Surviving Childhood: Effects of Removing a Child From Home

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Abstract

This paper studies the effects of the court-ordered removal of children from home on health, crime, and education. To isolate causal effects, I exploit quasi-random variation in judge assignment together with across-judge variation in the tendency to favor removal in an instrumental variable (IV) design. Using a novel data set (N=26,579) based on Swedish court documents that I transcribe and link with detailed register data, I find that court-ordered out-of-home placement has large adverse effects on the mortality of the marginal child. These effects are primarily driven by suicides that occur while the removed child is still placed in out-of-home care. Removal also causes an increase in hospitalizations for mental illness and non-narcotic crimes. There is little evidence of adverse health effects for birth parents. I explore potential explanations for the detrimental effects on child health. Adverse care conditions and peer exposure appear to be important channels.

Key words: Child protection, Foster care, Mortality, Crime
JEL codes: I12, I14, I38, K42

We see major failures throughout the social welfare and healthcare systems.

– The Health and Social Care Inspectorate (2018)

1 Introduction

Suicide and drug use disorder are among the top three causes of teenage death in many Western countries (World Health Organization, 2020). A particularly vulnerable group is children placed in foster homes and other forms of out-of-home care. Studies in for example Australia, Denmark, and Sweden document that 2-6% of children will be placed in out-of-home care by age 18 (Berlin et al., 2021).[1] At the same time, these children are 3-5 times as likely to die in adolescence and early adulthood as their peers (NBHW, 2013; Segal et al., 2021; Sariaslan et al., 2022; Sørensen et al., 2023). They are also more likely to use heavy drugs, attempt suicide, and be diagnosed with a range of physical and mental disorders (Braciszewski and Stout, 2012; Deutsch and Fortin, 2015; Evans et al., 2017). Despite these striking statistics, there is little causal evidence on the effects of out-of-home placement on health outcomes. In this paper, I leverage a novel Swedish data set to study the effects of court-ordered out-of-home placement on all-cause and cause-specific mortality. To further deepen our understanding, I also examine the effects on healthcare utilization, criminal behavior, education, and a range of parent outcomes.

One reason for the scarce evidence on the causal effects of child removal on health outcomes is data availability. A large, longitudinal, and rich data set at the individual level is needed to obtain credible estimates. To overcome this challenge, I collected 21,597 Swedish child protection court files from 2001 to 2019 and extracted relevant information, including the personal identity number of each child. Using these unique identifiers – which are given to all residents in Sweden – Statistics Sweden linked the children and their parents to rich registry data, including death, patient, crime, and education registers.

The paper is focused on child protection cases in which the parents or the child do not consent to removing the child from home. While only around 30% of children in Swedish out-of-home care are placed in care without consent, such cases are particularly policy-relevant as they involve taking government actions that conflict with the individual's right to family and home. I refer to these placements as 'court-ordered placements'.[2]

Another key challenge is selection bias. For example, children placed in out-of-home care likely have experienced more severe maltreatment than others, which in itself can impact future outcomes and confound the estimates. In this paper, identification is achieved by utilizing as-if-random assignment of judges to cases together with across-judge variation in removal tendency in an IV design. With this strategy, I estimate the causal effect of removing children at the margin

[1] Similar rates are reported in Ubbesen et al. (2015), Rouland and Vaithianathan (2018), and Yi et al. (2020).

[2] There are two key explanations for the large share of voluntary placements. First, unaccompanied minors are included in the statistics and they make up one-third of children in voluntary care. Second, according to Swedish law, children are not allowed to live in a home that does not belong to a person with legal custody of the child without the involvement of the child protection authorities.

of placement, i.e. cases that judges may disagree about. From a policy perspective, the effect on this group is especially relevant because these are the children who are affected if there is a change in the threshold for when child removal is required.

In my baseline specification, I define judge removal tendency in a particular court case as the mean removal rate in all other cases handled by the same judge. Since the decisions for siblings in the same court case are correlated, the entire case is left out to ensure there is no mechanical relationship between the instrument and decision-making in the case of interest.

Three key features of the Swedish setting enable me to use the judge instrument. First, there is meaningful variation in judge behavior and the instrument is highly predictive of the removal decision. Second, due to Swedish law, the assignment of court cases to judges is quasi-random. This is confirmed by court staff and empirically validated. Third, the assigned judge only has contact with the family during the oral hearing (if at all) and is essentially tasked with making a single, binary decision: remove the child from home or not. All other decisions are made by caseworkers at the local child protection authority (known as Social Welfare Committee; SWC). Hence, it is unlikely that the judge influences the child's outcomes in any other way than via the removal decision, which is critical to meet the exclusion restriction needed for a causal interpretation.

