64)	(-0.06)	(2.99)	(-0.81)	(0.27)
Observations	2087	2087	2087	2087	821	426	1265

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E.4: Survey regression estimates, change with respect to t=0, post-periods pooled, matched sample

	(1) Δ Number of GP visits	(2) Δ Somatic hospital contact	(3) Δ Psychiatric hospital contact	(4) Δ Prescription drug purchase	(5) Δ Enrolled in elementary school	(6) Δ Absenteeism	(7) Δ Criminal charge
Placed in out-of-home care	0.428*	0.0318	-0.0459	0.0822	-0.0994	-0.0130	0.00529
	(2.02)	(0.65)	(-1.45)	(1.71)	(-1.45)	(-0.22)	(0.09)
Constant	-0.163	-0.0394	0.00288	-0.0125	0.0675	0.0105	-3.69e-17
	(-1.03)	(-1.19)	(0.14)	(-0.39)	(1.63)	(0.56)	(-0.00)
Observations	2087	2087	2087	2087	759	332	1076

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

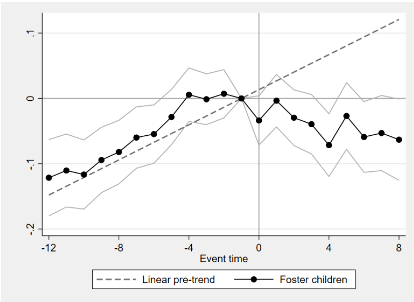
F First preventive care measure

Here we present results in the same way as the main results in the paper, but where we have defined the event to be the first preventive care measure, rather than the first out-of-home care placement. For some children, this event coincides with their first out-of-home care placement, but other children are never placed in out-of-home care and only ever receive preventive care measures.

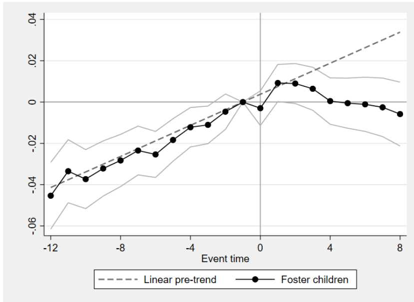
Table F.1: Foster children at time of first preventive care measure, balanced samples

	Health sample		Enrolment sample		Absenteeism sample		Crime sample	
	mean	(sd)	mean	(sd)	mean	(sd)	mean	(sd)
Age	13.46	(4.1)	11.35	(1.4)	11.11	(1.4)	17.83	(1.1)
Girl	0.45	(0.5)	0.39	(0.5)	0.39	(0.5)	0.49	(0.5)
Placed in FC with no prior preventive care	0.08	(0.3)	0.03	(0.2)	0.03	(0.2)	0.17	(0.4)
Quarters from first preventive care to first out-of-home care placement	6.59	(5.0)	8.41	(5.9)	9.34	(6.2)	1.65	(0.7)
Placement ongoing	0.10	(0.3)	0.26	(0.4)	0.29	(0.5)	0.01	(0.1)
Legal action								
Placement with consent	0.86	(0.4)	0.80	(0.4)	0.86	(0.4)	0.93	(0.3)
Placement without consent	0.07	(0.3)	0.09	(0.3)	0.07	(0.3)	0.01	(0.1)
Urgent placement	0.02	(0.2)	0.11	(0.3)	0.07	(0.3)	0.00	(0.0)
Other	0.05	(0.2)	0.00	(0.0)	0.00	(0.0)	0.06	(0.2)
Placement Type								
Foster family care	0.21	(0.4)	0.40	(0.5)	0.56	(0.5)	0.05	(0.2)
Kinship care	0.06	(0.2)	0.13	(0.3)	0.25	(0.4)	0.00	(0.0)
Group home	0.14	(0.4)	0.15	(0.4)	0.00	(0.0)	0.08	(0.3)
Institutional care	0.27	(0.4)	0.27	(0.4)	0.19	(0.4)	0.16	(0.4)
Independent living	0.32	(0.5)	0.04	(0.2)	0.00	(0.0)	0.72	(0.5)
Length of placement								
Duration, years	1.02	(1.1)	2.13	(2.1)	3.19	(2.3)	0.55	(0.4)
Spell duration, years	2.37	(3.1)	3.94	(3.9)	4.83	(4.5)	1.98	(2.9)
Reason for placement								
Child risk/externalizing behavior	0.89	(0.3)	0.82	(0.4)	0.79	(0.4)	0.91	(0.3)
Child health concerns	0.20	(0.4)	0.23	(0.4)	0.14	(0.4)	0.19	(0.4)
Abuse/neglect of child	0.52	(0.5)	0.64	(0.5)	0.79	(0.4)	0.47	(0.5)
Adult risk/externalizing behavior	0.48	(0.5)	0.50	(0.5)	0.71	(0.5)	0.59	(0.5)
Other	0.27	(0.4)	0.27	(0.4)	0.21	(0.4)	0.16	(0.4)
Share of reasons due to child	0.51	(0.3)	0.43	(0.3)	0.34	(0.3)	0.49	(0.2)
Share of reason due to parents	0.49	(0.3)	0.57	(0.3)	0.66	(0.3)	0.51	(0.2)
At end of first placement								
Exit before age 18	0.34	(0.5)	0.44	(0.5)	0.40	(0.5)	0.09	(0.3)
New placement	0.13	(0.3)	0.28	(0.5)	0.10	(0.3)	0.05	(0.2)
Continued care after age 18	0.01	(0.1)	0.00	(0.0)	0.00	(0.0)	0.00	(0.0)
Age out	0.52	(0.5)	0.28	(0.5)	0.50	(0.5)	0.86	(0.4)
N	12,995		4,790		1,887		4,434	

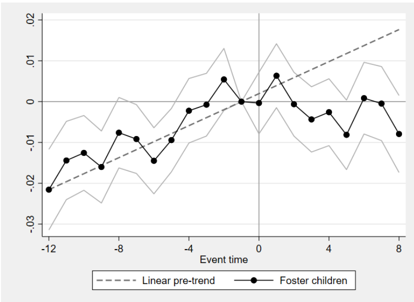
Figure F.1: Health



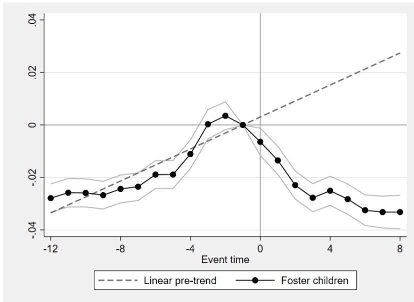
(a) Number of visits to general practitioner



(b) Share with prescription drug purchase



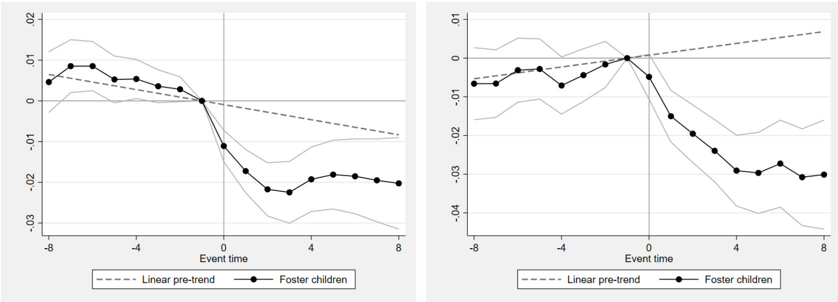
(c) Share with somatic hospitalization



(d) Share with psychiatric hospitalization

Note: The figure shows the estimated event coefficients from model 2 and the pre-trend from model 3, estimated for the balanced health sample of children in preventive care.

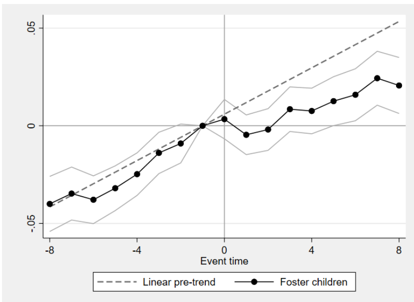
Figure F.2: Schooling



(a) Enrollment (b) Absenteeism

Note: The figure shows the estimated event coefficients from model 2 and the pre-trend from model 3, estimated for the balanced school sample of children in preventive care. Absenteeism is conditional on enrollment.

Figure F.3: Juvenile crime



(a) Juvenile crime

Note: The figure shows the estimated event coefficients from model 2 and the pre-trend from model 3, estimated for the balanced crime sample of children in preventive care.

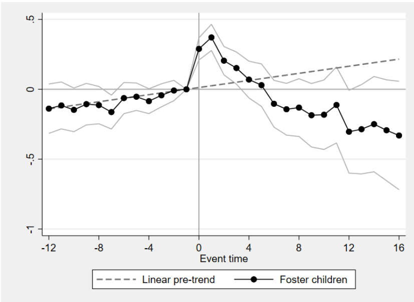
G Longer term effects - 16 quarters

Here we show results similar to the main results in the paper, but where we have extended the post-event period from 8 quarters to 16 quarters to look at the changes over the longer run at the cost of a reduced sample size.

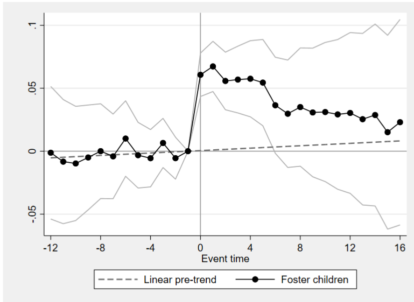
Table G.1: Foster children at time of first placement, balanced samples

	Health sample		Enrolment sample		Absenteeism sample		Crime sample	
	mean	(sd)	mean	(sd)	mean	(sd)	mean	(sd)
Age	12.64	(4.0)	10.14	(0.8)	10.06	(0.8)	16.99	(0.1)
Girl	0.48	(0.5)	0.40	(0.5)	0.46	(0.5)	0.48	(0.5)
Placed in FC with no prior preventive care	0.56	(0.5)	0.54	(0.5)	0.50	(0.5)	0.53	(0.5)
Quarters from first preventive care to first out-of-home care placement	6.67	(4.4)	6.54	(4.2)	6.75	(4.2)	5.84	(3.8)
Placement ongoing	0.21	(0.4)	0.51	(0.5)	0.57	(0.5)	0.00	(0.0)
Legal action								
Placement with consent	0.85	(0.4)	0.78	(0.4)	0.73	(0.4)	0.90	(0.3)
Placement without consent	0.10	(0.3)	0.17	(0.4)	0.19	(0.4)	0.03	(0.2)
Urgent placement	0.03	(0.2)	0.05	(0.2)	0.07	(0.2)	0.01	(0.1)
Other	0.03	(0.2)	0.01	(0.1)	0.01	(0.1)	0.07	(0.3)
Placement Type								
Foster family care	0.28	(0.4)	0.46	(0.5)	0.53	(0.5)	0.06	(0.2)
Kinship care	0.07	(0.2)	0.07	(0.3)	0.09	(0.3)	0.02	(0.1)
Group home	0.20	(0.4)	0.12	(0.3)	0.06	(0.2)	0.19	(0.4)
Institutional care	0.31	(0.5)	0.34	(0.5)	0.32	(0.5)	0.29	(0.5)
Independent living	0.15	(0.4)	0.01	(0.1)	0.00	(0.0)	0.44	(0.5)
Length of placement								
Duration, years	1.56	(1.3)	2.30	(1.9)	1.79	(1.8)	0.61	(0.3)
Spell duration, years	1.96	(1.4)	2.94	(2.0)	2.54	(2.1)	0.67	(0.3)
Reason for placement								
Child risk/externalizing behavior	0.81	(0.4)	0.78	(0.4)	0.71	(0.5)	0.85	(0.4)
Child health concerns	0.37	(0.5)	0.43	(0.5)	0.38	(0.5)	0.37	(0.5)
Abuse/neglect of child	0.60	(0.5)	0.69	(0.5)	0.73	(0.4)	0.42	(0.5)
Adult risk/externalizing behavior	0.53	(0.5)	0.62	(0.5)	0.62	(0.5)	0.48	(0.5)
Other	0.27	(0.4)	0.25	(0.4)	0.28	(0.5)	0.32	(0.5)
Share of reasons due to child	0.51	(0.3)	0.46	(0.3)	0.42	(0.3)	0.60	(0.3)
Share of reason due to parents	0.49	(0.3)	0.54	(0.3)	0.58	(0.3)	0.40	(0.3)
At end of first placement								
Exit before age 18	0.36	(0.5)	0.66	(0.5)	0.58	(0.5)	0.13	(0.3)
New placement	0.15	(0.4)	0.30	(0.5)	0.40	(0.5)	0.03	(0.2)
Continued care after age 18	0.00	(0.1)	0.00	(0.0)	0.00	(0.0)	0.00	(0.0)
Age out	0.48	(0.5)	0.05	(0.2)	0.01	(0.1)	0.84	(0.4)
N	3,553		730		195		803	

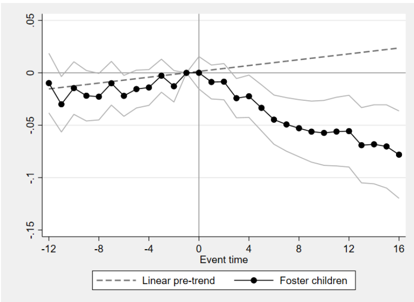
Figure G.1: Health



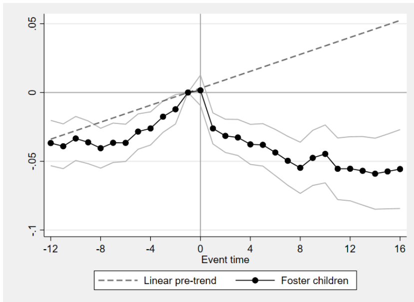
(a) Number of visits to general practitioner



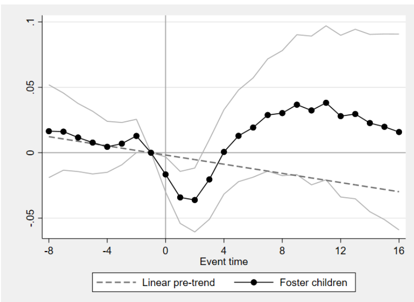
(b) Share with prescription drug purchase



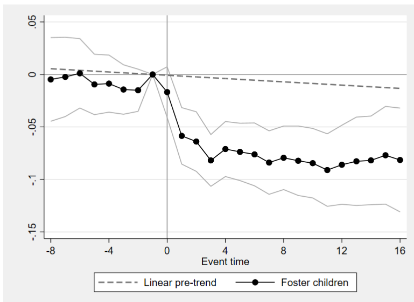
(c) Share with somatic hospitalization



(d) Share with psychiatric hospitalization



(e) Enrollment



(f) Absenteeism

Note: The figure shows the estimated event coefficients from model 2 and the pre-trend from model 3, estimated for the balanced sample of foster children who are observed in a post-period of 16 quarters following their first out-of-home care placement.

Chapter 2

Economic causes and consequences of foster parenting

Economic causes and consequences of foster parenting

Petra Gram Cavalca *

July 29, 2021

Abstract

Many child protection systems experience recurring shortages of foster parents for children in out-of-home care. This paper investigates the role of employment and earnings in selection into foster parenting and how the choice to become foster parents is subsequently reflected in household earnings and employment. I set up a simple theoretical model for foster parents' labor market supply that shows that foster parents are more likely to have a lower wage in the regular labor market or a higher intrinsic motivation for foster parenting. This gives rise to a potential quantity-quality trade-off when setting the compensation rate. I empirically investigate whether there are any signs of adverse selection into foster parenting at the current compensation rate, in particular I look for an Ashenfelter's dip in employment and earnings in the months leading up to foster parenting and find no sign of adverse selection due to economic circumstance. Using an event study approach, I look at how the choice to foster parent is reflected in employment and earnings and find that foster parents decrease their regular labor market supply when they start foster parenting. The resulting drop in labor market earnings is, however, more than offset by the foster care compensation, resulting in a yearly net increase of 16 percent in total household earnings.

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1 Introduction

A well-functioning child protection system is important for the large number of children in out-of-home care each year. In the US, as in Denmark, around 5-6 percent of children are placed in out-of-home care at some point during their childhood (Doyle and Aizer, 2018, Ejrnæs and Gørtz, 2017). In the last decade there has been an increasing desire to place children in family foster care rather than institutional care. This development is partly based on a belief that children profit more from entering everyday family life in a foster family compared to an institutional setting, but can also be rationalized from a cost-saving perspective, as institutional care is up to twice as costly as family foster care (Cavalca, Ejrnæs, and Gørtz, 2021). The move towards family-based care naturally increases the importance of foster families in the child protection system because more families are needed, and also because the children who were previously been placed in institutional care tend to have more complex needs, an they now have to be accommodated in a family setting. Child protection systems in most countries experience recurring shortages of foster parents and the shift towards more family-based care will only increase demand for high-quality foster parents in the future.

Foster parents often receive financial compensation for their work as foster parents beyond compensation for the direct expenses related to the foster child, such as food and clothing expenses. The foster care compensation rate is one of the few policy tools available to regulate the supply of foster parents. Existing literature confirms that increasing compensation has the potential to increase the supply of foster parents (Doyle and Peters, 2007, Doyle, 2007, Duncan and Argys, 2007), but we know little about how increased compensation might affect the quality of care provided.

In this paper I raise the important question of whether increasing the compensation to foster parents may impact on the quality of the families who select into foster parenting. I present a theoretical model of foster parents labor market supply. The model illustrates a potential trade-off between the compensation rate and the quality of families who select into the foster care market. I model quality as intrinsic motivation for foster parenting, which consists of a combination of the indirect utility foster parents gain from the foster child's utility and their generosity in sharing family consumption with the foster child. The model highlights that to select into foster parenting, families must have either high intrinsic motivation for foster parenting or a low wage in the regular labor market. This raises the concern about adverse selection into foster parenting based on economic circumstance at the cost of skills or intrinsic motivation for caring for at-risk children. In other words, I show that there is a potential quantity-quality trade-off in setting the compensation rate for foster parents.

The idea that a wage increase may decrease quality goes against traditional efficiency wage theory, but there is a small theoretical literature on the potential for increases in wages to crowd out intrinsic motivation in some circumstances (first brought into economic theory by Frey and Oberholzer-Gee (1997)). The literature has focused on care work in other sectors, in particular in the context of nursing (Taylor, 2007; Heyes, 2005). The inefficiency result is disputed and depends on the way one models intrinsic motivation and productivity (Fedele, 2018; Barigozzi and Turati, 2012). My model reflects this ambiguity, since increased compensation not only decreases selection due to intrinsic motivation, but also allows for the recruitment of families with higher wages in the regular market. To the extent that outside wages reflect productivity in foster parenting, this may be another important aspect of measuring quality of foster parents.

I proceed to investigate the ambiguous relationship between foster care compensation and quality of care in the empirical part of the paper. I answer two related research questions. Are there signs of adverse selection due to economic circumstance in the foster parents we currently recruit? How is the choice to become foster parents reflected in subsequent household employment and earnings? Answering these questions is an important first step in understanding whether it is possible to increase compensation without sacrificing quality of care. I use detailed full-population Danish administrative data to identify close to 6,000 foster families and their employment and earnings over time.

The empirical approach consists of two steps. In the first step, I estimate a logistic regression model of the selection into foster parenting. The model identifies important predictors of becoming foster parents, such as level of education, field of study and earnings and employment trajectories of both parents in the time leading up to becoming foster parents. The highly detailed monthly data on employment and earnings allow me to look for adverse selection due to economic circumstances in the time leading up to foster parenting. In particular, I look for signs of a so-called Ashenfelter's dip in employment or earnings in the months leading up to foster parenting (Ashenfelter, 1978). Based on the estimated probability of becoming foster parents from the logistic model, I identify a matched control group of families who do not foster but who have similar characteristics and labor market trajectories as the foster families.

The second part of the empirical approach consists of an event study of a balanced sample of foster parents across an 11-year window. I trace out how the choice to become foster parents is reflected in the employment and earnings trajectory of the foster families relative to the matched control group.

While the two groups likely differ on unobserved characteristics, their common labor market trajectory in the time leading up to the choice of becoming foster parents makes them representative of the potential counterfactual labor market trajectory for foster parents.

The first set of results show that the most important predictors of becoming foster parents are living in a rural or provincial municipality relative to an urban municipality, having higher than elementary education for both parents, and being educated in fields related to education or health and welfare. Most importantly, there is almost no predictive power in past employment or earnings, except for a positive effect of male employment in the year prior to fostering. I interpret this as a sign that there is no adverse selection on economic circumstance. An important caveat is that the current selection of foster families reflects not only supply side selection, but also demand side selection. The municipality may screen applicants for past labor market experience when they choose who to approve as future foster parents. To the extent that the municipality has an effective screening mechanism, this should support the possibility for increasing compensation without compromising the quality of the selected foster parents.

The second set of results show that foster families decrease their earnings in the regular labor market when they become foster parents. This change is primarily driven by a substantial decrease in female labor market participation at the time when the family starts foster parenting. Despite the decrease in household earnings from the regular labor market, families experience a large increase in total household earnings of around 16 percent. This net economic gain is a result of average foster care compensation being larger than the average decrease in household earnings in the regular labor market. The surprisingly large economic gain from foster parenting may be attenuated by several factors. In contrast to other public employment in Denmark, there is no additional pension contribution for foster care work. But even if foster families have to save part of the foster care compensation for old age, it is unlikely to account for the full earnings increase. Another factor could be an earnings loss when the family stops foster parenting, for example due to skill depreciation. To look into the potential earnings loss at exit, I look at a subsample of foster parents at the time they stop foster parenting and find suggestive evidence that their total household earnings are not significantly worse than the control sample in the three years following exit.

Few economists have looked at the role of foster parents, their decision making and their response to economic incentives. One of the few studies on this topic has shown that increasing compensation has the potential to increase the supply of foster families (Doyle and Peters, 2007). In a different study, a reform of the subsidy for kinship care in the state of Illinois is exploited to show a significant drop (15%) in the supply of kinship care following a substantial decrease (30%) in the subsidy (Doyle, 2007). Both studies suggest that foster families respond to economic incentives and that compensation rates may play a role for how many foster families enter the market. The scant empirical literature on the effects of compensation for quality of care shows no or small effects of higher compensation on disruption rates for non-kinship foster families (Pac, 2017; Duncan and Argys, 2007), and the sociological literature on the motives for foster parenting shows that very few foster parents self-report economic reasons as an important motive (Bryderup, Engen, and Kring, 2017), although Doyle and Melville (2013) highlight that this result may reflect the social expectation that fostering is altruistically motivated.

I contribute to the scarce literature on the potential for increased compensation to reduce supply shortages in the foster care market. I provide a simple theoretical framework that illustrates the potential trade-off between compensation and quality in the supply of foster care. The model highlights the importance of the compensation-to-wage rate in regulating the quantity of foster families and the potential trade-off between quality and quantity of care when setting the foster care compensation rate. It is, to the best of my knowledge, the first paper to investigate the selection into foster parenting and related labor market decisions from a theoretical and an empirical perspective. It is also the first empirical study to identify and characterise a full national population of foster parents. The empirical results contribute new knowledge on foster parents' economic circumstances both before and after they become foster parents, and serve as a framework for discussing the potential for increasing supply of foster parents without sacrificing quality. In particular, I show that there is no sign of adverse selection into foster parenting due to economic circumstance and that foster parents have a net earnings gain from foster parenting at the current compensation rate.

2 Family Foster care in Denmark

Around 1 percent of Danish children are living in out-of-home care at any given time; a rate that has been stable over the last decade (Ejrnæs and Gørtz, 2017). This is in line with the percentage of children in care in the US, while rates for the other Nordic countries and the UK tend to be lower. The most common types of out-of-home placement is in family foster care. Around two thirds of children in care live in foster families and one third live in institutional care.

To be approved for fostering, parents must complete a course that aims to prepare parents for the difficult task of fostering children and for the municipality to assess the parents' suitability as potential foster parents (for a more detailed description of the approval process see Luckow (2019)). During the course, the responsible authority (in Denmark one of five regional offices) or the parents themselves can decide to discontinue the application process. There are few official requirements for becoming a foster family. According to the association of Danish municipalities, Local Government Denmark, anyone above the age of 25 and no more than 40 years older than the foster child can become a foster parent. The upper age limit can be waived for a second foster child. The most important requirement is to be able to provide a healthy and stable home environment, but for full-time placements, it is also a requirement that the family has a spare bedroom. Parents are approved to foster a certain number of children and accommodate a certain degree of special needs in the foster children they take into their care.

Once a family is approved for fostering, they wait to be contacted by the municipality regarding a potential foster child. It is up to the caseworker to find a suitable foster family for a foster child. Once a foster family is selected as potential foster parents for a child, they can decide whether they want to foster the child or not. Caseworkers usually attempt to place a child as close to the home of the biological parents as possible, but in many urban areas this proves difficult as housing space is often limited in the metropolitan areas and the foster child needs a separate bedroom. It is not unusual for children to be placed in a different municipality (60 percent of foster care placements in my sample).

Foster parents are hired by the municipality in charge of placing the child (usually the municipality of residence of the biological parents of the foster child) following compensation rate guidelines from Local Government Denmark. In addition to the compensation, the foster family receives tax-free transfers to cover the living costs of the foster child. These transfers are fixed rates, but foster parents can apply for additional funds to cover additional expenses on special occasions, such as birthdays. The compensation beyond direct expense transfers can be thought of as the wage for foster parenting. It compensates parents for taking responsibility of the child and for foregone wages in the regular labor market. Based on the sample in this paper, the average yearly household foster care compensation is approximately 320,000DKK per foster child (2015 prices). The contract should be renegotiated on a yearly basis, although it is uncertain whether this is done in practice (Deloitte, 2016). According to the guidelines, the compensation level can and should be adjusted if the needs of the child have changed. This gives rise to an inverse incentive structure, where foster parents will receive a lower compensation if the foster child gets better and needs less care over time.

Most municipalities currently follow these guidelines for managing the contractual relationship with foster parents, although some have experimented with alternative wage models.

Contracts can generally be terminated with two weeks notice in the first three months of a placement, after which, notice must be given a month in advance. The short-term contracts are cost saving from the point of view of the municipalities and allow quick adaptation to changes in the needs of the foster child. From the foster families' point of view, the combination of short-term contracts and the yearly renegotiation of compensation introduces financial uncertainty. In some cases, foster parents are required to completely or partly leave their regular job in order to care for the foster child. It is unclear how often compensation is reduced, but according to Deloitte (2016) and anecdotal evidence, foster parents do worry about the economic uncertainty.

3 Theoretical Framework

The simple model I present here draws on ideas from the influential literature on household production by Becker and Mincer in the 1960s. One of their main contributions is the insight that time allocation and foregone earnings are central factors in shaping labor supply (Becker, 1965), and that parents internalize children's utility (Becker, 1981). I consider the labor market decisions of foster parents in a unitary, single-period, static decision model. In this stylized framework, parents' utility depends on their own consumption, their leisure and time spent foster parenting. In order to solve the model analytically, I assume a specific functional form of the utility function. Consider that the utility of foster parents is given by

$$\begin{aligned} U_{FP} &= U_P(c,l) + \theta U_{FC}(c,F) - \delta \mathbb{1}\{\text{if } F > 0\} && (1) \\ &= c^{\mu} l^{\alpha} + \theta((\gamma c)^{\mu} F^{\alpha}) - \delta \mathbb{1}\{\text{if } F > 0\}, && (2) \end{aligned}$$

where $c > 0$ is family consumption, $l > 0$ is share of time spent on leisure and $F \geq 0$ is share of time spent foster parenting. This utility function assumes that foster parents get direct utility from own consumption and leisure (U_P), and indirect utility from the foster child's utility (U_{FC}), which depends on the fraction ($0 \leq \gamma \leq 1$) of family consumption that benefits the foster child and the time parents spend with the foster child (F). The indirect utility is weighted by the parameter $0 \leq \theta \leq 1$, which represents the share of the foster child's utility that translates into utility for foster parents, which is a measure of their altruism. The parameters of the utility function, $0 < \alpha < 1$

and $0 < \mu < 1$, are preference parameters, determining the shape of the utility function. Both are assumed to be between 0 and 1 to ensure positive but decreasing marginal utilities. Note that a simplifying assumption here is that marginal utilities of time spent on leisure and time spent foster parenting are assumed to decrease at the same rate, α. This is a restrictive assumption, that is necessary in order to obtain an analytical solution to the model. The model assumes a fixed utility cost of foster parenting (δ) arising only when foster parents participate in the foster care market ($F > 0$).

Foster parents face two constraints, namely a time and a budget constraint with respect to the three choice variables: consumption (c), leisure (l) and time spent foster parenting (F). The time constraint is given by

$$1 = L + l + F \implies F \leq 1 - l, \tag{3}$$

where $L \geq 0$ is share of time spent in the regular labor market and the total amount of time available is normalized to one. The constraint can be rewritten as an inequality constraint for F, when $F = 1 - l$ then $L = 0$, implying that no time is spent working in the regular labor market. The budget constraint is given by

$$c = wL + \tau \mathbb{1}\{\text{if } F > 0\}, \tag{4}$$

where w is the wage rate in the regular labor market, τ is the foster care compensation rate, which is assumed to be fixed. I assume that the budget constraint holds with equality, such that families spend all available resources. See appendix C for solving the model. The model shows some interesting features of the selection into foster parenting.

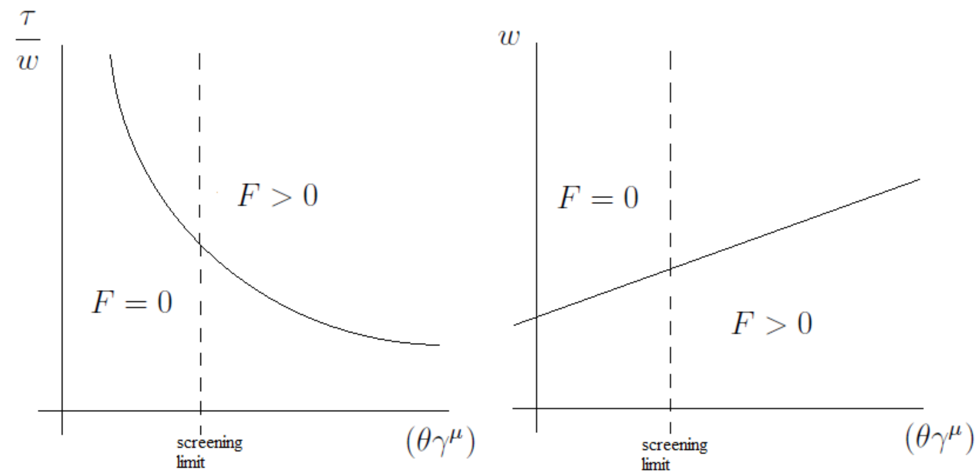
3.1 Selection into the foster care market

The model predicts that families who enter the foster care market have high intrinsic motivation, low outside wages or low direct costs of foster parenting. If a family chooses to enter the foster care market, we expect the maximum utility from foster parenting to exceed the maximum utility when not foster parenting when the following is satisfied:

$$\left(\frac{\mu/\alpha + \tau/w}{1 + \mu/\alpha} + \frac{\tau}{w}\right)^{\mu}\left(1 + \frac{\tau}{w}\right)^{\alpha} - \delta \geq \left(\frac{1}{1 + \alpha/\mu}\right)^{\mu} \frac{1}{(1 + (\theta\gamma^{\mu})^{1/1-\alpha})^{1-\alpha}}. \tag{5}$$

When this expression holds with equality, it traces out the optimality border between becoming a foster parent and not becoming a foster parent as a function of the compensation-to-wage ratio (τ/w), the direct cost of foster parenting (δ), and the parents' altruism or skill for foster parenting ($\theta\gamma^{\mu}$). In the model it is not possible to identify altruism (θ) or the fraction of family consumption that benefits the foster child (γ) separately, so the set of parameters ($\theta\gamma^{\mu}$) must be thought of as jointly determining a combination of parental altruism towards the foster child and generosity in sharing family consumption, which I refer to as foster parents' *intrinsic motivation*. The selection into foster parenting is illustrated in figure 1, where the direct cost of foster parenting (δ) is held constant. Families must either have a high $\theta\gamma^{\mu}$ or a sufficiently high compensation-to-wage rate (τ/w) in order to choose to become foster parents. Holding the compensation rate τ constant, this is equivalent to having either a high level of intrinsic motivation for foster parenting ($\theta\gamma^{\mu}$) or a low outside wage (w). Having a low cost of foster parenting (δ) will, all else equal, increase the probability of becoming a foster parent. In figure 1 the vertical dashed line represents a potential screening limit imposed by the municipality in the hiring process. The screening is external to the model, but illustrates the municipality's ability to (partly) observe a lower limit of foster parents' intrinsic motivation during the training and hiring process.

Figure 1: Illustration of optimality



3.2 Policy implications

The municipality's goal is to ensure the well-being of children This makes it an important task for child protection services to recruit a sufficient number of highly skilled foster parents. This gives rise to two distinct policy goals; ensuring a high enough *quantity* of foster parents, and ensuring a high enough *quality* of foster parents. [1]

First, consider the *quality* of care. In the model we may think of quality of foster care as the amount of time spent foster parenting (F), or some combination of the altruism, generosity and skill level of foster parents ($\theta, \gamma^{\mu}, \alpha$). Time spent foster parenting is a relevant measure of quality on the intensive margin, but compensation is decoupled from the number of hours spent foster parenting and would be hard to monitor in practice. The municipality has little control over the amount of time foster parents spend with the foster child. The balance between time spent on leisure versus foster parenting is instead determined by foster parents' intrinsic motivation. Parents with a higher intrinsic motivation for foster parenting will choose to spend a larger fraction of their time on foster parenting relative to leisure.

The intrinsic motivation and the compensation-to-wage ratio jointly determine the likelihood that a family chooses to enter the foster care market, that is, the *quantity* of foster parents. The only real policy parameter in this setting is the compensation rate (τ). The higher the compensation, the lower the foster parents' intrinsic motivation has to be to make it attractive to enter the foster care market for a given family. One strategy for recruiting families with a high intrinsic motivation could be to set a low compensation rate. Since the determinant is the compensation-to-wage rate, low-wage parents may find foster parenting attractive and a higher compensation could lead to a lower average intrinsic motivation of foster parents. This gives rise to a potential quality-quantity trade-off when setting the compensation rate. There may be several reasons why the quality-quantity trade-off is not observed empirically. If we believe the regular labor market wage holds information on the productivity or skills of the family and contributes to the quality of care, then the effect of increased compensation becomes ambiguous. In addition, the municipality screens families before hiring them to foster a child. If this screening is effective, setting a higher compensation rate may not constitute a quantity-quality trade-off. On the other hand, if it is difficult to observe quality in the recruitment process, the municipality may have a hard time ensuring the desired selection of families. Another natural quality control mechanism, which is not modelled here, arises from the dynamic aspect of foster care. The foster child, municipality or foster parents may, at any given time, choose to terminate the placement. Assuming that foster parents want to foster children for more than one period, there is a dynamic incentive to provide quality care. Another important aspect is that quality may also be

[1] While the model presented above maximizes the utility of foster parents, the municipality may be more interested in directly maximizing the foster child's utility function and interests are not necessarily aligned between the two. This may result in a principal-agent problem not modelled here.

partly determined in the match between foster family and foster child. If this is the case, then a larger pool of potential foster parents could increase the match-quality.

Despite the simplicity of the model, it leaves us with two main insights. First, families with higher intrinsic motivation for foster parenting, lower wages or lower costs of foster parenting, are more likely to enter the foster care market. Second, there is a potential trade off between quantity and quality when setting the compensation rate, since increasing compensation will increase the quantity of applicants, but may reduce the average intrinsic motivation. Effectively screening families for quality could make increased compensation a viable way to reduce the shortage of foster families without sacrificing quality.

4 Data

The main data source is individual-level Danish administrative data. Despite the very high quality of the data, there is currently no central register of foster parents. I construct a sample of foster families by combining employment data with information on the address of children placed in family foster care. The data goes back to the early 1990s and the final sample consists of 5,732 families who were full-time foster parenting for the first time between 1997 and 2012. For details of the sample selection process and data quality see appendix A.

4.1 Descriptive statistics

Table 1 shows descriptive statistics for the foster families in the sample in the first period they are observed as full-time foster parents. I refer to the families as full-time foster parenting in years where they receive foster care compensation and have a foster child living at their address. A substantial proportion of the foster families are foster parenting part-time before becoming full-time foster parents and receive foster care compensation without sharing an address with a foster child. The female is the primary caregiver in 77 percent of families. The primary caregiver is defined as the parent in the foster family who receives the most foster care compensation in the first year of fostering. The majority of families have biological children living at home. 15 percent of foster families foster more than one child in their first year as foster parents. The average yearly foster care income per foster child is 318,774 DKK. In 91 percent of foster families at least one of the foster parents holds a job in the regular labor market. For females, the labor market participation rate is 64 percent, and it is 80 percent for males. This likely reflects that the woman is the primary caregiver for the children in the majority of foster families and also the one who is more likely to leave her regular job as the family starts fostering. After 3 years, 81 percent of families are still full time foster parenting, while after 5 years, 72 percent are, which suggests that retention of foster families is quite good.

Table 1: Foster family characteristics, first year of full time fostering

	Mean	(sd)
Primary foster parent female	0.77	(0.42)
At least one biological child living at home	0.73	(0.45)
Average age of biological children living at home	13.46	(4.38)
More than one foster child in care	0.15	(0.36)
Average age of foster children	7.32	(5.28)
Yearly household FC income (DKK)	358,924.92	(187,531.11)
Yearly compensation per child (DKK)	318,774.17	(166,151.81)
Actively foster parenting after 3 years	0.81	(0.39)
Actively foster parenting after 5 years	0.72	(0.45)
Household participation rate	0.91	(0.29)
Female participation rate	0.64	(0.48)
Male participation rate	0.80	(0.40)
Yearly household non-FC wage income (DKK)	429,174.90	(279,175.40)
Yearly total household wage income (DKK)	788,099.81	(262,050.47)
N	5,732	

I construct a subsample of foster families that I can observe in the monthly data at the moment when they begin foster parenting. I have access to monthly data in the years 2008 to 2018. Table 2 shows descriptive statistics for the 1,010 foster families that started foster parenting between 2009 and 2017. The characteristics are quite similar to the main sample.

Going back to the yearly data, I construct a second subsample of foster families that can be observed at the time when they stop full time foster parenting. I condition on observing the families at the time they exit the foster care market and for the three following years. This leaves me with a sample of 2,401 foster families. Table 3 shows that 12 percent of families have more than one foster child in care when they exit and only around 40 percent have been full time foster parenting for at least five years. The large majority of families have had more than one foster child in care while they have been fostering. The yearly compensation per child is lower at around 280,000DKK per child per year in the last year of fostering.

Table 2: Foster family characteristics, first month of full time fostering

	Mean	(sd)
Primary foster parent female	0.80	(0.40)
At least one biological child living at home	0.76	(0.43)
Average age of biological children living at home	13.78	(4.33)
More than one foster child	0.07	(0.26)
Average age of foster children	3.39	(3.30)
Monthly household FC income	35,544.43	(22,552.00)
Monthly compensation per child	32,727.22	(20,076.44)
Share of year the family receives FC income	0.73	(0.29)
Household participation rate	0.94	(0.23)
Female participation rate	0.53	(0.50)
Male participation rate	0.88	(0.32)
Monthly household non-FC wage income	35,952.90	(28,812.94)
Monthly total household wage income	67,906.30	(31,377.92)
N	1,010	

Table 3: Foster family characteristics, last year of full time fostering

	Mean	(sd)
Primary foster parent female	0.72	(0.45)
At least one biological child living at home	0.47	(0.50)
Average age of biological children living at home	14.68	(4.89)
More than one foster child in care	0.12	(0.33)
Average age of foster children	13.44	(4.12)
Actively fostering for more than 3 years before exit	0.57	(0.50)
Actively fostering for more than 5 years before exit	0.39	(0.49)
More than one child in care throughout career	0.87	(0.34)
Foster children in care throughout career	7.07	(6.15)
Yearly household FC income	296,356.03	(195,363.25)
Yearly compensation per child	277,584.44	(163,247.04)
Household participation rate	0.88	(0.32)
Female participation rate	0.70	(0.46)
Male participation rate	0.76	(0.43)
Yearly household non-FC wage income	441,606.39	(311,250.89)
Yearly total household wage income	737,962.42	(290,711.66)
N	2,401	

5 Empirical strategy

The empirical strategy consists of two parts. The first part is a logistic regression model of whether a family chooses to foster or not. The model allows me to identify important predictors of the selection into foster parenting as well as to identify a control group. I will rely on a propensity score matching procedure to identify non-foster families that are comparable to the foster families in my sample on a range of observable characteristics. The matching procedure is performed in two steps. First, I estimate the probability of becoming a foster family in the logistic regression model. In the main specification of the model, the probability of becoming foster parents is estimated on a set of family characteristics including an indicator for having biological children at home and their age, where the family lives, and parents' characteristics including age and, in particular, past labor market participation and earnings. Second, I use the estimated propensity score to match foster families to non-foster families using a one-to-one nearest neighbor matching algorithm without replacement. For a discussion and review of the reliability of propensity score matching see Smith and Todd (2001, 2005)). I check the robustness of the estimates using different specifications of the propensity score (see appendix B).

In the second part of the empirical strategy, I want to evaluate the economic impact of becoming foster parents. I use a difference-in-difference event study approach to compare the foster parents to the matched control group across event time. The approach has the advantage of tracing out the dynamic impact across time relative to the event. The use of a matched control group in an event study design resembles what is known in the causal effects literature as a synthetic difference-in-difference method with staggered adoption Arkhangelsky et al., 2019. The decision to become a foster family is not exogenous, but the event generates a distinct change in household earnings, which we would otherwise expect to evolve smoothly over time. The event study approach exploits family-level variation in the timing of becoming foster parents to estimate the economic impact of foster parenting. For each family in the data, I normalize the year in which they first foster parent full-time to event time t=0. Event time t is then measured relative to that year. The main sample is balanced to consist of families that are observed in the data at least 5 years before and 5 years after the event. Families that are never foster families can be matched in any year they are observed in the data, and the 'placebo' event time is assigned based on the year in which they match the foster family characteristics.

I estimate a two-way fixed effect model of the outcome of interest, Y_{ikt}, for family i in calendar year k at event time t

$$Y_{ikt} = \sum_{j \neq -1} \beta_j \cdot \mathbf{I}[j = t] + \alpha_i + \delta_k + u_{ikt}, \tag{6}$$

where the first term on the right-hand side is a full set of event time dummies, with coefficients measuring the impact of foster parenting relative to the baseline event period $t = -1$. α_i are family fixed effects and δ_k are calendar year fixed effects. Without the calendar year and family fixed effects, the difference-in-difference estimate would be equal to the difference in the average outcome between the two groups. I measure earnings in levels rather than logs in order to retain non-participants. For the main results I present two graphs for each outcome. The first graph shows the group average outcomes across event time for the foster families and matched control families separately. The second graph shows the estimated event coefficients, which measure the difference in outcome between the two groups relative to the year before the event, taking into account year and family fixed effects.

Using a subsample of the treatment and control sample, the monthly graphs show the exact development in the 12 months leading up to and the 12 months following the entry into foster parenting. For the exit graphs, the event time is redefined to the last year in which the foster family is foster parenting.

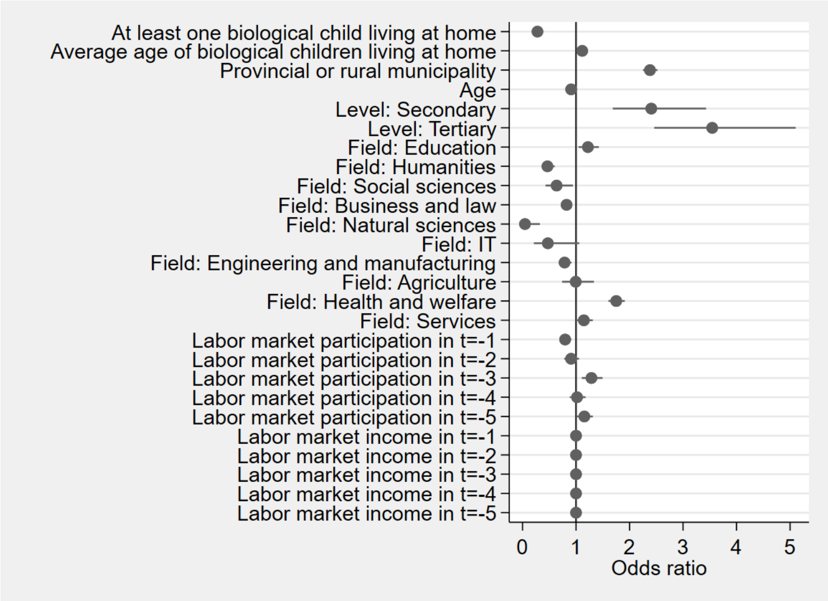
6 Results

6.1 Selection into the foster care market

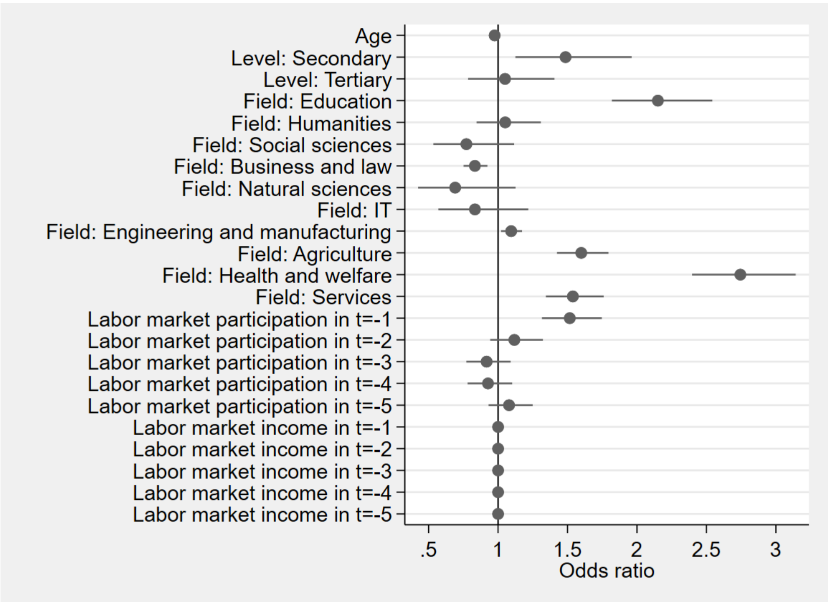
The theoretical model predicts that foster families have higher intrinsic motivation for foster parenting and lower labor market wages prior to fostering. The results from estimating the probability of becoming foster parents are presented in terms of odds ratios in figure 2a for family and female characteristics and 2b for male characteristics. The odds ratio reports the relative probability of becoming foster parents controlling for the other characteristics included in the model (for more detail see appendix B). The figures show that families are less likely to have biological children living at home and they are more likely to live in provincial or rural municipalities with respect to urban municipalities. The strong predictive power of rural versus urban likely reflects the lower rural housing costs which reduce the direct cost of foster parenting, which accords with the prediction in the theoretical model. Women with secondary or tertiary education had 3 to 4 times higher odds of becoming foster parents than women with primary education. For males, secondary education gives higher odds of being foster parents, but tertiary education does not. The parents' field of study seems to be an important predictor of becoming foster parents, particularly for males where a degree in education or health and welfare are among the strongest predictors of becoming foster parents. Educational level and, in particular, field of study may reflect parental specialization in fields relevant to caring for vulnerable children, and be interpreted as a proxy for quality, skill or motivation for foster parenting. Labor market earnings are not an important predictor for becoming foster parents. Labor market participation seems more important, particularly male participation in the year prior to fostering, and it gives increased odds of becoming foster parents.