There are multiple reasons to expect that removing a child from home affects health outcomes. These reasons can be structured around four themes: disruption, care conditions, peers, and parents. First, disruption of the child's social and physical environment can be a deeply stressful event and can have long-lasting adverse effects on the child's health and development (Goldsmith et al., 2004; Jelleyman and Spencer, 2008; Astrup et al., 2017; Cohen and Mannarino, 2019). In addition, we should expect particularly large reactions among children at risk of removal to new adverse experiences since prior empirical evidence suggests that disadvantaged children are more sensitive to stress and adverse events (Adda et al., 2011; (Oreopoulos et al., 2008; Van Heeringen and Mann, 2014; Dobbie, Grönqvist, et al., 2018; Turecki and Brent, 2016; Carballo et al., 2020).

Second, out-of-home placement might improve or worsen the care of the child. Removing a child from an abusive or neglectful home and placing them in a loving family may positively affect child outcomes as child abuse and neglect are associated with later-life mental illness, substance use disorder, and suicide (Felitti et al., 1998; Dube et al., 2001). Unfortunately, Swedish government agencies have repeatedly found widespread and severe deficiencies in out-of-home care (see The Ombudsman for Children, 2010, 2011, 2019, for overviews).

Third, peers living in the same home may affect the child, both through peer effects and victimization. Helénsdotter (2025b) finds that exposing youths with a history of substance abuse or self-harm to peers with a similar background increases the risk of experiencing severe adverse events related to substance abuse and self-harm. In addition, Mazzone et al. (2018) conclude

in an international review that violent victimization by peers during out-of-home placement is a widespread phenomenon. Sweden is no exception: during the last two decades, there have been numerous news stories on murders, rapes, and assaults committed in Swedish foster homes, group homes, and institutions (e.g., Järkstig, 2016; Hellman, 2019). Victimization can directly increase the risk of death through murder, and indirectly through deteriorated mental health and increased risk of suicide (Dustmann and Fasani, 2016; Nikolaou, 2017; Bharadwaj et al., 2021).

Fourth, out-of-home placement might facilitate the take-up of health and substance abuse treatment among parents (Grimon, 2020), and encourage parents to improve the home environment (Baron and Gross, 2022). On the other hand, the trauma of losing a child might lead parents to fall deeper into substance abuse, mental illness, and destructive behaviors (Kendler et al., 1993).

Using IV analysis, I find that out-of-home placement has significant adverse effects on the mortality of the marginal child. Removal increases the risk of death by the year the child turns 19 by 6.8 percentage points (relative to a control complier mean of 1.8%). This increase is primarily driven by suicides that occur while the removed children are still placed in out-of-home care. I also trace out the effects over the months following the court's judgment. For children who are old enough to self-harm and use harmful substances, there is a significant increase in the risk of suicide (but not accidental overdose) already by month 9.

To understand why child removal has such adverse effects on mortality, I explore the four channels discussed above (disruption, care conditions, peers, and parents). First, I collected SWC case notes and investigations of deaths from unnatural causes that occur by the year the child turns 19. Physical and sexual victimization by peers and harmful peer-to-peer spillovers contributed to 20% of the deaths. The main identified contributor, however, is deficient care conditions. Frequent and long disruptions in psychiatric treatment, inadequate supervision, and failure to meet the child's emotional needs are some of the deficiencies found to have contributed to over half of the deaths.

In line with these results, court-ordered removal significantly *decreases* the number of planned outpatient specialist visits for mental illness but *increases* the risk of hospitalization for mental illness in the months before the steep rise in suicides at month 9.

I also present empirical evidence in support of the peer channel. First, I show that there are harmful peer effects in self-harm and substance abuse. I then show that exposure to peers with a criminal background increases both the risk of being hospitalized for injuries sustained through assault and (among boys) death. In line with the rise in injuries sustained through assault, court-ordered removal significantly increases the risk of the marginal child committing non-narcotic crimes and, especially, crimes against persons (e.g., violent and sexual crimes) within the first year following the court's judgment. Conditional on being removed, almost all of these crimes are committed *during* placement.

My paper contributes to the literature on the effects of child protection interventions (for a review, see Bald, Doyle, et al., 2022).[3] In Appendix A, I present an overview. To date, the literature focuses on education, crime, and labor outcomes. Only five papers (using different empirical strategies) examine any health-related outcomes (with mixed findings): behavioral problems (Berger et al., 2009), emergency health visits (Doyle, 2013), parental take-up of treatment programs (Grimon, 2020), and healthcare usage (Drange et al., 2022; Gram Cavalca et al., 2022). By using plausibly exogenous variation in removals to study the effects on mortality, hospitalizations, and outpatient care, I extend our knowledge about the health effects of child removal. I also add to a rapidly growing economic literature on the determinants of mental health (e.g., Persson and Rossin-Slater, 2018; Adhvaryu et al., 2019; Fruehwirth et al., 2019; Baranov et al., 2020; Kiessling and Norris, 2023) and the determinants of harmful substance use (e.g., Powell et al., 2018; Alpert et al., 2022). My findings — which concern a highly disadvantaged population — are also relevant to the literature on mortality inequality (Miller et al., 2021; Case and Deaton, 2022).