An important caveat is that while the theoretical model considers only supply side effects, the empirical evidence also reflects how the municipality selects foster families. The evidence I present here does not support the hypothesis that the families we observe choose to become foster parents out of economic necessity, but I cannot exclude that increasing compensation could decrease the quality of applicants. It may be a result of effective municipality screening in selecting families who do not have pecuniary motives for becoming foster parents or who have a high intrinsic motivation for foster parenting.

Figure 2: Odds ratios, becoming a foster family



(a) Family and female characteristics



(b) Male characteristics

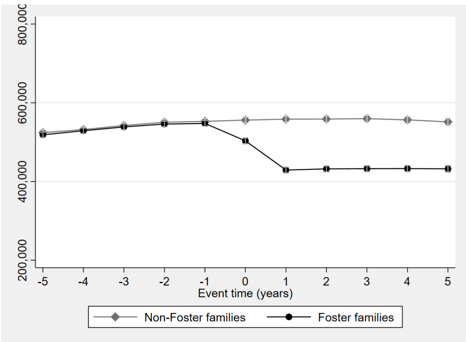
Note: Baseline level of education is primary education, baseline field of education is uncategorised.

6.2 Consequences of foster parenting

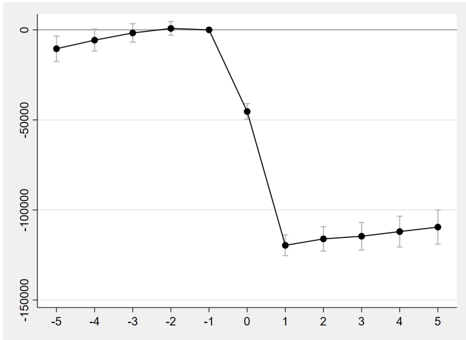
I will now turn to the results from the event study. The two left-hand panels of figure 3 show average household earnings for foster families and matched control group separately (for balance see appendix table B.2). The two right-hand panels show the estimated impact of foster parenting from event model 6. In the period between event times -1 and 1, foster parents transition into foster parenting. The last year before fostering is -1 and the first full year of foster parenting is year 1. Figure 3a shows a sharp decrease in regular labor market earnings as families enter the foster care market. After year 1 it seems that labor market earnings remain constant at a lower level than before. However, as shown in figure 3c, total earnings increase. There is a divergence in total household earnings prior to the event relative to the matched control group. The pre-trend in this figure reflects the foster care compensation earned by some foster families prior to full-time foster parenting (see appendix D for average outcomes for foster families who do not receive foster care compensation prior to full-time fostering). Figure 3b shows that the estimated impact of becoming foster parents on household labor market earnings is a yearly decrease in earnings of almost 120,000DKK. Figure 3d, on the other hand, shows that total household earnings increase by approximately 110,000DKK. This is equivalent to an increase of 16 percent in year 1 relative to the baseline household earnings in the year before the event (see appendix table B.3). This reflects that foster care compensation more than makes up for the lost earnings in the regular labor market.

Figure 4 shows that the decrease in labor market earnings is driven by a decrease in the participation rate when the family starts foster parenting. While both men and women reduce their labor supply when they become foster parents, figure 4a clearly shows that the impact is largest for women, with an 18 percent decrease in participation relative to the baseline participation rate of .83 in the year before the event. This likely reflects that women are the primary caregivers in the majority of families.

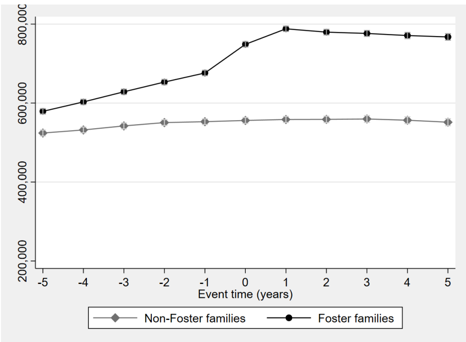
Figure 3: Household earnings at entry



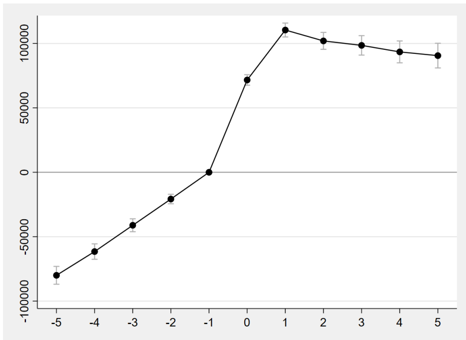
(a) Labor market earnings



(b) Impact on labor market earnings



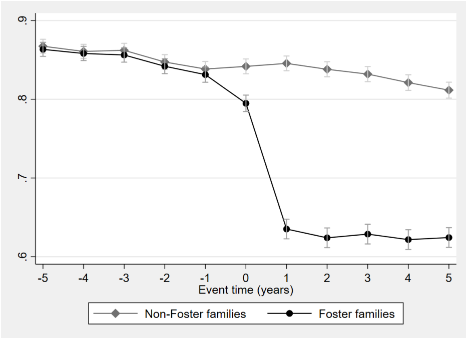
(c) Total earnings



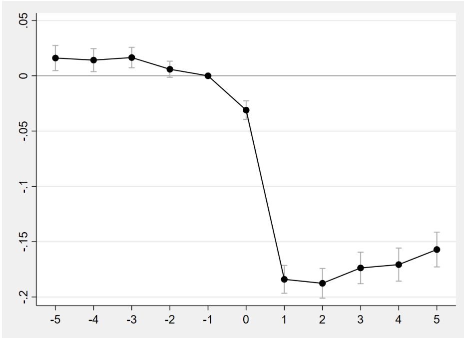
(d) Impact on total earnings

Note: The left-hand panels show the groups' averages across event time. The right-hand panels show the estimated event coefficients from event model 6.

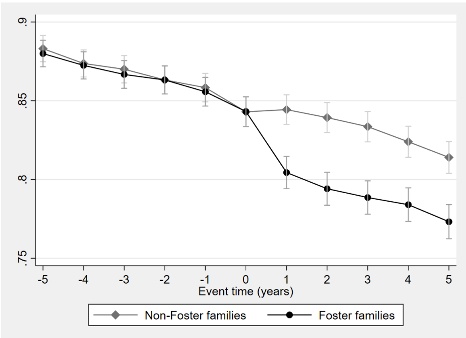
Figure 4: Labor market participation at entry



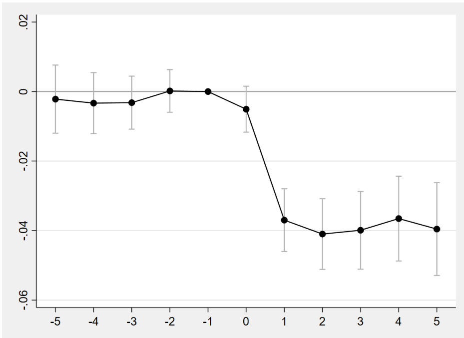
(a) Female participation rate



(b) Impact on female participation rate



(c) Male participation rate

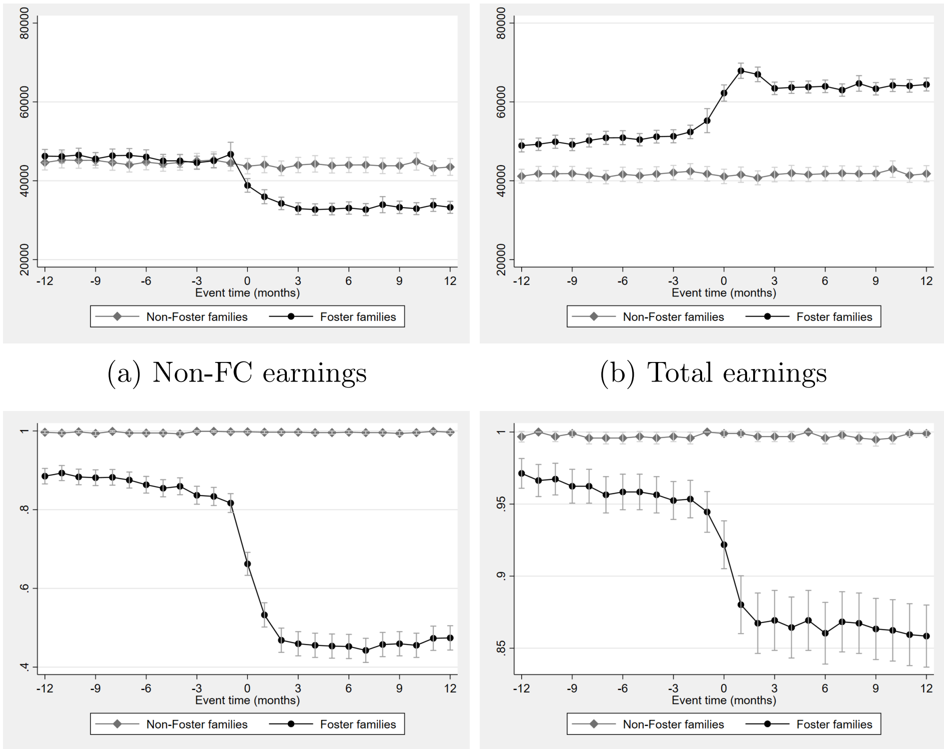


(d) Impact on male participation rate

Note: The left-hand panels show the groups' averages across event time. The right-hand panels show the estimated event coefficients from event model 6.

It is a well-known result in the empirical literature on programme evaluation that there often is a pre-programme dip in earnings or participation, also known as an Ashenfelter's dip (Ashenfelter, 1978). To look for such a change prior to becoming a foster family, I make use of monthly data for a sub sample of the foster families. Figure 5 shows the average household earnings and participation rates for foster families and the matched control group. Figures 5a and 5b show that there is no evidence of a dip in earnings prior to becoming foster parents. Total earnings are at higher level for foster parents in the pre-event period because they earn part-time foster care income on top of their regular labor market income. Figures 5c and 5d show a slight decrease in the participation rate for both women and men in foster families prior to fostering. This divergence in participation between the foster families and the matched control group is likely due to their part-time foster care activities, which are gradually increasing in the months leading up to full-time foster parenting.

Figure 5: Monthly earnings and participation at entry



(a) Non-FC earnings

(b) Total earnings

(c) Female participation rate

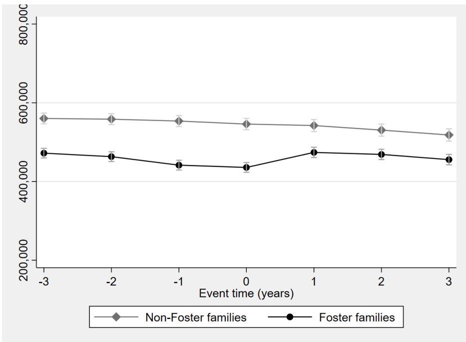
(d) Male participation rate

Note: The figure shows averages outcomes for the monthly subsample, for foster families and non-foster families separately.

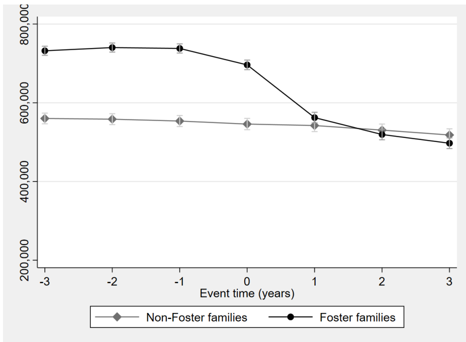
To sum up, the evidence shows that the foster care compensation allows both parents to decrease their attachment to the regular labor market as they start foster parenting, while still experiencing a substantial net increase in total household earnings. In line with the theoretical model, the empirical evidence shows that foster parents decrease time spent in the regular labor market as they become foster parents. This does not affect their total household earnings negatively as long as they stay active in the foster care market. The empirical estimate of the compensation-to-wage rate is 1.16. This significant economic gain from foster parenting presents us with a puzzle: Why are there recurring shortages of foster families if there is an economic gain from entering the market? There may be several reasons for this. One reason may be that employment as foster parents does not give an additional pension contribution as is the case for most public sector employees. Other municipality employees would, as part of their contract, receive a 12-15 percent pension contribution on top of their wage earnings. To the extent that foster parents' alternative employment would be public sector employment, they may have to save part of the earnings gain from foster parenting for old-age. However, it is unlikely to account for the full earnings gain, since most families still have other employment on top of foster parenting. Another cost of foster parenting could be a loss of earnings when the family stops foster parenting.

Figure 6 shows earnings across the year in which families stop foster parenting. While there was a significant gain from entering the foster care market, families do not recover to their previous labor market earnings when they exit the foster care market. Figure 6b shows that total household earnings decrease to the level of the control group over the three years following exit. Figures 6c and 6d show that the drop in earnings is due to a less than full recovery of labor market participation for men in particular. While labor market earnings do not recover completely, families continue to earn some foster care compensation due to part-time fostering. This means that foster parents do not decrease total earnings when they stop foster parenting. It is not possible to know whether foster parents could recover labor market earnings if they wanted to, or if they have to continue part-time fostering to maintain their household earnings.

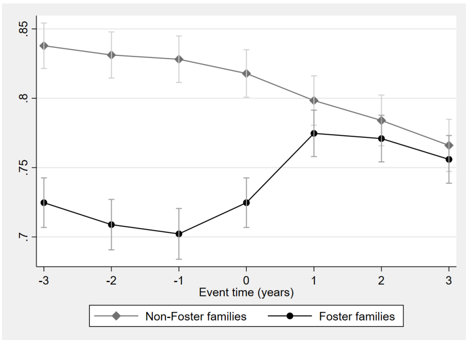
Figure 6: Yearly earnings and participation at exit



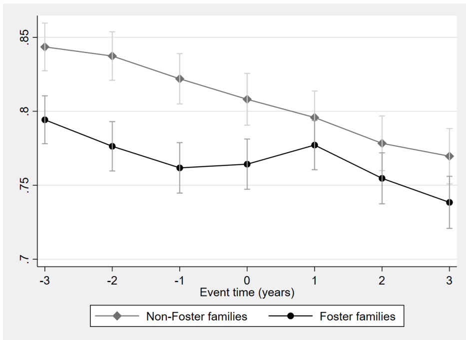
(a) Non-FC income



(b) Total income



(c) Female participation rate



(d) Male participation rate

Note: The figure shows average outcomes for the subsample of families observed at exit, for foster families and non-foster families separately.

7 Conclusion

This paper provides new evidence on selection into foster parenting and how the choice to foster parent is reflected in labor market supply and household earnings. The theoretical prediction is that families with high intrinsic motivation for foster parenting or low wages are more likely to become foster parents. The empirical results show that foster families are, on average, well educated in fields related to care work, supporting the idea that families with higher intrinsic motivation for foster parenting are more likely to become foster parents. Foster parents have strong labor market attachment prior to fostering and there is no sign of an Ashenfelter's dip prior to entry. This suggests that foster parents do not select into foster parenting because they are out of a job or have low or decreasing earnings prior to fostering. Contrary to model predictions, these families are not low wage earners before they start fostering. The results suggest that the recruitment of foster parents is successful in attracting and selecting a group of foster parents with relevant skills, and there is no evidence of adverse selection due to economic circumstance.

The event study shows that foster parents experience an increase in household income when they start fostering, a gain which is sustained throughout their time as foster parents. With such a significant gain in household income, why are not more families interested in becoming foster parents? There are two important potential explanations. First, contrary to most other public sector jobs in Denmark, there is no pension contribution on top of the wage. Second, foster families do not seem to recover to their previous labor market earnings path as they exit the foster care market. The drop in regular labor market income could reflect skill depreciation during their time as foster parents. Both the lack of pension savings and the skill depreciation attenuate the economic gain from foster parenting. In addition, there are other potentially important non-monetary costs (or benefits) of foster parenting. For example, job and earnings insecurity, working hours, or potential externalities on other family members, e.g., biological children.

The results are important because recruiting enough foster parents is a recurring problem for municipalities. From a theoretical perspective, the municipality faces a potential trade-off between ensuring high quality and high quantity of foster parents. The main policy instrument is the compensation paid to foster parents, and while increasing the compensation could increase the supply of foster parents, it may also decrease the quality of applicants. The empirical analysis shows that the foster families that are currently being recruited are average earnings families with strong labor market attachment prior to fostering, and relevant skills for foster parenting. The results, however, cannot be used to predict whether municipalities would encounter a decrease in the quality of applicants if they decide to increase compensation for foster parenting. In conclusion, the evidence supports that increasing compensation for foster parenting in combination with an effective screening mechanism could be a viable way to increase the supply of foster families without sacrificing quality of care. However, more research is still needed to establish the causal link between compensation and quality of care.

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Appendix

A Sample selection and data quality

Sample selection

The foster family sample is constructed by identifying families in the population register who received income with the industry code for foster care and have a foster child living at the address. By the end of 2013 the Danish Council of Appeals estimated the number of active foster families with children in full-time care in Denmark to be 4,491 Ankestyrelsen, 2014. I am able to identify 3,826 families who receive foster care compensation and had a foster child living at their address by the end of 2013. This is group A in table A.1 and is equivalent to 85.2 percent of the estimated total. Some families receive foster care compensation without having a child registered at the address (group B). Since the population register is a stock registered at the end of the year, it is not possible to identify short-term placements that do not take place around the new year. Another explanation for the families in group B is that foster parents may be part-time fostering, in which case they receive foster care income but are not full-time foster parenting. Families in group C may be biological families in cases where the child's address has not yet been updated. Only families in group A are included in the final sample.

Table A.1: Number of Foster Families in the registers, 2014

	FC compensation	No FC compensation
Foster child at address	A: 3,826	C: 5,453
No foster child at address	B: 7,285	D: 665

Source: Statistics Denmark (BUAH, BEF, RAS)

Note: The table represents the raw data for the year 2013 before the sample selection process. In the analysis only foster families in group A are used. Group D is the estimated number of families I fail to identify. The number is calculated from the estimated number of active foster families in Denmark at the end of 2013 according to Ankestyrelsen, 2014, 4491-3826=665

I clean the sample of foster families to contain only the uniquely identifiable foster families. For simplicity, and since it applies to the large majority of foster families, I restrict the sample to families with two parents, one male and one female. The sample is balanced to include only families that are observed in the data 5 years before they start fostering and 5 years after.

The sample selection process is illustrated in table A.2. The adult receiving the highest average foster care compensation in the first year of foster parenting is treated as the primary foster parent, and the foster family is identified from this person using family and spouse identification numbers. The sample follows the primary foster parent over time, despite address changes, divorce or other significant changes in family composition. Observations where a foster family cannot clearly be identified are dropped from the sample, for example, cases where there is more than one family with foster care income living at the address. This leaves us with 5,732 foster families, corresponding to 46,508 family-year observations.

Table A.2: Identification of active foster families in Denmark

Sample selection for foster family observations, 1992-2017	N, families	N, family-year	%
All observations with both			
a) a foster child registered at the address, and			
b) receiving foster care compensation	8,590	60,577	100.0
Excluding families with:			
>2 potential foster families at the address	8,479	59,922	98.7
1 parent	7,921	57,755	92.2
Balanced event sample			
observed for at least 11 consecutive years around event	5,732	46,508	66.7

Source: Statistics Denmark (BUAH, BEF, RAS)

The comparison sample for the propensity score matching procedure is selected from a ten percent sample of Danish families that never receive foster care income or have a foster child at their address between 1992 and 2013. This leaves me with a balanced sample of 114,069 potential comparison, non foster families to be matched.

Table A.3 shows the percentage of foster children I am able to identify in the sample over time using the identification method described above compared to the estimated number of children in foster care according to the placement register. In the final sample I observe mainly children in foster care, but also some children in other types of care or where there is no reported type of care.[2] As can be seen from the table, it seems that some children are either not reported to be in foster care in all the years in which I observe them living with a foster family, or are reported to be placed in other types of care despite living in what I identify as foster families. The table shows the sum of children in placement in the final sample compared to the registered number of children in foster care placement according to the

[2]There can be missing values either because the type of care is missing in the data, or because the children are previously placed, but not registered as placed in that particular year. I still count them as being in out-of-home care if they were observed as living with a foster family.

placement register (Statistics Denmark, BUAH). As shown in the rightmost column in table A.3, the share of identifiable children in the final sample is quite stable at around 80 percent over time.

Table A.3: Number of Children in Care in the Data

	Final Sample			Placement Register	
Year	Foster Care	Other or Missing Type	Sum	Foster Care	% of Children in Final Sample
1992	179	3	182	285	63.9
1993	258	8	266	417	63.9
1994	383	16	399	571	69.9
1995	509	21	530	768	69.0
1996	678	34	712	983	72.4
1997	877	44	921	1,257	73.3
1998	1,121	55	1,176	1,635	71.9
1999	1,471	81	1,552	2,046	75.9
2000	1,857	95	1,952	2,488	78.5
2001	2,240	135	2,375	2,956	80.3
2002	2,748	181	2,929	3,497	83.8
2003	3,253	193	3,446	4,060	84.9
2004	3,781	265	4,046	4,719	85.7
2005	4,520	326	4,846	5,560	87.2
2006	4,988	420	5,408	6,181	87.5
2007	4,974	432	5,406	6,348	85.2
2008	4,930	467	5,397	6,485	83.2
2009	5,071	463	5,534	6,662	83.1
2010	5,249	464	5,713	6,855	83.3
2011	5,422	491	5,913	7,103	83.2
2012	5,682	509	6,191	7,303	84.8
2013	5,864	517	6,381	7,361	86.7
2014	5,997	506	6,503	7,419	87.7
2015	6,181	460	6,641	7,335	90.5
2016	6,258	443	6,701	7,723	86.8
2017	6,392	481	6,873	7,742	88.8

Source: Statistics Denmark (BUAH, BEF, RAS)

Note: The percentage of children in the final sample is calculated as the sum of children in care as a percentage of the number of children in foster care as reported by the BUAH register.

Quality of Stipend Data

As discussed in the previous section, the available data is noisy for several reasons. In particular there are many families with foster care income who do not have a foster child registered at their address. Similarly, there are families with a foster child registered at their address who do not seem to receive any foster care compensation. To get an understanding of the precision with which I measure the foster care income that is observed in the registers, I will compare the register information to a dataset from Copenhagen Municipality containing information on payouts from the municipality to all employed foster parents from 2011Q1-2015Q4. This data identifies the monthly payment to each foster parent. To allow for a comparison with the register data, I aggregate compensation per family over the entire period and normalize by the number of quarters of foster care summed over all foster children in the family in the period. This gives me an estimate of the family compensation per quarter per child from the registers and a similar estimate for Copenhagen data based only on compensation and number of children placed by Copenhagen Municipality. As foster families may have foster children coming from more than one municipality, this comparison relies on there being no systematic differences in the average compensation for children placed by Copenhagen municipality compared to the rest of the country. The absolute difference between the two measures is defined as

$$\Delta C_i = \underbrace{\frac{\sum_{q=1}^{Q}\sum_{j=1}^{J}\sum_{e=1}^{E} \text{FC Income}_{ijqe}}{\sum_{q=1}^{Q}\sum_{j=1}^{J} \mathbb{1}\{\text{if placed in FC}\}_{ijq}}}_{\substack{=\text{ average quarter-child compensation}\\ \text{per family, based on register data}}} - \underbrace{\frac{\sum_{m=1}^{M}\sum_{j=1}^{J}\sum_{e=1}^{E} \text{Payout from KK}_{ijme}}{\sum_{q=1}^{Q}\sum_{j=1}^{J} \mathbb{1}\{\text{if placed in FC by KK}\}_{ijq}}}_{\substack{=\text{ average quarter-child compensation}\\ \text{per family, based on Copenhagen data}}},$$

where $q = 1, 2, ..., Q$ is quarter, $m = 1, 2, ..., M$ is month, $j = 1, 2, ..., J$ is family member, $i = 1, 2, ..., N$ is family and $e = 1, 2, ..., E$ is employment.

There are at least three reasons why these two measures might differ. The first and most concerning source of error is the potential misreporting in the register data. I am worried that even in families that do receive some foster care income, not all income from foster parenting is registered as such.[3] This would result in a negative error, since the average quarter-child compensation based on the registers would be underestimated. This is the most important source of error because the registers are currently the only centrally accessible data source on foster parents' compensation in Denmark. The second source of error comes from the Copenhagen data. Some children in the Copenhagen municipality do not get placed through the municipality system, which means that the compensation parents receive does not show up in the payout data.[4] The children would still be registered as placed by

[3]The income could instead have missing values in the industry code or have a different industry code, for example, due to the fact that some foster parents register as self-employed.

[4]The percentage of children placed by other agencies is estimated by the municipality to be around 5 percent.

the municipality in the registers, which could cause the average Copenhagen quarter-child compensation to be underestimated and show up as a positive difference between the two measures. The third and last possibility is that there may be a true difference between the average compensation per child-quarter in Copenhagen versus the rest of the country. Depending on whether that true difference is positive or negative, we may under- or overestimate the error.

I am interested in measuring only the error coming from the first source, since this is what is important to me when I wish to use the register data on foster parents. Comparing the families with foster children only from the Copenhagen municipality should eliminate the third error source. As shown in Table A.4, there is a negative mean difference of around 6,500 DKK per child-quarter. When looking at families that only have foster children from Copenhagen in their care, this number changes to a positive difference of around 5,650 DKK per child-quarter. Due to the various possible error sources it is hard to make any definitive conclusions about the origins of the measurement error. It is clear that the data does not perfectly measure the foster care income of each family and there will be some measurement error in the estimates.

Table A.4: Statistics on absolute difference in compensation measures

	N	Mean	(sd)
All families, absolute difference	423	-6461.784	(34757.85)
Families with children from KK only, absolute difference	241	5648.962	(27301.67)
All families, percentage difference	423	-1.742769	(59.97484)
Families with children from KK only, percentage difference	241	12.26647	(62.14651)

Note: KK stands for Copenhagen Municipality.

B Propensity score matching

The fundamental problem in causal inference is the lack of an observed counterfactual outcome, often described in terms of potential outcomes in the Rubin causal model, Rubin, 1974, 1977.

When comparing the difference in income between two families, one of which has chosen to be a foster family (treatment group) and the other not (control group), the observed difference will consist partly of the effect of becoming foster parents and partly of the difference in unobserved potential outcomes of the two groups. This bias arises when the assignment of treatment is non-random, and there is a correlation between the choice of becoming foster parents and the potential outcomes. Following the notation of Angrist and Pischke, 2008, I consider the decomposition of the comparison of means between the treatment and control group in the potential outcomes framework

$$E[Y_i|D_i = 1] - E[Y_{1i}|D_i = 0]$$
$$= \underbrace{E[Y_i - Y_{0i}|D_i = 1]}_{\text{ATT}} + \underbrace{E[Y_{0i}|D_i = 1] - E[Y_{0i}|D_i = 0]}_{\text{selection bias}},$$

where Y_i is the potential outcome of individual i, which can take on the values Y_{0i} and Y_{1i}, where subscript 0 refers to the outcome of individual i in the untreated case and subscript 1 refers to the outcome in the treated case. It is important to keep in mind that these are potential cases, and in practice we only ever observe one of the two outcomes for individual i. D_i is the treatment indicator, which takes on the values 0 or 1 depending on whether we observe individual i in the untreated or the treated case, respectively. In the context of foster parents, we may think of Y_i as the potential outcome of a family. If this family is a foster family, we observe only Y_{1i}, whereas Y_{0i} is unobserved. Only when D_i is independent of Y_i does the selection bias disappear, and we can treat the simple comparison of means as causal. In this context, only if the choice to become a foster family is in no way related to potential earnings, can a simple comparison of treated versus non-treated serve as a causal estimate.

Propensity score matching is an approach to estimate the causal effect in the absence of randomized data. The two important insights underlying propensity score matching following Rosenbaum and Rubin, 1983 are that, in order to take into account the selection bias, it is sufficient to control for the covariates that affect the probability of treatment. The second result is, that given that we know the probability of treatment, we need only control for that probability itself to obtain an unbiased estimator for the treatment effect (on the treated). For a matching estimator to have a causal interpretation, two important assumptions must be fulfilled;

1. The Conditional Independence Assumption says that all relevant differences between the treated (foster parents) and the untreated (matched sample) are captured by the observables controlled for in the model.
2. The Common Support requirement says that for all sets of observables, for all values of the propensity score, we must observe both a treated and an untreated family. If an observation is off the common support, it means that there is no observation in the other group 'close enough' in terms of observables to estimate a counter factual outcome for that observation.

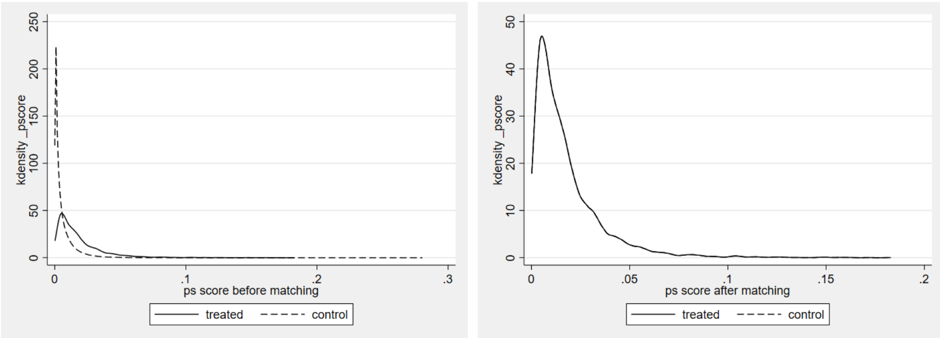
If these two assumptions hold, we can use the mean of the untreated control group to estimate the mean counter factual outcome for the treated group. The common support assumption is imposed in the matching procedure and can be verified in the data. Conditional Independence is much harder to justify since unobservable differences between the two groups cannot be ruled out. Although propensity score matching can get us closer to a causal interpretation, the results can not be interpreted as causal.

In the main analysis I use nearest neighbor matching with 1:1 matching without replacement. Nearest neighbor matching is a matching algorithm that, based on the covariates, will choose the 'nearest' n untreated observations to each treated one, where n represents the number of untreated control units chosen to match each treated unit. Distance is measured by the difference in the propensity score. The choice of the number of neighbors to match is subject to a bias-variance trade-off. A larger number of control units per treated unit will decrease the variance of the estimate, but will also decrease the precision of those controls since they may be further away from the treated unit. However, with access to a large pool of potential control units, matching to more than one nearest neighbor can reduce the variance of estimates without significantly increasing the bias, since there may be many control units that closely match the treated unit. One-to-one nearest neighbor matching is used in the main specifications of the propensity score matching. The results are robust to different specification of the matching procedure, as shown in the following.

Entry - Model 1

Here I present some additional material from the propensity score matching procedure in Stata (psmatch2, Leuven and Sianesi, 2003. Model 1 is the preferred specification, with the propensity score being estimated on family and parental characteristics separately by parents' gender. Table B.1 presents the logit estimation of the propensity score, figure B.2 the estimated ATT, table B.2 the balancing on covariates and figure B.1 the match on propensity score. The control group selected through this procedure is the one presented in all the main event study figures in the paper.

Figure B.1: Propensity score of the treatment and control groups



(a) Before matching (b) After matching

Figure B.2: Model 1, ATT

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
hus_sum_loen_~e2	Unmatched	788099.812	510637.31	277462.502	5512.58919	50.33
	ATT	788099.812	558440.467	229659.344	5226.27707	43.94
hus_sum_loen_~e3	Unmatched	779664.343	497683.025	281981.318	5609.00562	50.27
	ATT	779664.343	558722.047	220942.296	5511.37489	40.09
hus_sum_loen_~e4	Unmatched	776340.899	484139.096	292201.802	5691.78235	51.34
	ATT	776340.899	559705.939	216634.96	5794.54898	37.39
hus_sum_loen_~e5	Unmatched	771062.78	469915.595	301147.185	5768.95428	52.20
	ATT	771062.78	556570.358	214492.421	5961.35347	35.98

Table B.1: Selection into foster parenting

	Foster family	
At least one biological child living at home	-1.42***	(0.06)
Average age of biological children living at home	0.11***	(0.00)
Missing municipality information	1.16***	(0.19)
Urban municipality	0.00	(.)
Provincial or rural municipality	0.87***	(0.03)
Foster mother characteristics		
Age	-0.12***	(0.00)
Level: Primary	0.00	(.)
Level: Secondary	0.99***	(0.19)
Level: Tertiary	1.39***	(0.19)
Field: Uncategorized	0.00	(.)
Field: Education	0.19*	(0.08)
Field: Humanities	-0.82***	(0.13)
Field: Social sciences	-0.48*	(0.20)
Field: Business and law	-0.24***	(0.04)
Field: Natural sciences	-3.13**	(1.00)
Field: IT	-0.79	(0.41)
Field: Engineering and manufacturing	-0.32***	(0.08)
Field: Agriculture	-0.13	(0.15)
Field: Health and welfare	0.52***	(0.04)
Field: Services	0.03	(0.07)
Labor market participation in t=-1	-0.15	(0.09)
Labor market participation in t=-2	-0.07	(0.09)
Labor market participation in t=-3	0.25**	(0.09)
Labor market participation in t=-4	0.02	(0.09)
Labor market participation in t=-5	0.18*	(0.07)
Labor market income in t=-1	-0.00*	(0.00)
Labor market income in t=-2	-0.00	(0.00)
Labor market income in t=-3	-0.00**	(0.00)
Labor market income in t=-4	-0.00	(0.00)
Labor market income in t=-5	-0.00	(0.00)
Foster father characteristics		
Age	-0.03***	(0.00)
Level: Primary	0.00	(.)
Level: Secondary	0.40**	(0.14)
Level: Tertiary	0.09	(0.15)
Field: Uncategorized	0.00	(.)
Field: Education	0.86***	(0.09)
Field: Humanities	0.08	(0.11)
Field: Social sciences	-0.26	(0.19)
Field: Business and law	-0.17**	(0.05)
Field: Natural sciences	-0.31	(0.25)
Field: IT	-0.24	(0.19)
Field: Engineering and manufacturing	0.08*	(0.04)
Field: Agriculture	0.44***	(0.06)
Field: Health and welfare	1.05***	(0.07)
Field: Services	0.36***	(0.07)
Labor market participation in t=-1	0.45***	(0.08)
Labor market participation in t=-2	0.10	(0.10)
Labor market participation in t=-3	-0.08	(0.10)
Labor market participation in t=-4	-0.06	(0.10)
Labor market participation in t=-5	0.07	(0.08)
Labor market income in t=-1	-0.00***	(0.00)
Labor market income in t=-2	0.00	(0.00)
Labor market income in t=-3	0.00	(0.00)
Labor market income in t=-4	-0.00	(0.00)
Labor market income in t=-5	0.00	(0.00)
constant	0.50	(0.26)
N	974978	

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.2: Balance, treatment vs control

	Treatment mean	Control mean	Difference b	t
At least one biological child living at home	0.77	0.79	0.01	(1.7)
Average age of biological children living at home	9.61	9.85	0.24	(1.9)
Live in urban municipality	0.34	0.34	0.00	(0.4)
Foster mother characteristics				
Age	41.98	41.96	-0.02	(-0.2)
Primary school highest level	0.01	0.01	-0.00	(-0.1)
Secondary school highest level	0.62	0.62	0.00	(0.4)
Tertiary school highest level	0.38	0.37	-0.00	(-0.4)
Field: Education	0.05	0.05	0.00	(0.0)
Field: Health and welfare	0.39	0.39	-0.00	(-0.3)
Labor market participation in t=-1	0.83	0.84	0.01	(1.0)
Labor market participation in t=-2	0.84	0.85	0.01	(0.8)
Labor market participation in t=-3	0.86	0.86	0.01	(0.9)
Labor market participation in t=-4	0.86	0.86	0.00	(0.4)
Labor market participation in t=-5	0.86	0.87	0.00	(0.6)
Labor market income in t=-1	232,388.57	233,915.68	1,527.11	(0.5)
Labor market income in t=-2	229,808.38	232,253.07	2,444.69	(0.9)
Labor market income in t=-3	224,949.19	226,770.63	1,821.44	(0.7)
Labor market income in t=-4	221,563.54	223,103.24	1,539.71	(0.6)
Labor market income in t=-5	217,192.87	220,353.00	3,160.13	(1.2)
Foster father characteristics				
Age	44.29	44.25	-0.04	(-0.3)
Primary school highest level	0.01	0.01	0.00	(0.8)
Secondary school highest level	0.77	0.76	-0.00	(-0.3)
Tertiary school highest level	0.22	0.23	0.00	(0.1)
Field: Education	0.04	0.04	-0.00	(-0.3)
Field: Agriculture	0.07	0.07	-0.00	(-0.5)
Field: Health and welfare	0.07	0.07	-0.00	(-0.5)
Field: Services	0.05	0.05	0.01	(1.3)
Labor market participation in t=-1	0.86	0.86	0.00	(0.4)
Labor market participation in t=-2	0.86	0.86	0.00	(0.0)
Labor market participation in t=-3	0.87	0.87	0.00	(0.5)
Labor market participation in t=-4	0.87	0.87	0.00	(0.2)
Labor market participation in t=-5	0.88	0.88	0.00	(0.5)
Labor market income in t=-1	315,222.42	318,900.34	3,677.92	(1.0)
Labor market income in t=-2	316,176.27	318,374.22	2,197.96	(0.6)
Labor market income in t=-3	313,735.20	315,407.40	1,672.20	(0.4)
Labor market income in t=-4	307,403.42	308,839.45	1,436.03	(0.4)
Labor market income in t=-5	301,458.22	303,796.91	2,338.69	(0.6)
N	5,732	5,732	11,464	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table B.3: Baseline average outcomes, event time $t = -1$

	mean	(sd)	mean	(sd)
Yearly total household wage income	676,232.01	(262,848.64)	552,816.02	(279,909.79)
Yearly household non-FC wage income	547,611.00	(277,748.76)	552,816.02	(279,909.79)
Yearly household FC income	128,621.01	(181,741.97)	0.00	(0.00)
Female participation rate	0.83	(0.37)	0.84	(0.37)
Male participation rate	0.86	(0.35)	0.86	(0.35)
Observations	5,732		5,732	

Table B.4: Regression estimates from event model

	(1) Yearly total household wage income	(2) Yearly household labor market earnings	(3) Yearly household FC income	(4) Female participation rate	(5) Male participation rate
t_5	-79983.7***	-10470.3**	-69513.4***	0.0161**	-0.00217
	(-22.65)	(-2.92)	(-32.63)	(2.77)	(-0.44)
t_4	-61551.5***	-5719.4	-55832.1***	0.0142**	-0.00333
	(-20.11)	(-1.83)	(-28.47)	(2.68)	(-0.74)
t_3	-41139.8***	-1714.2	-39425.6***	0.0165***	-0.00319
	(-16.10)	(-0.66)	(-22.22)	(3.50)	(-0.82)
t_2	-20766.2***	784.9	-21551.1***	0.00593	0.000177
	(-11.01)	(0.40)	(-15.62)	(1.60)	(0.06)
t_1	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)
t0	71652.7***	-45337.2***	116989.9***	-0.0310***	-0.00507
	(34.41)	(-20.65)	(57.19)	(-7.21)	(-1.50)
t1	110433.9***	-119620.5***	230054.4***	-0.184***	-0.0370***
	(39.97)	(-40.93)	(88.82)	(-28.65)	(-8.04)
t2	101984.7***	-116038.7***	218023.4***	-0.188***	-0.0410***
	(30.50)	(-33.67)	(70.81)	(-27.34)	(-7.90)
t3	98510.9***	-114582.0***	213092.9***	-0.174***	-0.0399***
	(25.58)	(-29.36)	(62.43)	(-23.93)	(-6.99)
t4	93444.0***	-111995.7***	205439.7***	-0.171***	-0.0365***
	(21.60)	(-25.68)	(55.07)	(-22.36)	(-5.87)
t5	90551.6***	-109496.2***	200047.8***	-0.157***	-0.0396***
	(18.51)	(-22.66)	(50.02)	(-19.59)	(-5.81)
Observations	126,104	126,104	126,104	126,104	126,104

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Entry - Model 2

Alternative specification of the logit model, using only labor market covariates.

Figure B.3: Model 2, ATT

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
hus_sum_loen_~e2	Unmatched	788099.812	380664.52	407435.292	5042.4086	80.80
	ATT	788099.812	499138.738	288961.073	5642.91564	51.21
hus_sum_loen_~e3	Unmatched	779664.343	372819.383	406844.96	5094.90824	79.85
	ATT	779664.343	487175.474	292488.869	5814.51672	50.30
hus_sum_loen_~e4	Unmatched	776340.899	364242.683	412098.216	5138.34879	80.20
	ATT	776340.899	478113.883	298227.015	6083.96402	49.02
hus_sum_loen_~e5	Unmatched	771062.78	354919.044	416143.736	5175.58541	80.41
	ATT	771062.78	465754.639	305308.141	6335.06023	48.19

Table B.5: Balance, treatment vs control

	Treatment mean	Control mean	Difference b	t
At least one biological child living at home	0.77	0.48	-0.30***	(-34.5)
Average age of biological children living at home	9.61	5.82	-3.79***	(-29.3)
Live in urban municipality	0.34	0.60	0.26***	(28.7)
Foster mother characteristics				
Age	41.98	41.76	-0.22	(-0.9)
Primary school highest level	0.01	0.01	0.00**	(2.8)
Secondary school highest level	0.62	0.59	-0.03**	(-3.1)
Tertiary school highest level	0.38	0.28	-0.10***	(-11.4)
Field: Education	0.05	0.05	-0.00	(-0.6)
Field: Health and welfare	0.39	0.21	-0.19***	(-22.5)
Labor market participation in t=-1	0.83	0.88	0.05***	(7.5)
Labor market participation in t=-2	0.84	0.89	0.05***	(7.3)
Labor market participation in t=-3	0.86	0.90	0.04***	(6.8)
Labor market participation in t=-4	0.86	0.90	0.05***	(7.5)
Labor market participation in t=-5	0.86	0.91	0.05***	(8.2)
Labor market income in t=-1	232,388.57	235,472.71	3,084.14	(1.0)
Labor market income in t=-2	229,808.38	234,687.31	4,878.93	(1.6)
Labor market income in t=-3	224,949.19	232,042.01	7,092.82*	(2.4)
Labor market income in t=-4	221,563.54	229,133.50	7,569.97**	(2.6)
Labor market income in t=-5	217,192.87	226,752.77	9,559.90***	(3.3)
Foster father characteristics				
Age	44.29	40.47	-3.82***	(-13.0)
Primary school highest level	0.01	0.01	0.00	(1.3)
Secondary school highest level	0.77	0.72	-0.04***	(-5.2)
Tertiary school highest level	0.22	0.27	0.04***	(5.0)
Field: Education	0.04	0.04	-0.01	(-1.7)
Field: Agriculture	0.07	0.03	-0.04***	(-8.9)
Field: Health and welfare	0.07	0.03	-0.04***	(-8.1)
Field: Services	0.05	0.03	-0.02***	(-3.9)
Labor market participation in t=-1	0.86	0.90	0.04***	(6.4)
Labor market participation in t=-2	0.86	0.90	0.04***	(6.4)
Labor market participation in t=-3	0.87	0.91	0.04***	(7.4)
Labor market participation in t=-4	0.87	0.91	0.04***	(6.6)
Labor market participation in t=-5	0.88	0.92	0.04***	(6.5)
Labor market income in t=-1	315,222.42	289,091.52	-26,130.90***	(-6.0)
Labor market income in t=-2	316,176.27	292,076.74	-24,099.53***	(-5.6)
Labor market income in t=-3	313,735.20	294,084.48	-19,650.72***	(-4.6)
Labor market income in t=-4	307,403.42	294,161.17	-13,242.25**	(-3.1)
Labor market income in t=-5	301,458.22	291,333.84	-10,124.38*	(-2.4)
N	5,732	5,732	11,464	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Entry - Model 3

Robustness check of model 1 using 5 nearest neighbors in the matching procedure.

Figure B.4: Model 3, ATT

Variable	Sample	Treated	Controls	Difference	S.E.	T-stat
hus_sum_loen_~e2	Unmatched	788099.812	510637.31	277462.502	5512.58919	50.33
	ATT	788099.812	557434.809	230665.003	3923.03785	58.80
hus_sum_loen_~e3	Unmatched	779664.343	497683.025	281981.318	5609.00562	50.27
	ATT	779664.343	558931.787	220732.556	4103.92657	53.79
hus_sum_loen_~e4	Unmatched	776340.899	484139.096	292201.802	5691.78235	51.34
	ATT	776340.899	558450.238	217890.661	4320.8592	50.43
hus_sum_loen_~e5	Unmatched	771062.78	469915.595	301147.185	5768.95428	52.20
	ATT	771062.78	555663.128	215399.652	4482.2093	48.06

Table B.6: Balance, treatment vs control

	Treatment mean	Control mean	Difference b	t
At least one biological child living at home	0.77	0.78	0.00	(0.6)
Average age of biological children living at home	9.61	9.68	0.07	(0.7)
Live in urban municipality	0.34	0.35	0.01	(1.5)
Foster mother characteristics				
Age	41.98	42.09	0.10	(1.0)
Primary school highest level	0.01	0.01	-0.00	(-0.3)
Secondary school highest level	0.62	0.62	0.01	(1.1)
Tertiary school highest level	0.38	0.37	-0.01	(-1.1)
Field: Education	0.05	0.05	-0.00	(-0.1)
Field: Health and welfare	0.39	0.39	-0.01	(-0.9)
Labor market participation in t=-1	0.83	0.84	0.01	(1.1)
Labor market participation in t=-2	0.84	0.84	0.00	(0.6)
Labor market participation in t=-3	0.86	0.86	0.00	(0.8)
Labor market participation in t=-4	0.86	0.86	0.00	(0.8)
Labor market participation in t=-5	0.86	0.87	0.00	(0.5)
Labor market income in t=-1	232,388.57	234,052.81	1,664.24	(0.8)
Labor market income in t=-2	229,808.38	231,357.29	1,548.91	(0.7)
Labor market income in t=-3	224,949.19	226,855.79	1,906.60	(0.9)
Labor market income in t=-4	221,563.54	223,268.47	1,704.94	(0.8)
Labor market income in t=-5	217,192.87	219,594.95	2,402.08	(1.2)
Foster father characteristics				
Age	44.29	44.36	0.07	(0.6)
Primary school highest level	0.01	0.01	0.00	(1.0)
Secondary school highest level	0.77	0.77	-0.00	(-0.2)
Tertiary school highest level	0.22	0.22	-0.00	(-0.0)
Field: Education	0.04	0.04	-0.00	(-0.9)
Field: Agriculture	0.07	0.07	-0.00	(-0.7)
Field: Health and welfare	0.07	0.07	-0.00	(-1.0)
Field: Services	0.05	0.05	0.00	(0.2)
Labor market participation in t=-1	0.86	0.86	0.00	(0.5)
Labor market participation in t=-2	0.86	0.87	0.00	(0.7)
Labor market participation in t=-3	0.87	0.87	0.00	(0.5)
Labor market participation in t=-4	0.87	0.88	0.00	(0.8)
Labor market participation in t=-5	0.88	0.88	0.00	(0.7)
Labor market income in t=-1	315,222.42	319,007.11	3,784.69	(1.3)
Labor market income in t=-2	316,176.27	319,266.87	3,090.61	(0.9)
Labor market income in t=-3	313,735.20	315,416.02	1,680.82	(0.5)
Labor market income in t=-4	307,403.42	309,178.21	1,774.79	(0.5)
Labor market income in t=-5	301,458.22	304,339.33	2,881.11	(0.9)
N	5,732	27,359	33,091	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

C Solving the theoretical model

Foster parents maximize the utility function with respect to consumption, leisure and time spent foster parenting, and subject to the two constraints. The problem is formulated in a Kuhn-Tucker set up to allow for possible corner solutions and non-negativity constraints. We can write up the Lagrangian as

$$\mathcal{L} = c^{\mu}l^{\alpha} + \theta\gamma^{\mu}c^{\mu}F^{\alpha} - \delta\mathbb{1}\{\text{if } F > 0\} - \lambda(c - w(1-l-F) - \tau) - \lambda_F(1-l-F), \quad (7)$$

giving us the first order conditions with complementary slackness

$$\frac{\partial \mathcal{L}}{\partial c} : \mu c^{\mu-1}(l^{\alpha} + \theta\gamma^{\mu}F^{\alpha}) = \lambda, \quad c > 0 \quad (8)$$

$$\frac{\partial \mathcal{L}}{\partial l} : \alpha c^{\mu} l^{\alpha-1} = \lambda w + \lambda_F, \quad l > 0 \quad (9)$$

$$\frac{\partial \mathcal{L}}{\partial F} : \theta\gamma^{\mu}\alpha c^{\mu}F^{\alpha-1} \leq \lambda w + \lambda_F, \quad F \geq 0 \quad (10)$$

$$\frac{\partial \mathcal{L}}{\partial \lambda} : w(1-l-F) + \tau = c, \quad \lambda > 0 \quad (11)$$

$$\frac{\partial \mathcal{L}}{\partial \lambda_F} : 1 - l \geq F, \quad \lambda_F \geq 0. \quad (12)$$

The inequality constraints leave us with 3 relevant cases to consider;

a. Interior solution: $F > 0$ and $\lambda_F = 0 \implies l + F < 1$ and $L > 0$

b. No time in regular labor market: $F > 0$ and $\lambda_F > 0 \implies l + F = 1$ and $L = 0$

c. No foster parenting: $F = 0$ and $\lambda_F = 0 \implies l + F < 1$ and $L > 0$,

where the first two may be considered to describe the intensive margin of the foster care market, i.e., how much time to spend on foster parenting with respect to leisure and regular labor supply, and the third case is the extensive margin, i.e., opting in or out of the foster care market. Let us consider each of the cases in turn.