Almost all credible papers on the effects of child protection interventions are conducted in North America. The only exceptions are Lindquist and Santavirta (2014), Drange et al. (2022), and Gram Cavalca et al. (2022). While none of these studies has access to exogenous variation in removals, they make use of detailed and longitudinal data to mitigate omitted variables bias. By creating a novel data set based on court documents and exploiting plausibly exogenous variation in judge behavior, I shed new light on the effects of child removal outside North America. Given that the institutional setting in the US is vastly different from Europe in terms of, e.g., child welfare, juvenile justice, healthcare, schooling, and social security systems (Gilbert et al., 2011), it is imperative to gain knowledge about the effects of child removal in Europe.

I also contribute to our knowledge about family effects of child removal by considering novel parent outcomes (mortality, marriage, and labor income). Bald, Chyn, et al. (2022) and Baron and Gross (2022) examine the effects of removal on crime outcomes for parents listed as maltreatment perpetrators and find conflicting results. The only other paper that can observe perpetrator and non-perpetrator parents is Grimon (2020). She finds that opening a child welfare case increases mothers' take-up of mental health and substance abuse treatment. This line of work fits into the literature on family spillover effects (Carneiro et al., 2015; Bhuller et al., 2018a, 2018b; Billings, 2018; Dobbie, Grönqvist, et al., 2018; Fadlon and Nielsen, 2019; Arteaga, 2021; Bhuller et al., 2021; Bingley et al., 2021).

[3] Around half of the children in my sample engage in destructive behavior, including crime. These children can be placed in secure facilities. Hence, another relevant literature is the work on the health effects of incarceration (Hjalmarsson and Lindquist, 2022; Norris et al., 2022). In contrast with my findings, these studies do not find that mortality increases during or after incarceration. Part of the explanation can be differences in the characteristics of the population and the alternative to treatment.

A last distinguishing feature of my paper is that I use a judge instrument to achieve identification. Judge decision-making has been exploited as an instrument in several influential papers (Kling, 2006; Aizer and Doyle, 2015; Dobbie, Goldin, and Yang, 2018; Eren and Mocan, 2019; Bhuller et al., 2020; Norris et al., 2021), but not in the context of child protection.[4] What has been used in the child protection literature is variation across workers at the Child Protection Services (CPS) in their tendency to file a petition with the courts for child removal. The margins studied using the judge and CPS worker instruments are slightly different. The CPS worker instrument identifies effects for children on the margin of being subject to a court petition for removal, while the judge instrument identifies effects for children at the margin of being removed via court order conditional on a petition already having been filed. Hence, the judge instrument might identify effects for cases in which it is especially difficult to determine whether the child should be removed.

Studies using the CPS worker instrument report diverging results, with some finding overall negative effects (Doyle, 2007, 2008, 2013; Warburton et al., 2014) and others finding positive or null effects (Roberts, 2018; Bald, Chyn, et al., 2022; Baron and Gross, 2022; Gross and Baron, 2022). There can be several reasons for the mixed findings: e.g., differences in population characteristics and welfare practices. One potential reason is differences pertaining to the underlying assumptions of the IV design. As discussed in, e.g., Grimon (2020), Bald, Chyn, et al. (2022), and Gross and Baron (2022), the CPS worker instrument can be challenging to apply. For example, if the worker also decides which support services should be prescribed to the family or whether the police should be contacted, the worker may affect child outcomes through channels other than the removal decision. While a reduced-form effect can be estimated, the exclusion restriction needed to isolate the effect of removal can be challenging to meet. The extent and character of this issue may vary between study settings due to local variations in practices. With the judge instrument, I avoid this issue since, in Sweden, the judge only decides whether the child should be removed and has very limited contact with the family.