Case a: Interior solution
Solving the maximization problem when $F > 0$ and $\lambda_F = 0$ yields the following solution

$$l^* = \frac{1 + \frac{\tau}{w}}{1 + \frac{\mu}{\alpha}} \frac{1}{1 + (\theta\gamma^{\mu})^{\frac{1}{1-\alpha}}} \quad (13)$$

$$F^* = \frac{1+\frac{\tau}{w}}{1+\frac{\mu}{\alpha}} \frac{(\theta\gamma^\mu)^{\frac{1}{1-\alpha}}}{1+(\theta\gamma^\mu)^{\frac{1}{1-\alpha}}}. \tag{14}$$

Since the time constraint in this case is not binding, we get that

$$F^* + l^* < 1 \implies \frac{1+\frac{\tau}{w}}{1+\frac{\mu}{\alpha}} < 1 \implies \frac{\tau}{w} < \frac{\mu}{\alpha}, \tag{15}$$

such that, for this solution to be an optimum, we need the compensation-to-wage ratio to be smaller than the ratio of the consumption-to-leisure parameters. The regular labor market supply and consumption in this case is then given by

$$L^* = 1 - \frac{1+\frac{\tau}{w}}{1+\frac{\mu}{\alpha}} \tag{16}$$

$$c^* = w(1 - \frac{1+\frac{\tau}{w}}{1+\frac{\mu}{\alpha}}) + \tau \mathbb{1}\{\text{if } F > 0\}, \tag{17}$$

and the maximum utility is equal to

$$U^* = [\frac{w(1+\mu/\alpha)(1+\tau/w)}{1+\mu/\alpha} + \tau]^\mu (1+(\theta\gamma^\mu)^{1/1-\alpha})[\frac{1+\tau/w}{1+\mu/\alpha}\frac{1}{1+(\theta\gamma^\mu)^{1/1-\alpha}}]^\alpha - \delta. \tag{18}$$

Whenever the compensation to wage ratio is sufficiently low, parents will choose to spend part of their time in the regular labor market and part of their time in the foster care market. A higher compensation to wage ratio will increase the amount of time spent on foster care and leisure. The share of time spent foster caring with respect to leisure depends positively on foster parent's capacity for foster parenting, $(\theta\gamma^\mu) \uparrow \implies F^* \uparrow$, and negatively on the relative productivity of consumption and leisure in producing parental utility $(\mu/\alpha) \downarrow \implies F^* \uparrow$. In other words, a decrease in the relative utility weight on consumption with respect to leisure and foster care will tend to increase the amount of time spent foster parenting.

Case b: No time in the regular labor market
Solving the maximization problem when $F > 0$ and $\lambda_F > 0$ yields the following solution

$$l^* = \frac{1}{1+(\theta\gamma^\mu)^{1/1-\alpha}} \tag{19}$$

$$F^* = \frac{(\theta\gamma^\mu)^{1/1-\alpha}}{1+(\theta\gamma^\mu)^{1/1-\alpha}} \tag{20}$$

$$L^* = 0. \tag{21}$$

In this case foster parents will spend no time in the regular labor market. The marginal utility of leisure and foster care is greater than the marginal utility of consumption. This implies that foster parents would like to increase time spent on foster care and leisure in order to regain equality of marginal utility. However, this is not possible since parents are at the limit of their time constraint

$$F^* + l^* = 1 \implies \frac{1+\frac{\tau}{w}}{1+\frac{\mu}{\alpha}} = 1 \implies \frac{\tau}{w} \geq \frac{\mu}{\alpha}. \tag{22}$$

As shown, this case arises when the compensation to wage ratio is sufficiently high compared to the ratio of the preference parameters. As no time is spent on the regular labor market, consumption in this case is financed solely by foster care compensation

$$c^* = \tau. \tag{23}$$

The maximum utility is then given by

$$U^* = \tau^{\mu}(1 + (\theta\gamma^{\mu})^{1/1-\alpha})[\frac{1}{1+(\theta\gamma^{\mu})^{1/1-\alpha}}]^{\alpha} - \delta. \tag{24}$$

When the compensation for foster parenting is sufficiently high, parents will spend all their time on leisure and foster parenting, and no time in the regular labor market. As in case a, the amount of time spent on foster parenting with respect to leisure is determined by the capacity for foster parenting.

Case c: No foster parenting

Solving the maximization problem when $F = 0$ and $\delta_F = 0$ yields the following solution

$$l^* = \frac{\alpha/\mu}{1+\alpha/\mu} \tag{25}$$

$$L^* = 1 - \frac{\alpha/\mu}{1+\alpha/\mu} \tag{26}$$

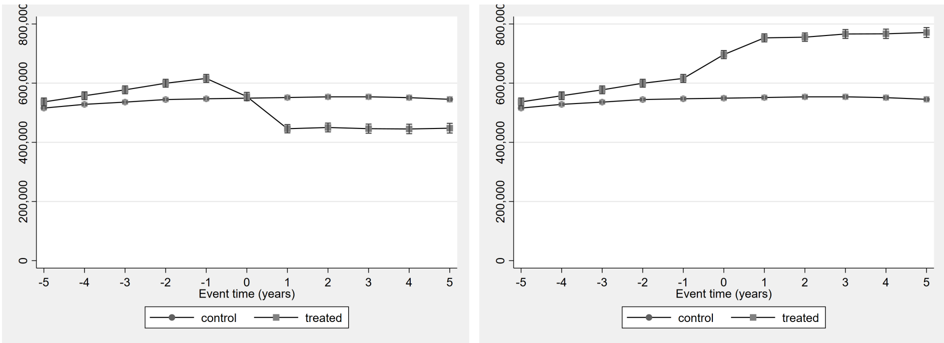
$$c^* = w(1 - \frac{\alpha/\mu}{1+\alpha/\mu}) \tag{27}$$

$$U^* = [w(1 - \frac{\alpha/\mu}{1+\alpha/\mu})]^{\mu}[\frac{\alpha/\mu}{1+\alpha/\mu}]^{\alpha}, \tag{28}$$

which represents the case where parents choose not to become foster parents. This case reduces to the most simple labor supply model where time is spent on either leisure or work in the regular labor market. The relative amount of time spent on these two activities depends on the preference parameters of the model, with leisure increasing in labor market skill (α) and decreasing in the preference for consumption (μ).

D Robustness of results

Figure D.1: Yearly household earnings at entry, no prior foster care compensation



(a) Average labor market earnings (b) Average total earnings

Note: The figure shows labor market earnings and total earnings for a subsample of foster parents who did not receive any foster care compensation prior to full-time fostering and for the matched control group.

Chapter 3

Trading off fiscal budget adherence and child protection

Trading off fiscal budget adherence and child protection

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Abstract

We investigate if budgetary constraints influence child protection decisions using high-quality register data. We show that the introduction of fiscal sanctions to improve budget adherence contributed to a sharp decline in budget overruns on child protective services by reducing the number of children in out-of-home care. Most countries have federal laws to protect children from maltreatment, but delegate child protection decisions to local administrative levels. Strict regulations to reduce budget overruns leave local governments with a potential trade-off between adhering to fiscal budgets and supplying critical welfare services to vulnerable children. Our results show that monthly variation in budget adherence affects the probability of a placement in out-of-home care for children who needs help towards the end of a fiscal year. We estimate that a budget overrun of 10 percentage points by mid-year leads to a 1.2 percent reduction in the number of children in care over the remaining part of the fiscal year. Municipalities reduced child protection expenditure by choosing cheaper types of care and ending placement for children in out-of-home care, particularly for children turning 18. Our paper contributes to the literature on fiscal federalism by documenting the trade-off between managing public expenditure and providing safety and equal opportunity for vulnerable children. We show that enforcing strict budget adherence can have unintended and potentially devastating side effects. The results raise an important discussion about centralization versus delegation of critical public services.

1. Introduction

Governments all over the world struggle to curb public expenditure. Tight budgets combined with severe economic sanctions for budget overruns have led to fiscal restraint at the local administrative level across all domains of public services in the last decade (Blom-Hansen, et al., 2016; Danish Economic Council, 2019; Houlberg, 2018). At the macro level, budgetary austerity has in some countries led to improvements of public budgets, providing the foundation for a reserve buffer. An important part of public budget savings can be ascribed to budget reductions at the regional and municipal level, as a substantial proportion of public budgets are delegated to local government levels.

The literature on fiscal federalism highlights several advantages to delegating public services to a local level (Gruber, 2011; Gruber & Sommers, 2020; Oates, 1999; Kornai, et al., 2003; Stiglitz & Rosengard, 2015; Scotchmer, 2002). *First*, it may be easier to collect information on the public needs at the local level. Child protective services often rely on referrals from the local community, such as neighbors or schoolteachers, to identify children in need of help. *Second*, there may be an advantage to locally organized services, for example in supporting parents or recruiting a foster family close to the biological parents' home. *Third*, if local government handles the provision of public goods, they can more easily adapt to the preferences of local citizens. An argument referred to as the "Tiebout hypothesis" (Tiebout, 1956; Boadway & Tremblay, 2012), which states that citizens can signal their preference for public goods by moving to a local jurisdiction matching their preferences.

Recent literature questions the basic rationale behind decentralizing public services from the perspective of three concerns: efficiency, equity, and accountability (Arends, 2020). Decentralization offers specific challenges for public expenditures related to for example health and education (Boadway & Tremblay, 2012). It has been shown that the demographic composition of local areas determines local public expenditure levels. For example, Figlio & Fletcher (2012) document that the percentage of elderly adults in a school district is negatively related to the amount of support for public schooling.

While fiscal budgets stipulate the overall frame for public expenditure at the national and local administrative level, the provision of some public services is also regulated by law. Local governments may thus face a trade-off between showing fiscal restraint and supplying critical, law-mandated welfare services to vulnerable citizens. Child protection is a particularly important and relevant example of this trade-off. Child maltreatment has severe negative consequences for child health and well-being (Currie & Widom, 2010; Paxson & Waldfogel, 2002; Currie & Tekin, 2012; Doyle, 2013; Doyle, 2008; Doyle, 2007). Each year almost 1 percent of US children spend time in care and 6 percent of US children have been placed in foster care at least once before turning 18 (Turney & Wildeman, 2016). In most Western countries, federal child protection laws aim to improve equity in conditions and secure basic rights and safety for at-risk children. In practice, the administration of these laws is usually delegated to a local level such as municipalities or counties. Even small deviations from the expected number of children who receive costly interventions such as out-of-home care can lead to a budget overrun in a small municipality. This can leave the local administration in a conflict between budgetary adherence and the statutory responsibility to take action when a child needs help.

The research question we answer in this paper is whether variation in local budgetary pressure – *within* a fiscal year - influences child protection decisions. We investigate the question empirically by examining whether municipalities that have spent a larger than expected fraction of their budget on child protection subsequently reduce the number of children in care. We use a rich set of individual-level data to examine the mechanisms through which municipalities reduce expenses on child protection. We explore the causal effects of a reform, effective since 2011, that implemented sanctions on individual municipalities for overrunning budgets. To quantify our estimation results, we calculate that municipalities that have spent more than 60 percent of their budget by the middle of a fiscal year reduce the number of children in out-of-home care by 1.2 percent by the end of the year. The results also suggest that the introduction of fiscal sanctions contributed, although only moderately, to a decline in budget overruns on spending allocated to vulnerable children and, unintentionally, affected the provision of child protective services. Municipalities reduced child protection expenditures by choosing cheaper types of out-of-home care and by being more likely to end out-of-home care, particularly after a child's 18th birthday, after which the municipality is no longer legally obligated to provide child protective services. To test the robustness of our results, we exploit a reform that incentivized municipalities' budget adherence. Moreover, we perform a placebo test to rule out that the results we find are due to mean reversion or deliberate timing of municipal activities over the year.

Our paper contributes to the literature on fiscal federalism by documenting a trade-off between, on the one hand, providing effective and credible measures to curb local government expenditure and, on the other hand, ensuring safety and equal opportunities for vulnerable children. While variation *across* municipalities in the services provided is well known from other studies (Andersen, 2010), we document that *within-municipality* variation in expenditure over the fiscal year has an additional effect on the services offered to at-risk families. Our primary contribution is to show that the *timing* - within a fiscal year - of municipal expenditures affects the provision of child protective services.

Our results highlight important side effects of imposing strict budget adherence at the local level and contributes to a policy discussion about centralization versus delegation of critical public services. To design an effective public sector, knowledge of how local governments deal with conflicting requirements is key and can help delegate public services to the most appropriate administrative level. It is important to know if policies designed to uphold fiscal stability have unintended consequences, particularly if it affects vital welfare tasks such as child protection. Whereas the traditional advantages of decentralization such as local information and local organization clearly apply in the case of child protection, we argue that it is unclear whether it is a good idea to adapt the provision of child protection to local demand. Child protection receives less attention in the local public debate than large welfare goods such as health and education, and it is rarely high on the political agenda for local elections. Demand for child protection from citizens not directly involved may suffer from a lack of information, as is often the case in areas characterized by stigma and only a small fraction of the population is directly involved with child protective services. The biological parents of at-risk children may prefer no public intervention, and the children do not have the power to 'vote with their feet' as the Tiebout hypothesis would suggest.[1] If society's main concern is children's safety and well-being, adapting the provision of child protective services to local preferences may be problematic.

2. Background and institutional setup

Financial stability of local government budgets is at the center of fiscal policy in many Western countries. Legislation implemented in 2011 in Denmark imposed expenditure ceilings on municipal budgets and spending. The expenditure ceilings

[1] For a theoretical discussion of the utility functions in the Tiebout model see (Rubinfeld, 1987).

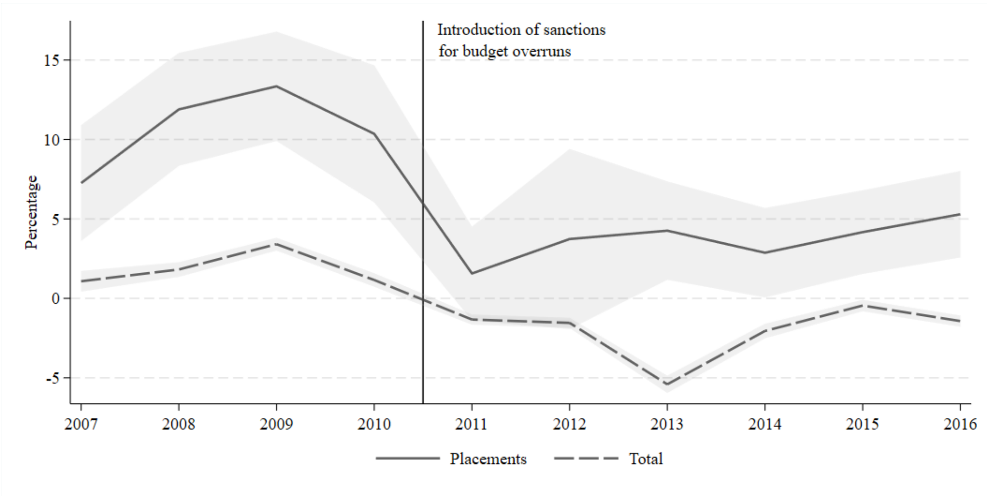
are enforced through economic sanctions if the municipal budget exceed the centrally mandated budget or if municipal expenditures exceed the budget. Individual municipalities that overrun their annual budget are required to pay 60 percent of the sanction imposed on the municipalities as a whole (Danish Economic Council, 2019; Bæk, et al., 2016). Since municipal budgets have complied with the expenditure ceilings it has not so far been necessary to use sanctions as a disciplinary instrument.

Municipalities make separate budgets for all their activities, including spending on out-of-home care, which amounted to about 2.4 percent of total municipal expenditures in 2016. The municipalities' budgets on out-of-home placement increased from 2007 to 2009, followed by a reduction after 2010, coinciding with the financial crisis and the national efforts to increase financial stability. Budgets vary substantially across municipalities, with budgets around 6 million Euro for municipalities at the 25th percentile, and budgets around 13 million Euro at the 75th percentile, depending, among other things, on the size of the municipality (see Figure A3 in the appendix).

The Danish child protection law stipulates that the main goal of child protective services is to support at-risk children to "obtain the same opportunity for personal development, health and an independent adult life as their peers" (Law on Social Services - *Serviceloven*, Ch. 11, §46). Municipalities are responsible for the care of vulnerable children in Denmark and may assign a range of interventions, from various types of preventive action to out-of-home care as the most drastic intervention. Out-of-home care is intended to be a temporary intervention and the final goal is reunification with the biological parents. Similar to numbers in the US, almost 1 percent of Danish children aged 0-17 spend time in out-of-home care each year (Ejrnæs & Gørtz, 2017; Ejrnæs & Gørtz, 2017). Since 2007, the 98 Danish municipalities have had full fiscal responsibility for at-risk children and it is the municipality's decision to place a child in out-of-home care. Parents can appeal the municipality's decision to the National Social Appeals Board (Svendsen, 2017). Child protection mainly concerns children aged 0-17, however the municipality can extend the out-of-home placement up to the age of 22. Around two thirds of children in care live in family foster care and a third live in institutional care (own calculations on register data, see Data section for details). For children exiting care, the average length of care was around four years per placement. Less than one third of children in out-of-home care return to their biological parents before age 18, and the rest "age out" of care at age 18 or over. Many children transition from one type of care into another, and children experienced an average of 1.4 placements. According to a recent survey based study among caseworkers in Copenhagen (Ejrnæs & Gørtz, 2017), the most common reasons for placing a child in out-of-home care is parental neglect (50 percent) and child externalizing behavior and social adjustment issues (33 percent). Less frequent reasons are violence or threats of violence (10 percent) or sexual abuse (2 percent).

Out-of-home care is generally very costly, but costs vary considerably by type of placement. The average cost for a child in institutional care amounts to more than 150,000 Euro annually, while the cost of a foster family is around 68,000 Euro annually (details on prices for child protection programs in appendix, Table A1). For small municipalities, placing one additional child in out-of-home care is likely to pose a serious threat to budgetary compliance. Three additional (unexpected) children in institutional care would result in a budget overrun of almost 5 percent for the median municipality and more than 9 percent for the 25 percent smallest municipalities.

Figure 1: Budget overruns on out-of-home care



Note: The graphs shows the average of annual actual expenses - expenses in the budget relative to the budget. The solid line is for expenses on out-of-home placement whereas the dashed line is for all municipality expenses. The vertical line indicates the introduction of sanctions against budget overrun. The shaded area indicates the 95 percent confidence interval. Source: Own calculations based on data from Statistics Denmark, Statistikbanken, Table BUDK53 and REGK53

Figure 1 shows the average budget overrun on the budget for out-of-home care and the total budget for Danish municipalities in our sample period. The figure shows that while municipalities were heavily overspending on out-of-home placements from 2007 to 2010, overspending dropped from around 13 percent at its peak in 2009 to around 2 percent in 2011 after the introduction of sanctions on overspending. While there is an increase in budget overruns from 2007 to 2009 on most municipal activities and then a reduction in deficits after 2010, the changes over time are much stronger for out-of-home care than for the other areas (see details in the online appendix).

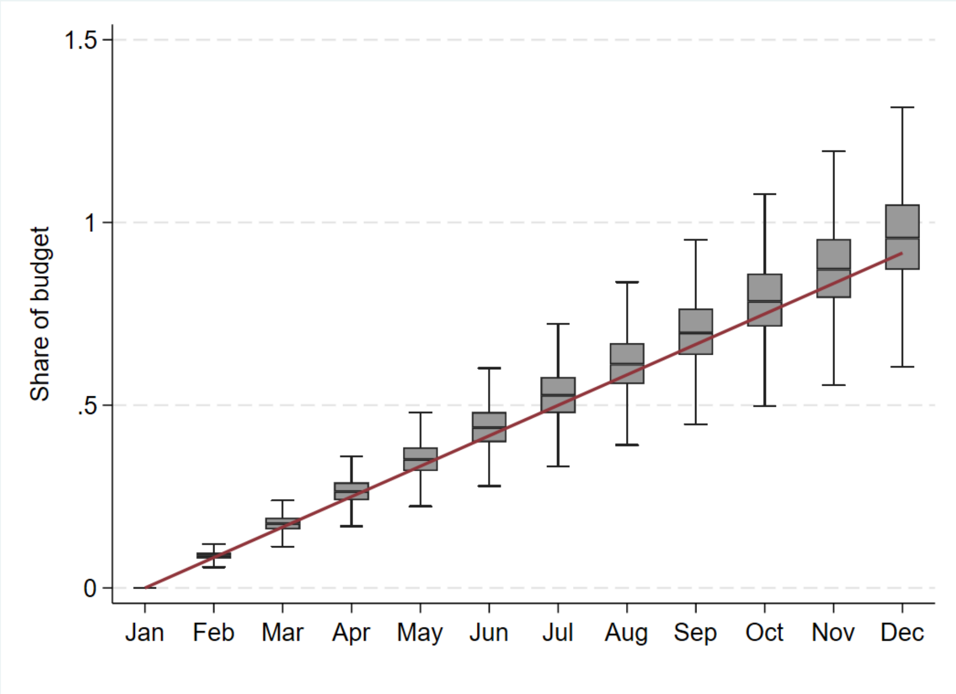
The averages mask considerable heterogeneity in over- and underspending across municipalities. Budget overruns occurred in municipalities in various parts of the country before 2010, while they were much less widespread after 2011 (see map of municipalities with budget overruns before and after 2011 in the online appendix). Almost all municipalities (95 percent) overspent on their out-of-home care budgets in at least three of the ten years in our sample period.

3. Data and methods

Our empirical analyses use unique Danish register data, a longitudinal dataset with individual-level information on socioeconomic characteristics including family status, education, income, government transfers and child protective services such as preventive action, out-of-home care spells and type of care. The sample consists of all children and youth receiving preventive action or out-of-home care during the years 2007 to 2016. Data cover 97 Danish municipalities (excluding the capital Copenhagen due to its size). The median Danish municipality had 120 children in out-of-home care in 2007. This number had decreased to 110 in 2016.

A crucial measure for our analyses is the municipalities' monthly expenditure for out-of-home care. Data on municipalities' budgets and accounts are annual, so we construct a measure of monthly spending using individual-level data. The individual register information allows us to calculate exactly how many children were in out-of-home care each month in each municipality. Furthermore, we can divide these "care months" according to the type of care. Based on the number of "care months" of each type of care and the average prices per care type, we impute a measure of monthly expenditure for municipalities (see more details in the online appendix). We construct a monthly variable "the budget share", which measures the cumulative monthly expenditure as a fraction of the planned annual budget. As an example, the budget share on March 1st is the sum of imputed expenses for January and February divided by the annual budget for that year. The budget share will be larger than one if a municipality's cumulative expenditures for out-of-home care in a given month exceeds the total annual budget. Figure 2 shows boxplots of the imputed budget share at the first day of the month over the fiscal year for all municipalities in the period 2007 to 2016. The budget share is by construction zero by January 1 for all municipalities. By October 1, the median municipality had spent about 80 percent of the annual budget, while some municipalities had spent substantially more and a few had already spent the entire annual budget. The divergence across municipalities in over- or underspending increases over the fiscal year.

Figure 2: Municipalities' expenditure as share of the budget, 2007-2016



Note: Budget share is measured at the beginning of the month. For January, the budget share is always zero. For February, the budget share is defined as the proportion of the total budget for year *t* that was used in January. For March the budget share is calculated as the share of the January and the February expenditure out of the total budget, etc. Finally, for December, the budget share is defined as expenditure share of the months January to November in that year. The red line indicates the expected budget share, if the expenses were equally distributed across the months.

Our identification strategy consists of two approaches to estimate the effect of budget shares on out-of-home placement decisions. The first approach uses municipal level data and allows us to quantify the total effect of budget shares on the number of children in out-of-home care. The second approach uses individual-level data to investigate the mechanisms to reduce expenditures on out-of-home care. Both analyses rely on a comparison of high- and low-spending municipalities across time. Consider a simplified illustration of our approach. There are two municipalities, A and B. We imagine that municipality A at the beginning of July that year had spent more than 60 percent of its budget on out-of-home care. Hence, municipality A is a high-spending municipality in that given year. Low-spending municipality B had spent only 50 percent of its out-of-home care budget by July 1 that same year. While municipality A had to cut expenditures for out-of-home care in the last six months of the year to stay within the budget, municipality B did not need to adjust its expenditures. Imagine that while municipality A had spent more than 60 percent of its out-of-home care budget by July 1 in year 1, it had only spent 50 percent of the budget in year 2. Municipality A was high-spending in year 1, but it was low-spending in year 2. Our analysis also exploits within-municipality variation across years. Using both within and between municipality variation allows us to account for both municipality fixed effects and calendar time fixed effects. It is important to note that although this stylized example considers a budget share threshold of 60 percent on July 1 our approach measures the effect of a marginal increase in the budget share at any value and for all months of the year. We will now describe each of the two approaches in more detail.

The *municipality-level* analysis estimates of the total effect of budget shares on the number of children in out-of-home care. The analysis builds on an error correction model, where the number of children in out-of-home care, y_{kt}, in municipality *k* in month *t* depends on the long run municipality specific mean, μ_k, and the budget share.

$$\begin{aligned}\Delta \log y_{kt} = {} & \alpha_1(\mu_k - log y_{kt-1}) \\ & + \alpha_2(Z_t - Budgetshare_{kt-1}) + \theta_t + \varepsilon_{kt}\end{aligned} \tag{1}$$

where *Budgetshare* is defined as the share of the budget for out-of-home care that has been spent from January 1 until month *t*-1. We interpret the long run mean as the expected number of children in need of out-of-home care in the municipality. Z_t measures the expected seasonal profile of the budget share, and θ_t contains year and month dummies. We hypothesize that the number of children in care adjusts to the long run mean, μ_k, and to the deviation between actual expenditure and the expected seasonal profile. We expect $\alpha_1 > 0$, suggesting that the number of children in care converges to the long run mean. We also expect $\alpha_2 > 0$ if deviations from the expected spending patterns lead to an adjustment in the number of children in care. We estimate the model using the following regression model

$$\Delta logy_{kt} = \alpha_0 - \alpha_1 logy_{kt-1} - \alpha_2 Budgetshare_{kt-1} + \tau_k + \varphi_t + \varepsilon_{kt} \qquad (2)$$

where τ_k contains municipality dummies and $\varphi_t = \alpha_2 Z_t + \theta_t$ contains year and month dummies. The municipality-level analysis exploits variation over time within municipalities to estimate the effect of an increase in the monthly budget share on the number of children in out-of-home care.

The *individual-level* analysis provides information on which margins municipalities reduce their expenditures. There are three relevant outcome margins to consider; the municipality can reduce expenditures by either ending placement for children already in care, placing fewer children in care or by choosing a less expensive placement for children who are placed in care. We estimate the effect of budget share on the probability of ending out-of-home placement for children already placed in out-of-home care separately for children younger than 18 years old and children who turn 18. We estimate the probability of initiating a new out-of-home placement separately for children who already receive a preventive care measure and for children who obtain either a preventive care measure or an out-of-home placement. We estimate the probability of choosing a less expensive type of care for all children placed in out-of-home care. For more details on sample selection, see appendix table A2. We estimate the relationship between the probability of each individual outcome and monthly budget shares using a logit specification:

$$y_{it}^{*} = \gamma \cdot Budgetshare_{kit-1} + X_{it}\beta + \mu_k + \theta_t + \varepsilon_{it}, \qquad (3)$$

where y_{it}^{*} is the latent outcome. *Budgetshare* is defined as the share of the annual municipal budget for out-of-home care spent by month *t*-1. X_{it} includes socioeconomic characteristics, such as the child's gender, age, birthweight, dummy for ethnic minority background, whether mother or father is not on record, and maternal characteristics (age, income, labor market status, education and marital status). We include a full set of municipality dummies (μ_k), and year and month effects (θ_t). Standard errors are clustered at the municipality-level. The individual-level analysis exploits variation within municipalities and across time to estimate the effect of an increase in the monthly budget share on the individual probability of ending out-of-home placement, initiating a new placement or choosing a cheaper placement type. This approach allows us to investigate how municipalities adjust the number of children in care in response to a higher budget share.

A potential threat to the identification strategy comes from mean reversion. If some municipalities deliberately organize their work over the year to place relatively many children in out-of-home care in the first half of the year for administrative reasons or seasonal variation, we may observe a high budget share by July 1 followed by fewer placements in the second half of the year. This would show up in our results in the same way as an effect of budget overrun on the number of children placed in care. We perform three different investigations to rule out that our results are driven by mean reversion.

First, our municipality-level regression explicitly controls for mean reversion by estimating an extended error correction model for the change in number of children in care. We allow each municipality to converge to their own long run mean for the number of children in care. In this way, we explicitly test for mean reversion by including the lagged number of children in care in our estimation.

Second, the reform in 2011 encouraged municipalities to pay more attention to the budget after 2011. If municipalities reacted more strongly to budget overruns after 2011, this would strengthen the interpretation that municipalities reduce out-of-home placements towards the end of the year due to the risk of budget overruns. To explore the effect of the 2011 sanctions, we interact the lagged *Budgetshare* with a dummy for years after 2011.

Third, we perform a placebo test where we assume a hypothetical fiscal year running from July to June. If potential effects on out-of-home care were the result of seasonal patterns or deliberate organization of activities in municipalities, we would also find an effect of budget shares on placement rates using the alternative fiscal year running from July to June.

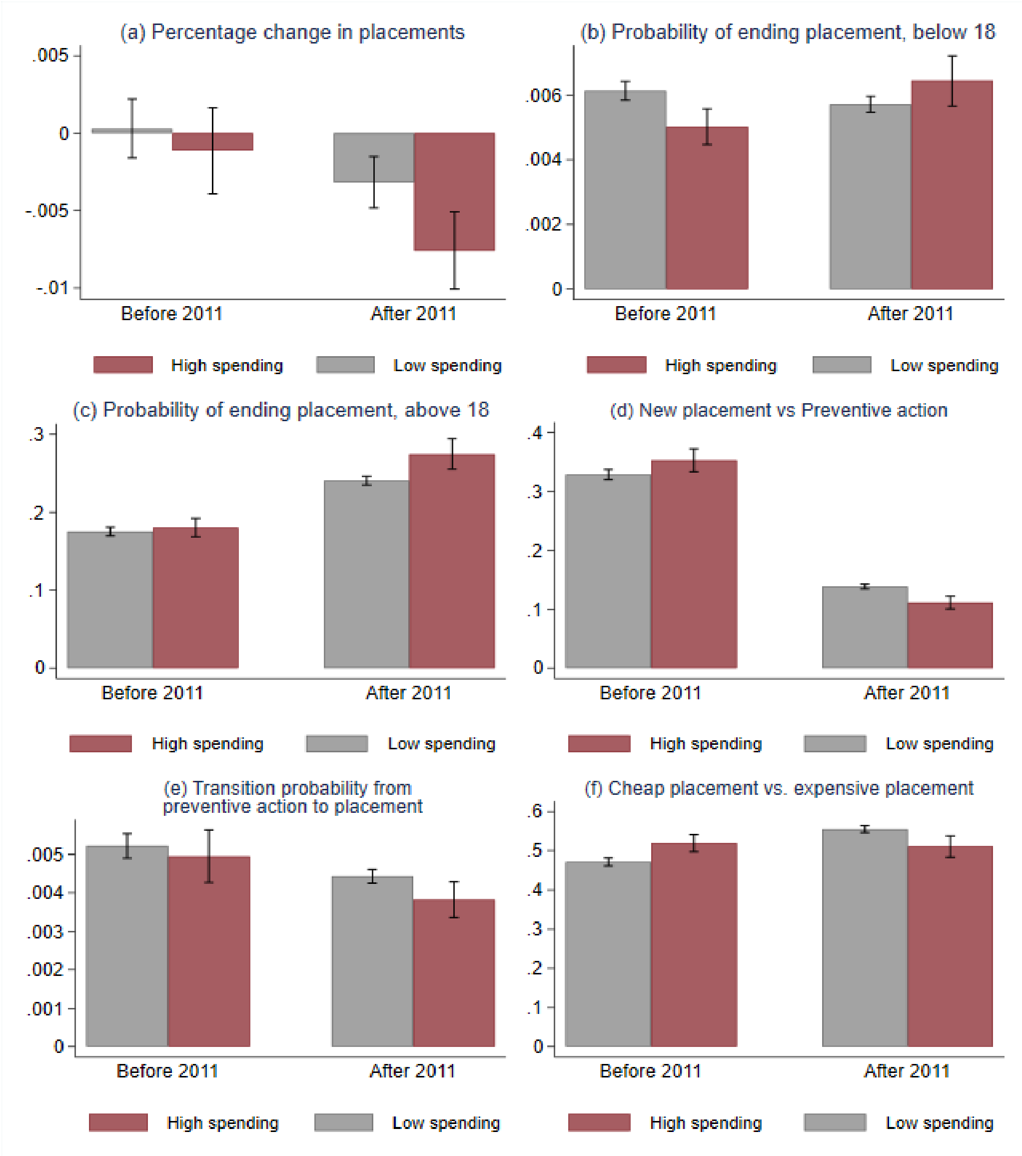
4. Results

4.1.Municipal-level analysis

We start by showing descriptive evidence on how municipalities adjusted their spending in the second half of a budget year following a budget overrun in the first half of the year. We split the municipalities into two groups according to their spending patterns in the first six months of the fiscal year. The high-spending group consists of municipalities that spent more than 60 percent of their total annual budget by July 1. The low-spending group consists of municipalities that spent less than 60 percent by July 1.[2] Figure 3(a) depicts the monthly change in the number of children in out-of-home care for the second half of the fiscal year, i.e., from July to December for low and high-spending municipalities, for the periods before and after 2011, the year of the municipal budget reform. Figure 3(a) clearly documents two findings. First, there is a reduction in placements in out-of-home care after 2011 for both high- and low-spending municipalities. Second, the number of placements dropped significantly more for high-spending municipalities compared with low-spending municipalities after 2011. In total, the number of children in out-of-home care dropped, on average, 4.5 percent from July to December (a monthly decrease of around 0.75 percent).

[2] We have also looked at the difference between high- and low-spending municipalities using a different threshold value, and this does not change the conclusion. The descriptive evidence illustrates the effect we are interested in, but we do not know if the effect is causal. To estimate the causal effect of a marginal increase in budget share we turn to the more rigorous municipality-level analysis.

Figure 3: The monthly probability of ending and initiating out-of-home care



Note: High (Low) spending municipalities in year *t* are defined as municipalities that in year *t* have spent more (less) than 60 percent of the annual budget by July 1st. Graph 4 (a): Outcome is changes in log number of children in care. Graph 4. (b) and 4 (c): Outcome variable is monthly probability of ending an out-of-home placement for children below (above) 18. Graph 4 (d): Outcome variable is monthly probability of initiating an out-of-home placement. Graph 4 (e): Outcome is probability for a child (below 18) who is not currently receiving preventive actions and the left panel is for a child that receive preventive actions. Graph 4 (f): Outcome is probability of choosing a "cheap" placement instead of an expensive placement. Confidence bands at 0.05 level.

We examine the impact of the budget share in the error correction model in equation (1) using monthly data at the municipality level. Estimation results are shown in Table 1, column 1. There is a negative and significant effect of the lagged log of number of children in a municipality in period t-1 on the change in the number of children in out-of-home care from period t-1 to t. This indicates that there is mean reversion to a municipality specific "long run" mean of number of children in care. Our main interest lies in the effect of the budget share in period t-1 on change in the log number of children in out-of-home care in period t. We estimate that if the budget share is 10 percentage points higher than expected, the number of children in care decreases by 0.2 percent per month. This is equivalent to a 1.2 percent reduction in the number of children in out-of-home care over half a year. The effects are not significantly different when comparing before and after 2011 (see column 2 in Table 1) in the municipal-level analysis. Column 3 in table 1 shows the result from estimating equation (2) using a placebo fiscal year and we return to this in the end of the section. We also consider if the budget share affects the number of children receiving preventive actions such as e.g. support for the family. These interventions are often used to avoid out-of-home placement. The analysis shows (see column 4, Table 1) that municipalities at risk of budget overrun lower the number of children receiving preventive actions after 2011 but not before 2011. If the budget share is, e.g., 10 percentage points higher than expected, the number of children receiving preventive actions decreases by 0.9 percent the following month after 2011.[3]

[3] We thank the referee for suggesting this additional analysis.

Table 1: Estimation results, municipality-level analysis

	(1)	(2)	(3)	(4)
Outcome (y_{kt})	Num. of children in care			Num. of children w preventive actions
	Baseline	Estimation w interaction	Placebo estimation	Estimation w interaction
$\log(y_{kt-1})$	-0.023***	-0.023***	-0.034***	-0.051***
	(0.005)	(0.005)	(0.006)	(0.005)
Lag Budget share	-0.017*	-0.017*	0.009	0.011
	(0.007)	(0.006)	(0.005)	(0.006)
Lag Budget share × D2011		0.003		-0.020***
		(0.002)		(0.002)
$\log(y_{kt-1})$ × D2011		0.000		-0.002
		(0.001)		(0.002)
Constant	0.114***	0.114***	0.166***	0.400***
	(0.026)	(0.024)	(0.028)	(0.035)
Year dummies (9)	Yes	Yes	Yes	Yes
Month dummies (11)	Yes	Yes	Yes	Yes
Muni. Dummies (96)	Yes	Yes	Yes	Yes
N	11,531	11,531	10,464	10,464
R^2	0.034	0.035	0.038	0.178

Note: The estimation results refer to estimation of equation (2) with the dependent variable being $\Delta log y_{kt}$. The model is estimated using monthly data for the period 2007-2016 at the municipality level. The data set consists of 97 municipalities (The municipality of Copenhagen is excluded). In column (1)-(3) the outcome is number children in care and in column (4) the outcome is number of children receiving preventive actions. Colum (3) contains a placebo test where the budget share is replaced by a hypothetical budget share (for more details see Appendix A.8). D2011 is a dummy for the period 2011-2016. The estimations include 11 month dummies, 9 year dummies and 96 municipality fixed effects. Standard errors in brackets are clustered at the municipality level.*, **, *** indicate significance at 0.05, 0.01 and 0.001 level.

4.2.Individual-level analysis

We now examine the mechanisms through which the municipality adjusts its expenditure on child protection. We consider five different outcomes: ending a placement for children aged below 18, ending a placement for children above 18, initiating a new placement, initiating a placement for children who receive preventive actions and choosing a less expensive placement versus an expensive placement (see details in the appendix).[4] Each of the five outcomes represents a margin where the municipality can adjust their expenditure on child protection. Figure 3 (b) to 3 (f) show descriptive evidence on the margins. The figure shows differences in the probability of each of the five outcomes in the last six months of a fiscal year (July to December) for high- and low-spending municipalities, before and after 2011. The probability of ending a placement in July to December is conditional on spending in January to June. Figures 3 (b) and 3 (c) show that the probability of ending a placement increased, for children under 18 (Figure 3 (b)) and children above 18 (Figure 3 (c)) after 2011. The largest difference is for youngsters above 18. The probability of out-of-home care reduced after 2011 for out-of-home care versus preventive action (Figure 3 (d)) and for the transition from preventive action to out-of-home placement (Figure 3 (e)). We also observe a substitution away from the more expensive institutional care to the less expensive foster family care, especially after 2011 (see Figure 3 (f)). High-spending municipalities were more likely to end placements after 2011, especially for young adults, and they were less likely to initiate new placements than before 2011.

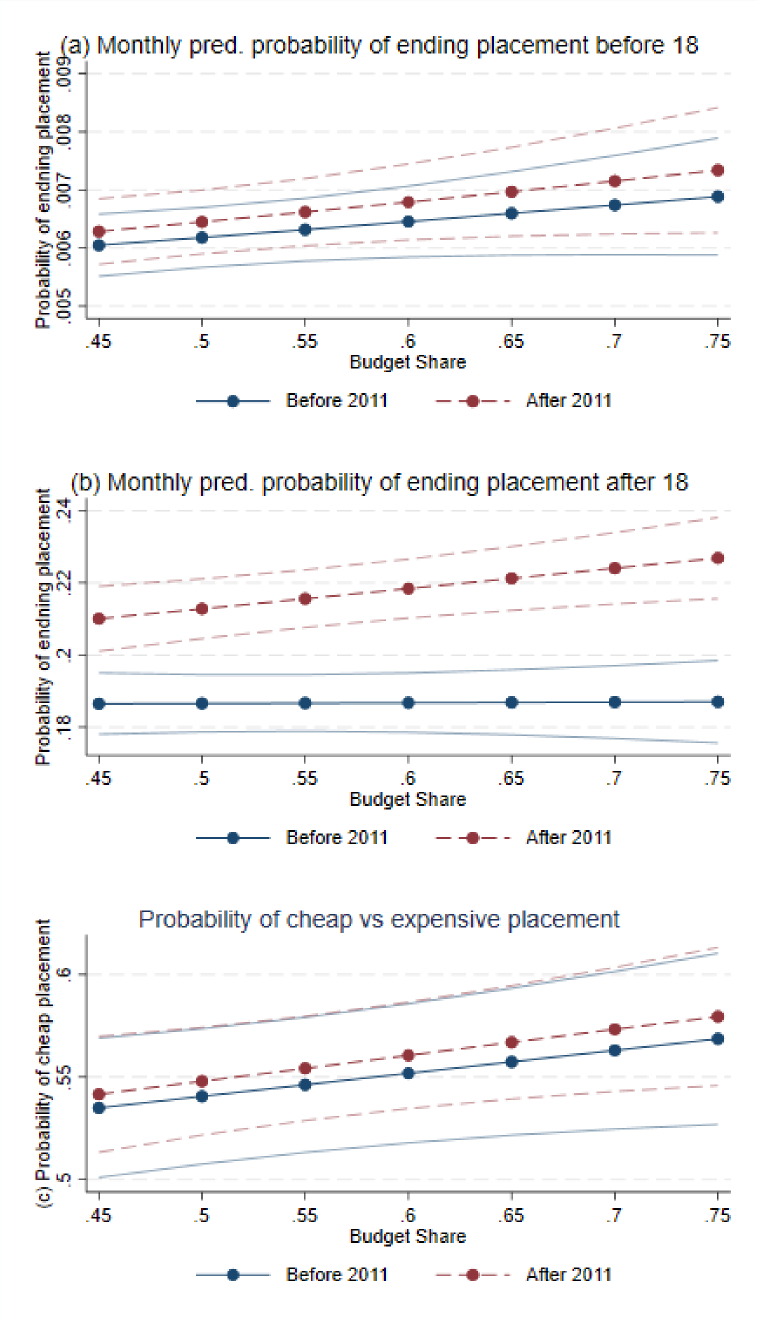
[4] The less expensive care is primarily foster families while the expensive care is institutional care.

We examine the effect of the budget share on the five outcomes in a logit estimation, with year, month and municipality dummies and a number of individual socioeconomic characteristics, such as child gender, age, and birthweight, marital status of parents, and mother's age, education and employment (detailed estimation output in the online appendix, table A4).

4.3.Marginal effects

Figure 4 illustrates the estimated marginal effects by budget share on July 1 from model (2). Figure 4 shows the three outcomes with significantly estimated effects: ending out-of-home care for children below and above 18 and choosing a less expensive type of out-of-home care. Figure 4(a) shows the likelihood of reunification for children in care as a function of the share of the budget spent by the municipality by July 1. The figure indicates that the monthly probability of reunification is 0.65 percent if a municipality has spent 50 percent of its budget by July 1, while it was 0.7 percent if the municipality had spent 60 percent of its budget. Figure 4(b) shows that the probability of ending out-of-home care placement for children turning 18 was significantly higher after 2011 than before, for all levels of budget shares at July 1. To quantify these effects, municipalities that, after 2011, had spent 10 percentage points more of their budget by mid-year (e.g., having spent 60 rather than 50 percent), had a 0.5 percentage point higher propensity to end placement for over 18-year olds. When compared with an average probability of ending out-of-home care for children who turn 18 of around 21 percent, this is equivalent to a 2-3 percent increase in the probability of ending out-of-home care for over 18-year olds. For the choice between a less expensive and an expensive placement (Figure 4(c)), we see that a higher budget share increases the probability of the less expensive placement, but the difference is small and statistically insignificant when comparing before and after 2011. After 2011, the effect of a higher budget share is stronger for all outcomes. For all levels of budget shares , the probability of ending placement after age 18 is significantly higher after 2011 as compared to the situation before the 2011-reform (Figure 4(b)).

Figure 4: Predicted probabilities by budget share, individual-level regression



Note: The Figures show the marginal effects of the budget share on the individual outcomes. Panel (a) shows the predicted monthly probability of ending a placement for children in care below 18. Panel (b) shows the predicted monthly probability of ending a placement for children above 18 in care. Panel (c) shows the predicted probability for a cheap out-of-home placement (e.g. foster families) instead of an expensive (institutional care). The predictions are based on estimates of the logit specification in equation (3). The predicted probabilities are calculated from July. The logit model is estimated using individual-level data from the period 2007-2016.The dashed lines indicate the 95 percent confidence interval.

4.4.Heterogeneous effects and robustness

We find suggestive evidence of heterogeneity in how sensitive municipalities are to budget concerns. The individual-level analyses show that budget concerns have a smaller impact on out-of-home care decisions in municipalities where the majority in the municipal council consists of parties on the center-left or if less than 30 percent of the municipality council are women. We also find that municipalities who have sizable debt are more sensitive to the risk of budget overrun and that municipality in election years are more sensitive to risk of budget overrun. The last result is surprising but may be due to the fact, that child protection rarely is a topic in local elections and politician may prioritize topics of greater importance to the majority. We looked for heterogeneous effects in other dimensions: population size, number of placements by 1. January 2007, a measure of needs of local residents, educational composition, fraction of single parents, average income (for a detailed description of the all four measures see the Appendix Table A5). Neither of the measures seem to matter for the size of the effect. The municipality-level analysis shows no indication of heterogeneous effects for any of the variables.

To test the robustness of our results, in particular whether our results could be driven by the way municipalities organize their work with at-risk children over the fiscal year (mean reversion), we run our analysis using a placebo budget share. We hypothesize that the fiscal year runs from July 1 to June 30 the following year (see Figure A8). We define a placebo annual budget as the average budget for two consecutive calendar years. We construct the placebo budget share for each month from the monthly expenditures and the placebo annual budget as described in the data section. We repeated our analyses with the placebo budget share instead of the real budget share. The placebo test indicated no effect of budget overruns in our "placebo" fiscal year. This confirms that our main results are not driven by mean reversion (see Table 1, column 3 for the municipality-level estimations, and appendix Figure A8 for the individual-level estimations).

5. Discussion and conclusion

Throughout the world, governments struggle to curb public expenditure at all administrative levels. In Denmark, this has led the government to impose fiscal sanctions on local municipalities who overrun their annual budgets. As a large share of public expenditure is spent at local levels – municipalities and regions – budgetary restraint at the local levels is important. In this paper, we show that such fiscal sanctions can have substantial effects on essential welfare services that are designed to protect some of the most vulnerable citizens, namely children at risk of neglect and maltreatment.

While other studies have studied the variation *across* municipalities in service levels (Andersen, 2010), we document that *within-municipality* variation in expenditure over the fiscal year impacts services provided to families. Thus, an at-risk child may receive differential treatment depending on whether information about the case is brought up in December or January. Using individual-level administrative register data, we show that municipalities at risk of overrunning their budget reduced the number of children in out-of-home care. We find that a 10 percentage points increase in the budget share by July decreased the number of children in out-of-home care by 1.2 percent by the end of the fiscal year. The detailed empirical analysis on individual-level data shows that municipalities primarily reduced the number of children in care by ending care for children in out-of-home care and by using less expensive types of out-of-home placement. The results show that local policy makers face a trade-off between meeting fiscal targets and offering public services mandated by law, such as policies to assist vulnerable children.

We find that the 2011-reform that introduced fiscal disciplinary devices contributed to a decline in budget overruns in general, but especially for out-of-home care. The result that the budget for out-of-home care is particularly sensitive to budget restrictions suggests that it is easier to up- or downscale child protective services with respect to other activities. One explanation for this could be that child protective services only affects a small and often marginalized group and are not subject to the same kind of public attention as core welfare activities such as the universal provision of schools and daycare. Another explanation for why expenditure on child protective services are particularly sensitive to budget variation can be attributed to the organization of the services in this area. While budgets for daycare, schooling or elderly care rely heavily on short-term fixed costs for buildings and wages for employees in fixed positions, spending on out-of-home care consists of more variable costs. Municipalities hire foster families on short-term contracts, and care interventions can be changed or terminated at a short notice. Thus, activities related to out-of-home care are relatively easy to scale up or down in case of budgetary pressure. The problem may be particularly relevant for small municipalities with fewer options for cost smoothing, where a few additional children in out-of-home care can lead to a substantial budget overrun. However, we do not find any significant difference in the estimated effects across municipalities depending on their population size, the demographic composition or resource pressure given by demographic composition (Appendix Table A5).

Our quantitative results are in accordance with qualitative evidence suggesting that financial circumstances and public expenditure aspects are present in discussions among municipal caseworkers when making child protection decisions. Qualitative evidence based on municipal decision makers indicates that managers do pay close attention to the budget, but budgetary issues seem to play a minor role in serious cases, for example involving violence and abuse (Schrøder, 2018). This suggests that budget considerations primarily affect the marginal child protection case when there is doubt as to whether an out-of-home placement is necessary. Recent Danish media coverage has furthermore documented cases in which municipalities ordered caseworkers to find considerable cost reductions on child protection cases (TV2 East, 2021).

Our research demonstrates that imposing sanctions on local municipalities for budget overruns can have unintended and potentially harmful consequences for the provision of welfare services, even when the sanction applies to the total budget. While our paper does not question the appropriateness of national levels of expenditure for out-of-home care or other child protective measures, we raise an important policy question regarding how to ensure that policies designed to ensure fiscal stability do not jeopardize vital welfare tasks such as protecting at-risk children. In particular, our analyses suggest that important decisions regarding child protection may be impacted by budget concerns. Policy makers should consider unintended side effects when designing disciplinary budget devices. Our results underline the need to carefully consider if sufficient public provision of essential welfare services can be guaranteed when delegated to the local government level, or whether more centralized coordination is warranted. Our paper thus contributes to the fiscal federalism literature by directing attention to the fact that the public sector faces difficult trade-offs between providing effective and credible measures to curb local government expenditure and ensuring safety and equal opportunities for vulnerable children.

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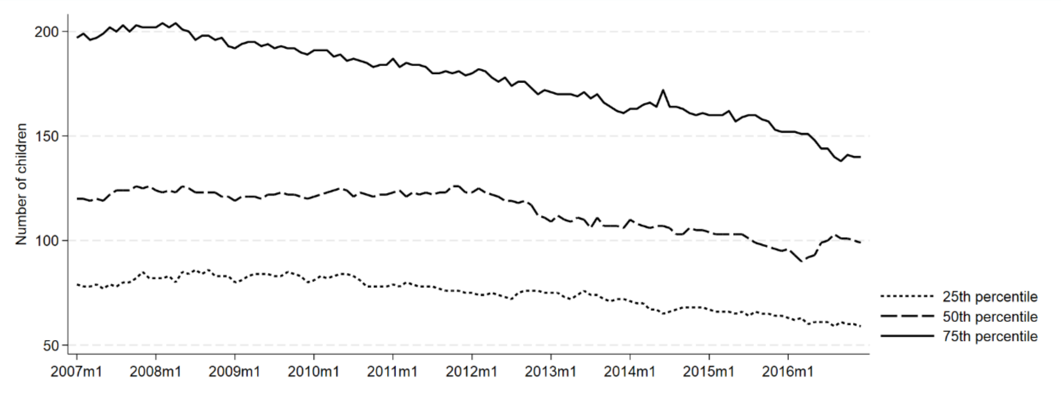
Supporting Information for Trading off fiscal budget adherence and child protection

Appendix

Data

The data set used in the paper combines Danish administrative register data with data on municipal level budgets and accounts. The rich Danish register data consists of longitudinal micro level data, which are accessible in anonymized form through Statistics Denmark's facility for researchers at Danish research institutions. The registers include administrative information on a wide array of socioeconomic characteristics, including detailed information on social services to at-risk children and families including start and end date for each service. This allows us to construct a data set with care status for all children at the monthly level linked to socioeconomic characteristics of the child and parents. Figure A1 shows municipal averages of the number of children in out-of-home care for the period 2007 to 2016 by quartile.

Figure A1: The number of children in out-of-home care per municipality, 2007-2016



Note: The distribution of number of children in care in municipalities in Denmark. The solid line 75th percentile of the municipal with respect to number of children in care. Dashed line the median municipal and the dotted line the 25th percentile of the municipality. Monthly observation from 2007-216. Source: Own calculation based on register data.

While data on municipalities' budgets and accounts are recorded annually, the individual register information allows us to calculate exactly how many children were in out-of-home care each month in the period. Furthermore, we can divide these "care months" according to the type of care. The cost of each type of care varies significantly, as shown in Table A1, which shows average prices per care type.

Table A1: Annual average price by type of care, 2016

	Price per month (Euro)
Living with relatives	1,570
Foster families	5,640
Institutional care	12,700
Boarding schools	4,250
Own room, dorm or similar	3,000
Secured institutional care	31,380
Social educational residency	12,160
Ship project	6,400

Note: The price are average prices across all municipalities. Prices exclude federal refunds.

Source: Socialstyrelsen, SocialAnalyse nr. 2, 03.2017, Table 3.

By combining the information on average annual prices of different types of care shown in Table A1 with register based monthly information on each municipality's interventions by care type, *h*, we calculated the total monthly expenses used on all types of care for each municipality, *k*:

$$cal.expenses_{kt} = \sum_h \overline{price}_{ht} \times number\ of\ months\ in\ care_{kht} \qquad \text{(A1)}$$

where the price $\overline{price}_{ht}$ is the average monthly price for each type *h* of care, *t* is time (month and year) across all municipalities, and *number of months in care* is measured for municipality *k*, type *h* and at time t.

Data imputation: Monthly expenses from annual budgets and monthly cares

Comparing these "calculated expenses" with the actual expenses shows considerable differences, which reflects that there is some variation in prices of the same type of care across municipalities. For foster families, for example, the prices in 2016 vary between 4,681 Euro and 7,220 Euro across municipalities. The price difference can arise because of the composition of children across municipality and the organization of the foster families. To capture such variation between municipalities, we estimate a time-invariant factor for each municipality to adjust the level of expenses. We here exploited that we do have the actual expenses on an annual basis, which we compared with our measure based on average prices and number of months' care. First, we construct an annual measure of calculated expenses relative to actual expenses:

$$F_{kt} = \frac{cal.expenses_{kt}}{actual\ expences_{kt}} \tag{A2}$$

Descriptive analyses (available upon request) show that there is a considerable variation in F between municipalities. Using this measure, we construct a municipality specific adjustment factor using the following two way fixed effect regression model

$$F_{kt} = \beta_0 + \gamma_t + \mu_k + \epsilon_{kt}. \tag{A3}$$

Based on the regression model we find the predicted factor:

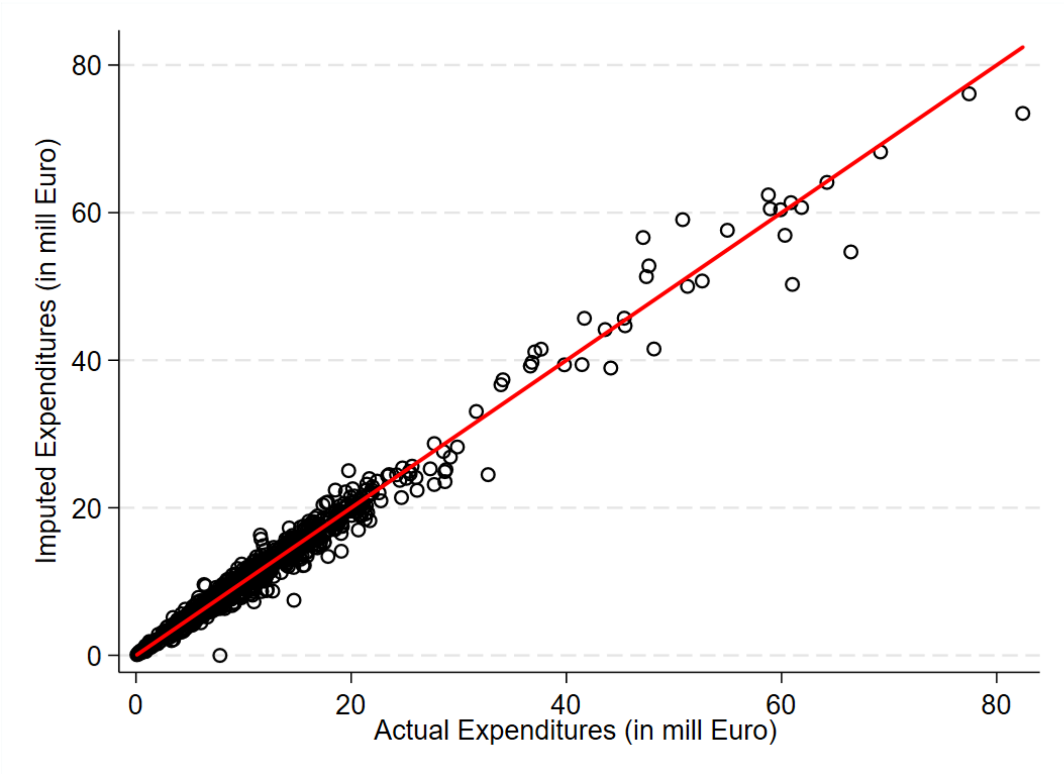
$$\hat{F}_{kt} = \hat{\beta}_0 + \widehat{\gamma_t} + \widehat{\mu_k} \tag{A4}$$

We subsequently impute the monthly expenses at the municipal level as:

$$Imp.Expenditure_{kt} = \sum_h \frac{price_{kht} \times months_{kht}}{\widehat{F_{kt}}} \tag{A5}$$

Figure A2 shows that there is a good fit between imputed and actual expenditures. The precision of the imputed expenditures can vary between years and between municipalities.

Figure A2: Plot of imputed expenditures and actual expenditures



Note: Each observation represents annual municipality expenditure. The solid red line represents the 45-degree line.

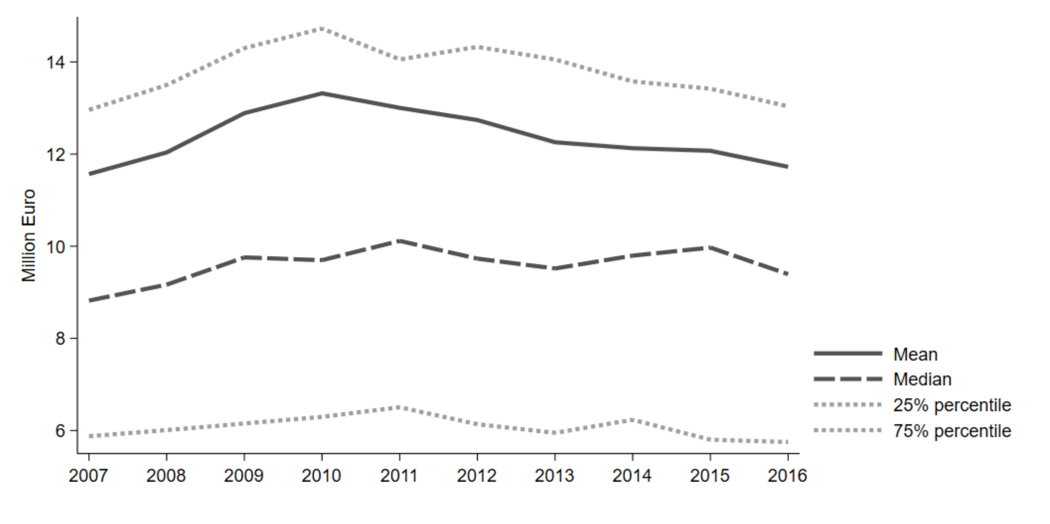
Based on imputed monthly expenses, we calculate the budget share each month *t*:

$$Budget\ share_{kt} = \frac{Imp.Expenses_{k,Jan}+..+Imp.Expenses_{kt}}{Total\ annual\ budget_{kt}}. \qquad \text{(A6)}$$

Our measure of monthly, imputed expenditures are calculated and based on average prices, which may give rise to measurement error in the budget share. Therefore, we expect an attenuation bias towards zero, and we will consider our estimate as a conservative estimate of the impact of the budget on child protection measures.

Figure A3 shows aggregate data for municipal budgets for out-of-home care. The mean municipal budget is around 12 million Euro. Naturally, budget sizes depend on municipality population size.

Figure A3: Municipality budgets for out-of-home care across percentiles



Source: The municipal budgets for out-of-home care. Annual observations for the period 2007-2016. Statistics Denmark, Statistikbanken, Table BUDK53.

Outcome variables and control variables

In our empirical analyses, we performed analyses at the municipal level and at the individual level. At the municipal level, the outcome variable is defined as the total number of children in care in each month.

When moving to the individual level, we examined how municipalities adjust their expenditures. Municipalities may adjust on five different margins when experiencing financial pressure on expenditures for at-risk children:

1. Municipalities may interrupt existing out-of-home care for children below age 18. If out-of-home care ends, children must either return to their biological parents (reunification) where other measures may initiated to help the family going forward. Reunification has direct and immediate budgetary consequences since it removes the expenditure for the out-of-home care.
2. Municipalities may end placement for children aged 18 or over. The municipality may choose to extend out-of-home care beyond age 18. However, at age 18 the child is legally an adult and the municipality's legal responsibility for the child changes.
3. For children in preventive care programs, municipalities may choose to keep those children and their families in the preventive care program for a longer period rather than taking the step towards a much more expensive out-of-home care placement.
4. Municipalities may delay or completely give up new placements. This reaction will impede a further aggravation of the municipality's financial distress. Municipalities may instead offer at-risk children a more inexpensive intervention, such as preventive care.
5. Municipalities can choose a cheaper type of out-of-home care when it initiates a placement, e.g. choose foster families instead of institutional care.

Table A2 depicts these five outcomes and the corresponding samples used to analyze the effects of budget shares for each outcome.

Table A2: Samples, outcomes, number of individuals and observations used in individual level estimations, 2007-2016

Sample	Selection	Outcome	# of individuals	# of observations
1	Children below 18 in out-of-home care	Ending care (re-unification)	36,680	1,530,176
2	Youth above 18 in out-of-home care	Ending care	19,827	94,508
3	Children below 18 in preventive action, but not in care	Out-of-home care the following month	78,090	1,648,987
4	Children below 18 ex ante neither in preventive nor out-of-home care, but will be the following month	Out-of home care (instead of preventive action)	82,011	98,022
5	Children below 18 where a new placement is initiated	Cheap (rather than expensive) placement*	23,569	24,693

*) Expensive placements are institutional care, secured institutional care, Social educational residency

Table A3 reports the monthly means and standard deviations of the outcome variables and explanatory variables for these four samples. As evident from Table A3, the monthly “reunion rate” before the age of 18 is very low, whereas the reunion rate for child who have turned 18 is substantially higher, suggesting that three months after the 18th birthday half of the children are no longer in out-of-home care. We also find that the monthly transition rate from preventive actions to out-of-home care is low. This is because preventive care often functions as the first step to prevent an out-of-home placement. For some children, the situation improves following preventive care, and they will thus never go into out-of-home care, while other children are placed in out-of-home care if preventive action turns out not to be sufficient. For children who have not previously received preventive action, we see that more than 16 percent of the children will immediately go on to out-of-home care while around 84 percent of the children will start with preventive actions.

Table A3 shows summary statistics for each of the five individual-level samples. Notably, the samples differ in terms of average age, with children in out-of-home care being older. This is consistent with the fact that in the majority of cases preventive care measures are initiated before considering a more serious out-of-home care intervention. There are slightly more boys in all four samples. In general, parents of children in care or receiving preventive action have a weak labor market attachment, that is, a higher probability of being unemployed or outside the labor force. Compared to parents of children who have never been in out-of-home care, parents of children in care are less educated, more likely to be single, and have lower labor market income.

Table A3: Summary statistics, individual-level data

	(1) Re-union <18	(2) Re-union >18	(3) Placement from prevention	(4) Placement vs. prevention	(5) Cheap placement	(6) No placement
Low birth weight	0.0679	0.00575	0.0695	0.0555	0.0548	0.0353
	(0.252)	(0.0756)	(0.254)	(0.229)	(0.228)	(0.184)
Female	0.455	0.424	0.416	0.449	0.466	0.489
	(0.498)	(0.494)	(0.493)	(0.497)	(0.499)	(0.500)
Age	11.55	18.48	10.89	10.63	10.98	8.606
	(4.445)	(1.653)	(4.299)	(4.950)	(5.313)	(5.162)
Missing Mother	0.0170	0.0283	0.00445	0.0125	0.0295	0.00267
	(0.129)	(0.166)	(0.0666)	(0.111)	(0.169)	(0.0516)
Missing Father	0.0635	0.0670	0.0404	0.0527	0.0774	0.0182
	(0.244)	(0.250)	(0.197)	(0.223)	(0.267)	(0.134)
Immigrant Mother	0.0894	0.107	0.152	0.157	0.127	0.119
	(0.285)	(0.309)	(0.359)	(0.364)	(0.333)	(0.323)
Descendant Mother	0.00418	0.00175	0.00708	0.00772	0.00506	0.00844
	(0.0645)	(0.0417)	(0.0838)	(0.0875)	(0.0710)	(0.0915)
Mother’s labor inc. (mill DKK)	0.270	0.331	0.271	0.229	0.320	0.531
	(0.444)	(0.470)	(0.445)	(0.420)	(0.467)	(0.499)
Mother outside labor force	0.320	0.292	0.158	0.157	0.274	0.0384
	(0.467)	(0.455)	(0.365)	(0.364)	(0.446)	(0.192)
Mother unempl.	0.141	0.0774	0.0729	0.0857	0.159	0.0198
	(0.348)	(0.267)	(0.260)	(0.280)	(0.366)	(0.139)
Mother single	0.503	0.444	0.491	0.480	0.509	0.158
	(0.500)	(0.497)	(0.500)	(0.500)	(0.500)	(0.365)
Mother’s age	31.79	35.65	36.70	36.16	31.61	31.32

	(1)	(2)	(3)	(4)	(5)	(6)
	(16.28)	(19.11)	(12.33)	(12.25)	(16.39)	(16.45)
Mother's education:						
Primary school	0.593	0.468	0.403	0.407	0.499	0.138
	(0.491)	(0.499)	(0.490)	(0.491)	(0.500)	(0.345)
Secondary educ.	0.220	0.269	0.357	0.343	0.287	0.343
	(0.414)	(0.443)	(0.479)	(0.475)	(0.452)	(0.475)
Outcome variables:						
Reunion	0.00571	0.206				
	(0.0754)	(0.405)				
Placement			0.00447	0.158		
			(0.0667)	(0.364)		
Cheap type					0.519	
					(0.500)	
Observations	1,530,176	94,508	1,648,987	98,022	24,693	12,840,842
N	36,680	19,827	78,090	82,011	23,569	1,989,925

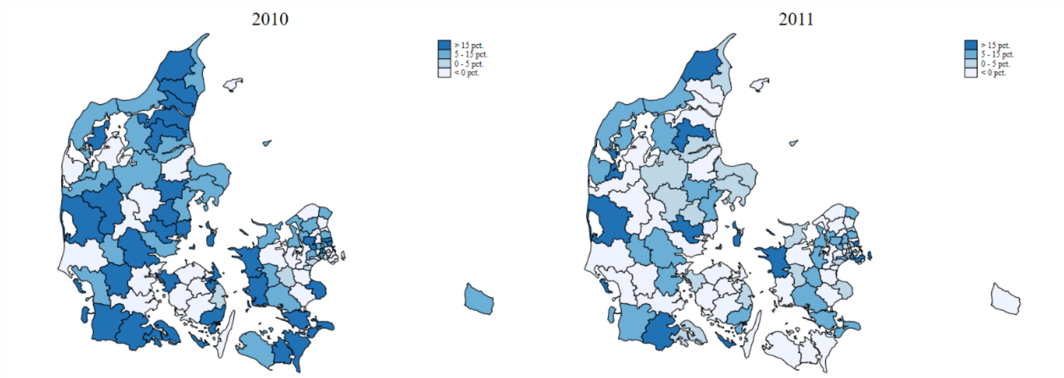
Note: On samples in each column, see Table A2. Column (1) represents Sample 1, column (2) Sample 2, column (3) Sample 3, column (4) Sample 4, column (5) Sample 5, and column (6) the entire population of 0-17 year-old children.

Additional results and material

The following section presents some descriptive results preceding our regression analyses. These results complement the estimation results presented in the paper. Moreover, we present the results from a number of robustness checks of the main analysis.

Figure A4 shows average municipal budget overruns on out-of-home care before and after 2011 for the 98 Danish municipalities.

Figure A4: Budget overruns on out-of-home care in 2010 and 2011



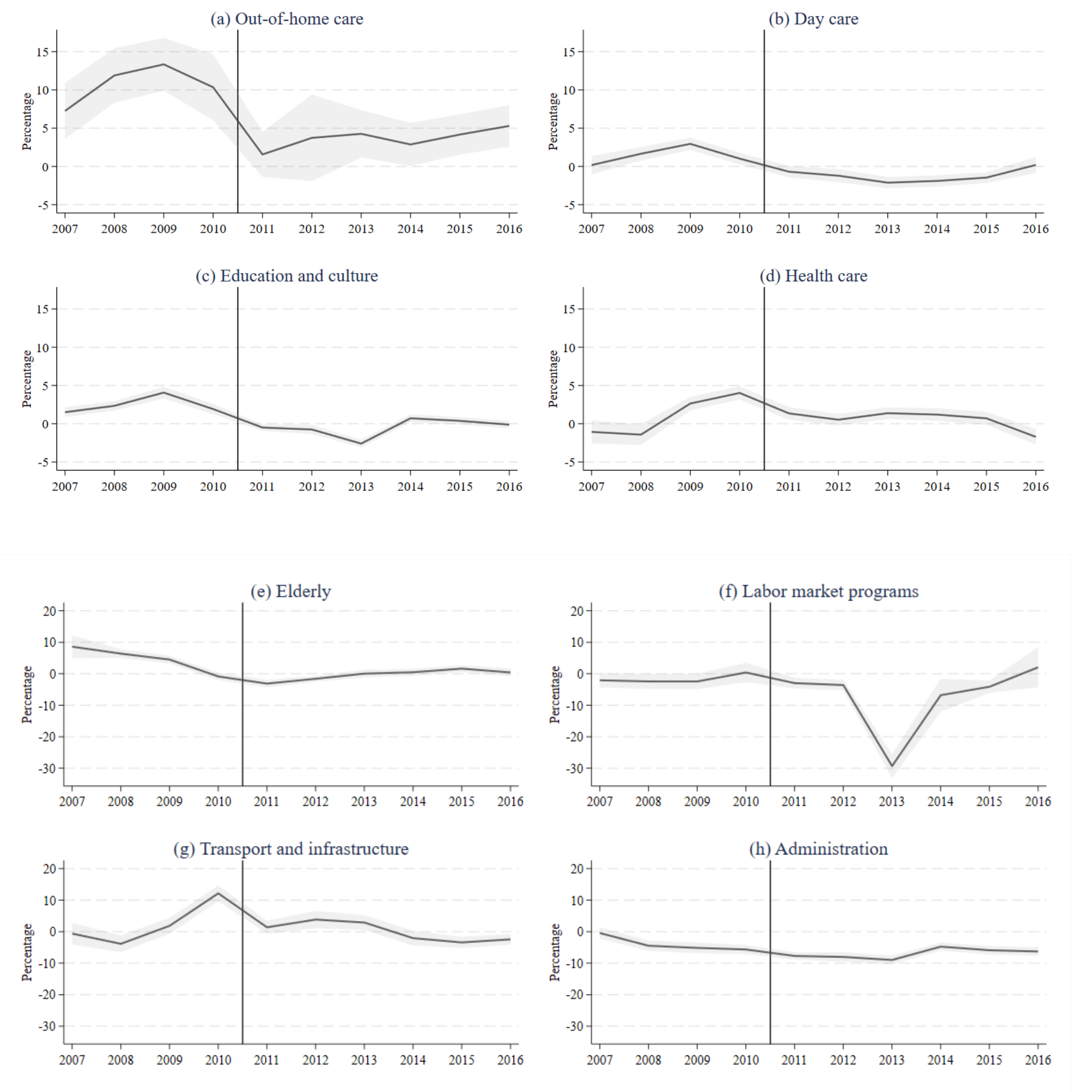
Source: Budget overrun is defined as the actual expenses on out-of-home care divided by the budget for out-of-home- care. Own calculations based on data from Statistics Denmark, Statistikbanken, Table BUDK53 and REGK53.

Figure A5 shows average municipal budget overruns on eight selected budget items for the period 2007-2016. We created this data based on municipal budgets by comparing individual municipal budgets with ex-post municipal accounts in the same year. As noted, budget over- and underruns are prevalent in the period. The budget for out-of-home care suffered from budget overruns in the period 2007-

2010, while budget overruns were reduced after 2010. The measure of budget overrun is particularly noisy for out-of-home care, indicating a large variance in out-of-home care budget deficits across municipalities over the entire period.

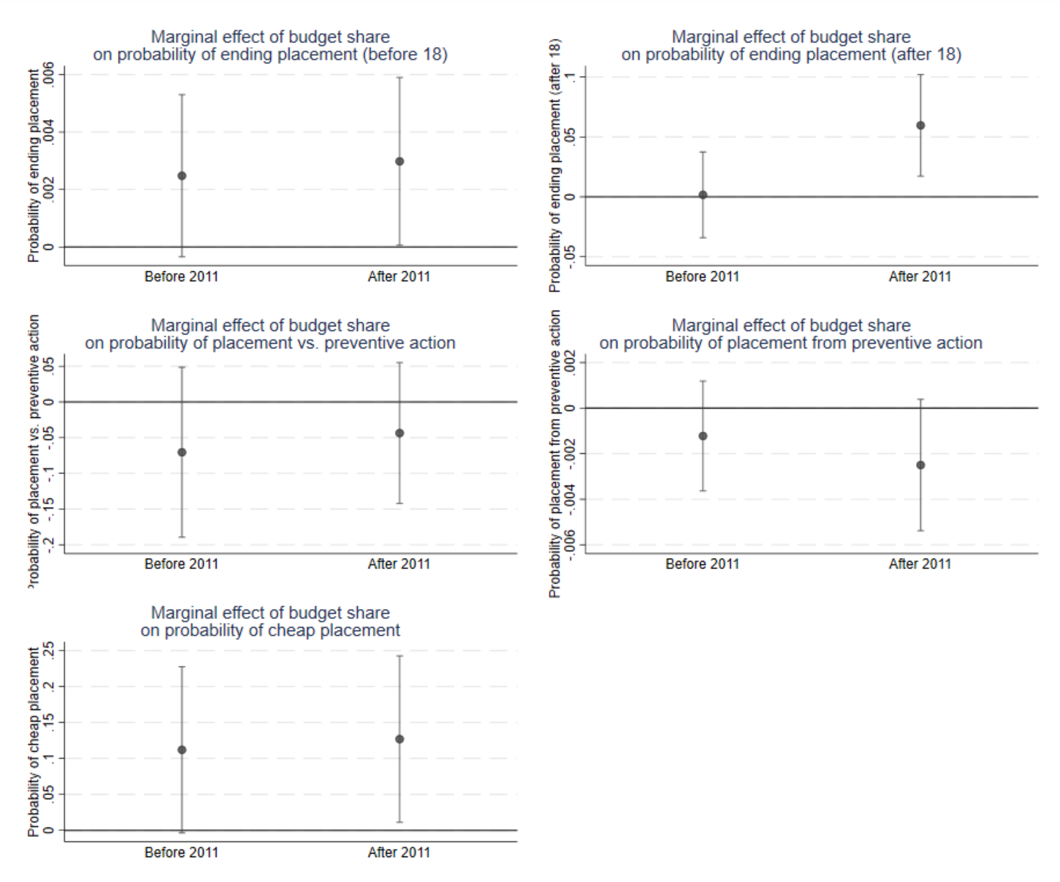
Figure A6 shows the effects of budget overrun for the five outcomes, before and after 2011. The effects of budget overrun are stronger and more significant for most outcomes.

Figure A5: Average budget overruns for a selection of municipal activities



Source: The graphs shows the average of annual actual expenses divided by annual expenses in the budget for different items in the municipalities. Own calculations based on data from Statistics Denmark, Statistikbanken, Table BUDK53 and REGK53. The vertical line marks the introduction of sanctions for budget overruns.

Figure A6: Marginal effects of budget share on child protection measures, before and after 2011



Note: Top Panel: The predicted probability of ending a placement for a youth above 18 in the month of July as a function of the budget share at July 1st. Middle Panel: The predicted probability of a placement instead of preventive action for a child below 18 in the month of July as a function of the budget share at July 1st. Bottom Panel: The predicted probability of initiating a cheap placement (instead of an expensive placement) for a child below 18 in the month of July as a function of the budget share at July 1st. Confidence intervals at 0.05 level. Standard errors clustered at municipal level.

Table A4: Estimation results, individual micro data based regression

	(1)	(2)	(3)	(4)	(5)
	Re-union <18	Re-union >18	Placement from prevention	Placement vs. prevention	Cheap placement
	b/se	b/se	b/se	b/se	b/se
lag budget before 2011	0.4369	0.0156	-0.2624	-0.3695	0.5377
	(0.2578)	(0.1685)	(0.2631)	(0.3189)	(0.2832)
lag budget after 2011	0.5227*	0.4487**	-0.4977	-0.2780	0.6078*
	(0.2571)	(0.1627)	(0.2948)	(0.3220)	(0.2822)
Girl	0.0270	0.0926***	0.1184***	0.2015***	0.2802***
	(0.0269)	(0.0169)	(0.0313)	(0.0214)	(0.0265)
Age	-0.2247***	-27.1900***	-0.1435***	-0.2597***	-0.2690***
	(0.0142)	(0.4902)	(0.0156)	(0.0107)	(0.0149)
Age squared	0.0156***	0.6894***	0.0100***	0.0136***	0.0103***
	(0.0007)	(0.0126)	(0.0007)	(0.0006)	(0.0009)
Missing info, mother	0.2890*	0.0626	0.1034	0.9431***	-0.5782***
	(0.1305)	(0.0729)	(0.2454)	(0.1484)	(0.1425)
Missing info, father	-0.1850**	0.0114	0.0497	0.4126***	0.1880**
	(0.0573)	(0.0585)	(0.0675)	(0.0460)	(0.0650)
Information mother					
Immigrant	0.5291***	-0.1528***	-0.2519**	-0.0933*	-0.3998***
	(0.0542)	(0.0406)	(0.0768)	(0.0389)	(0.0845)
Descendant	0.3052	0.3144***	-0.7768**	-0.0736	-0.4273*
	(0.2282)	(0.0891)	(0.2684)	(0.1530)	(0.2175)
Labor income	0.1137***	0.0867**	-0.2447***	-0.0852**	-0.3074***
	(0.0312)	(0.0325)	(0.0602)	(0.0312)	(0.0487)
Outside labor force	-0.2612***	0.0566	-0.0023	0.0410	0.1500*
	(0.0558)	(0.0396)	(0.0628)	(0.0463)	(0.0670)
Social	0.3152***	-0.0622	0.2711***	0.1388**	-0.0961

security					
	(0.0484)	(0.0458)	(0.0509)	(0.0454)	(0.0634)
Single parent	0.0896**	-0.0014	0.2128***	-0.0307	0.2765***
	(0.0278)	(0.0206)	(0.0348)	(0.0236)	(0.0270)
Age, mother	0.0207***	-0.0040	-0.0227***	-0.0224***	-0.0306***
	(0.0047)	(0.0031)	(0.0060)	(0.0047)	(0.0054)
Age, mother,	-0.0002***	0.0001	0.0001	0.0001	0.0002*
squared	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Primary	-0.1826***	-0.0083	0.2764***	0.2985***	0.3818***
school	(0.0327)	(0.0326)	(0.0495)	(0.0339)	(0.0489)
Secondary	0.0411	0.0315	0.0849	-0.0152	0.1757***
school	(0.0353)	(0.0306)	(0.0444)	(0.0342)	(0.0480)
Low birth	-0.2868***	0.0533	-0.0845	-0.0472	-0.1381
weight	(0.0592)	(0.0634)	(0.0552)	(0.0392)	(0.0882)
Constant	-5.6557***	262.3477***	-6.2091***	-0.2779*	-0.8061***
	(0.0926)	(4.7001)	(0.1773)	(0.1326)	(0.1419)
Month dummies	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes
Municipal dummies	Yes	Yes	Yes	Yes	Yes
N	1,530,086	94,637	1,200,258	70,340	24,305
R^2					

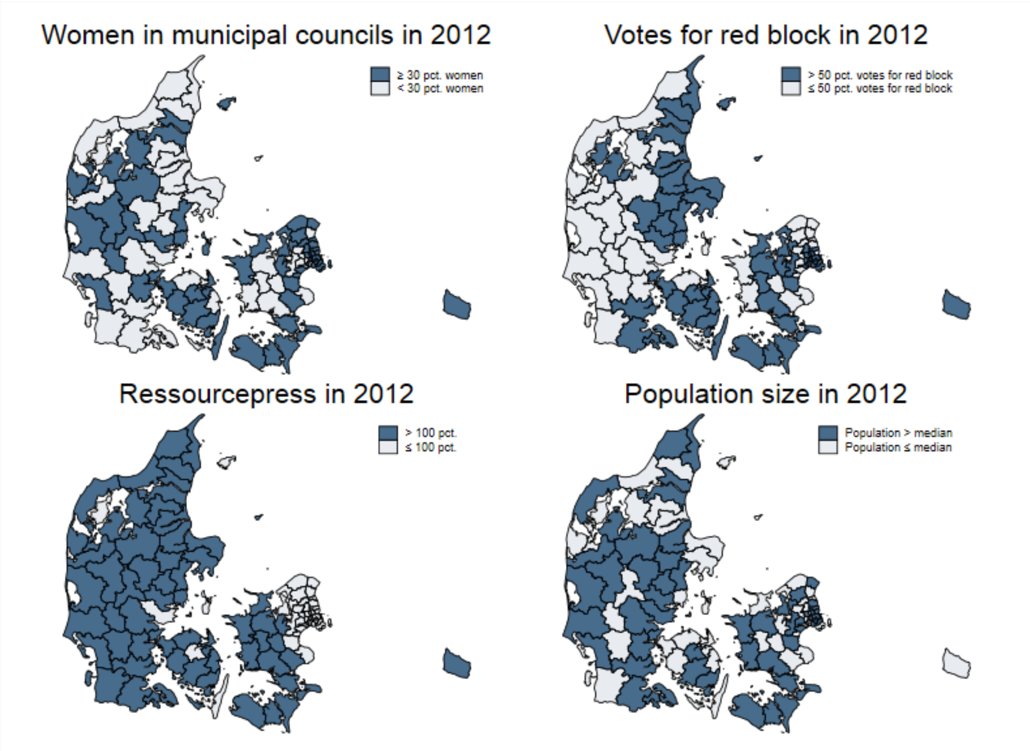
Note: On samples in each column, see Table A2. Column (1) represents Sample 1, column (2) Sample 2, column (3) Sample 3, column (4) Sample 4, column (5) Sample 5, and column (6) the entire population of 0-17 year-old children.

Heterogeneous effects

As a supplement to the individual level analyses presented in the paper, we tested whether some municipalities were more likely to react to budget concerns than others. We investigated four selected municipality characteristics that we hypothesize may affect municipal decisions regarding at-risk children. First, whether the majority in the city council consisted of parties on the center-left, second, whether the city council consisted of more than 30 percent women, third, population size, and fourth, a proxy for resource pressure in the municipality based on its socioeconomic characteristics.[5] See Figure A7 below for graphs of the geographical distribution of these four characteristics.

[5] We thank Kurt Houlberg, VIVE, for providing us with data on resource pressure at the municipal level.

Figure A7: Geographic distribution of four municipality characteristics



As shown in Table A5, none of these municipality characteristics matter for the size of the effect.

Table A5: Municipal level regressions, interactions with municipality characteristics

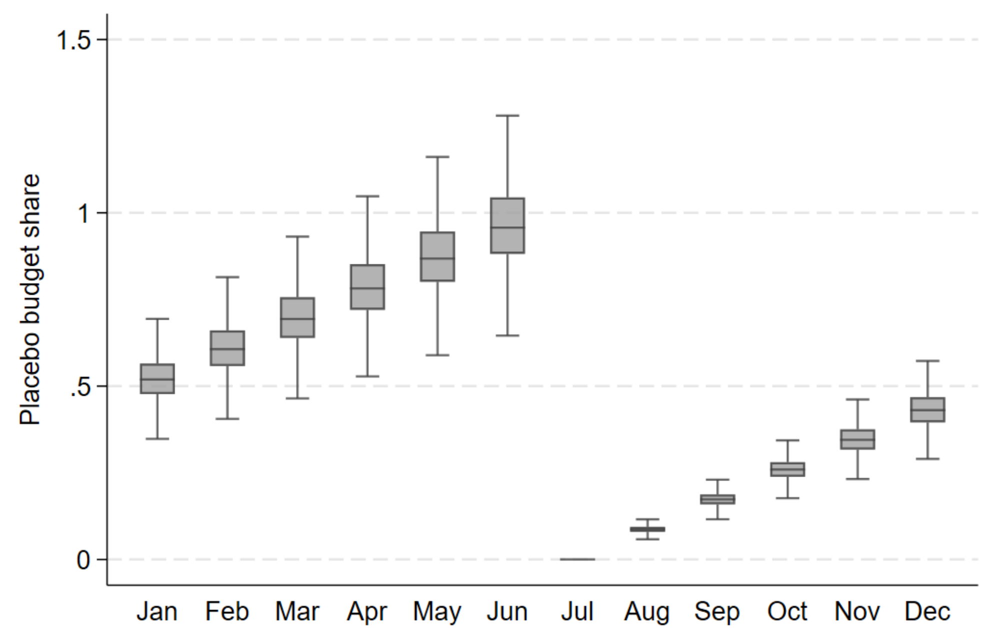
	Women in municipal council	Votes for red block	Resource pressure	Population size
	b/se	b/se	b/se	b/se
Lag log number	-0.023***	-0.023***	-0.023***	-0.023***
of children	0.005	0.005	0.005	0.005
Women>0.3×	-0.017*			
Budget share	0.007			
Women<0.3×	-0.017*			
Budget share	0.007			
Right×		-0.016*		
Budget share		0.007		
Center-Left×		-0.018*		
Budget share		0.007		
Less ress.×			-0.018*	
Budget share			0.007	
More ress.×			-0.016*	
Budget share			0.007	
Small pop.×				-0.016*
Budget share				0.007
Large pop×				-0.019*
Budget share				0.007
Year dummies	0.114***	0.114***	0.114***	0.115***
Month dummies	0.026	0.026	0.026	0.027
Municipal dummies	Yes	Yes	Yes	Yes
N	11,531	11,531	11,531	11,531
R^2	0.034	0.034	0.034	0.034
F-statistics	0.000	1.385	0.456	1.687

Note: The effects of lag budget share is interacted with (1) a dummy for whether share of women in municipal council is greater than or smaller than 30%, (2) a dummy for whether votes for "red block", i.e. parties in the left hand side of the political spectrum, had the majority in the municipal council, (3) a dummy for whether the municipality was under financial constraint (compared to other municipalities), and (4) a dummy for whether the municipality had a relatively small or large population size compared to the mean size.

Placebo tests

Finally, we performed a robustness test to check whether our results could be driven by the way municipalities organize their work with at-risk children over the (budget) year. We thus ran placebo tests, where we constructed "placebo" data, pretending that the fiscal year runs from July to June (instead of from January through December, as is the actual situation). We constructed "placebo" monthly budgets by defining a fiscal "annual" budget as the average of the budget for the two calendar years. We next constructed measures of monthly expenditures as before and then constructed a placebo cumulated budget share for each month in the placebo fiscal year starting July 1 and ending June 30 the following year (see Figure A8).

Figure A8: Placebo budget share



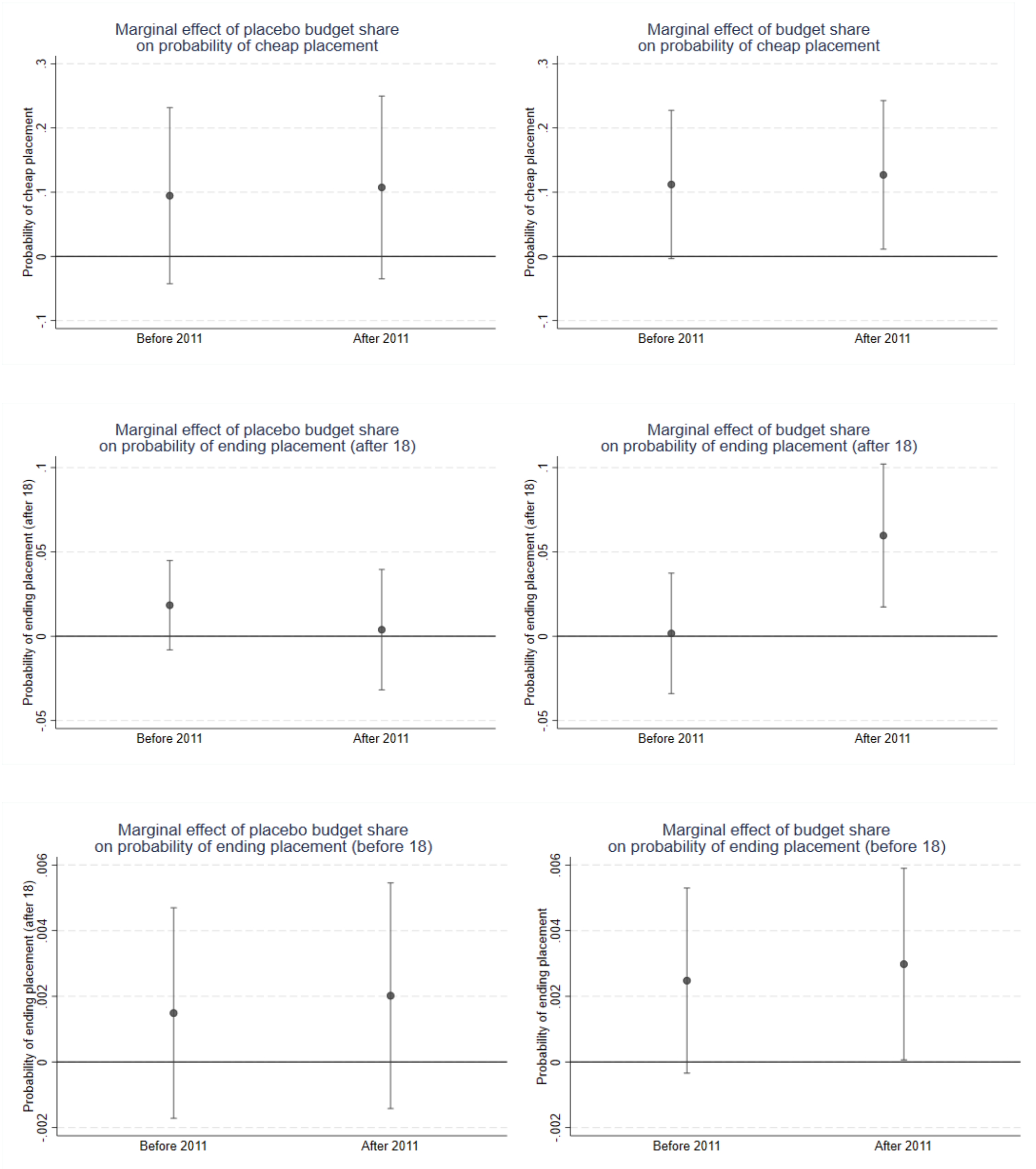
Note: The placebo budget share is constructed for a "placebo" fiscal year that we pretend runs from July one year to June the following year. The placebo budget share is measured at the beginning of the month. For July, the placebo budget share is always zero. For August, the placebo budget share is defined as the proportion of the total placebo budget (the average of the two years around) for year *t* that was used in July. For September, the placebo budget share is calculated as the share of July and August expenditure out of the total budget, etc. Finally, for June the following year, placebo budget share is defined as expenditure share of months July-May in the two years.

We then ran exactly the same estimations as in our main specifications with the placebo budget share instead of the actual budget share. The result of the placebo test for the analysis on municipality level is shown in Table 1 (main paper), last column. The results show that effect of the placebo budget is share is very small and positive, which indicates that the effect of the true budget share is not a result of mean reversion.

For the individual-level specifications, we ran the placebo test for the two individual outcomes that in the main analysis showed statistically significant effects. The results of these placebo tests are shown in Figure A9. They show that the effects of the budget share on the probability of ending out-of-home care for children below and above age 18, and on the probability of choosing a cheaper placement are now insignificant and the coefficients are numerically smaller. Thus, the individual-level placebo tests also suggest that mean reversion is not driving out main results.

Figure A9: Estimation results from placebo tests

Placebo test | **Actual results**



Note: The first row shows analysis of ending the placement after turning 18 and the second row the analyses of choosing a foster care instead of preventive action. The left column shows the placebo test and the right column the actual analyses.