2 Institutional Background

2.1 Child Protection System in Sweden

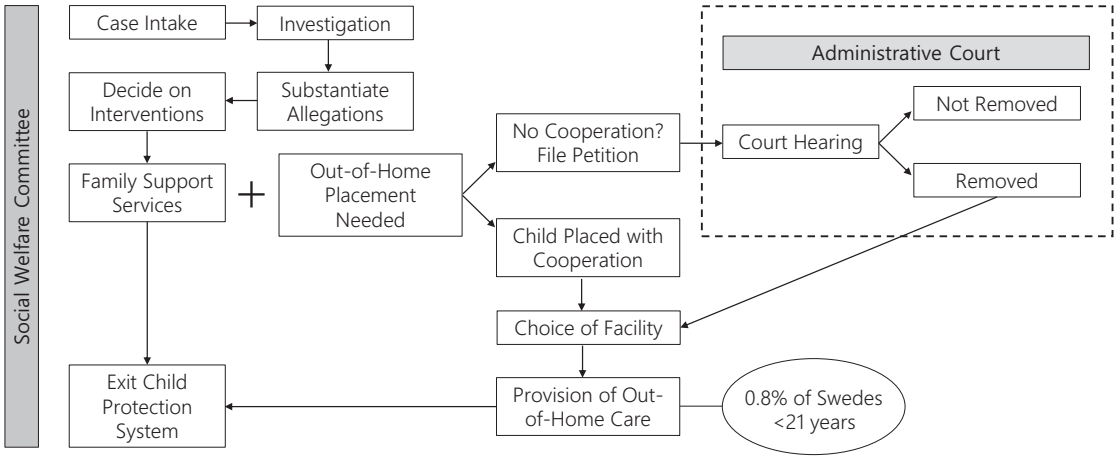
Figure 1 provides a representation of the child protection process in Sweden. The local SWC (*socialnämnden*) is responsible for child protection.[5] This responsibility is broad and encompasses,

[4] Decision-maker stringency has been used as an instrument in other non-criminal contexts (e.g., Maestas et al., 2013; Dahl et al., 2014; French and Song, 2014; Dobbie and Song, 2015; Dobbie et al., 2017; Autor et al., 2019; Collinson et al., 2022).

[5] Typically, there is one SWC per municipality. In large municipalities, there can be several SWCs. There are 290 municipalities in Sweden.

e.g., preventive work, investigations, evaluation of service needs, and service provision. Incoming allegations and reports are investigated by a caseworker with at least a bachelor's degree in social work. If the caseworker and their supervisor determine that the child should be placed in out-of-home care, the case is presented to the deciding body within the SWC, which usually is comprised of 10-20 politically appointed committee members. Decisions are made through majority voting. However, the SWC does not have the authority to take children into care without the consent of the caregivers and the child. When no consent can be attained, but the deciding body agrees with the caseworker and the supervisor that there are legal grounds to place the child in care, the SWC submits a petition for removal with one of Sweden's 12 administrative courts.[6]

Figure 1. *Child Protection Process in Sweden*



Note: This figure provides a representation of the child protection process in Sweden. The SWC handles case intake, determines whether an investigation is needed, conducts the investigation, determines whether the allegations that prompted the investigation are substantiated, and decides which interventions are needed. If the SWC determines that out-of-home care is necessary, but the family does not consent to removal, the SWC files a petition with the court. The court decides whether to approve the petition. If the court approves the petition, the SWC chooses where to place the child and continues to provide care until the child can exit the child protection system.

In 80% of cases, the SWC takes the child into emergency care. The SWC must then inform the court within one week. Judges can terminate emergency care before ruling on the petition for removal. However, judges only terminate emergency care in 0.6% of the baseline sample, typically because of administrative errors made by the SWC (Table B1).

If new information is obtained after the petition has been filed that causes the SWC to change its assessment, the SWC can withdraw its petition or change its claims before the judgment is handed down. I use the initial petition (i.e. before judge assignment) to construct background variables such as petition grounds. Withdrawn petitions are not part of the data.

The court's objective is described in the Care of Young Persons Act. First and foremost, what

[6]Before February 15, 2010, there were 23 courthouses.

is best for the child is to be decisive. If (i) one or more conditions of the home environment imply a palpable threat to the health or development of the child or (ii) the child endangers their health or development through substance abuse, criminality, or other destructive behavior, the court is to rule in favor of out-of-home care. I refer to the former as environment cases and the latter as behavior cases.

Mental illness (e.g., autism and post-traumatic stress disorder) is common in this population, but the legal mandate to place children with mental illnesses in out-of-home care has been discussed, changed, and clarified over the last two decades in several official reports, government bills, and rulings (e.g., Swedish Government, 2002; SOU, 1998:31, 2000:77). According to the Supreme Administrative Court (2010), a child cannot be taken into care on the *basis* of their mental illness, but children with mental illnesses can be removed if they engage in socially destructive behavior provided that the behavior is not a *symptom* of the child's mental illness. Further guidance is very limited and it is emphasized that decision-makers must decide which form of care (out-of-home versus in-home) is best on a case-by-case basis (Swedish Government, 1989).

When a petition has been filed, the case must promptly be assigned to a judge in accordance with predetermined and objective criteria, and the assignment may not be conducted to influence the outcome of the case. According to staff at the Administrative Court of Gothenburg, the registration office registers the case in the national case management system when the petition is received. The case is then automatically assigned to a department within the court according to a rotating system.[7] Cases are then manually assigned within the department to the next judge according to (again) a rotating system. This is done irrespective of the characteristics of the case, with one exception: junior judges. As specified in national guidelines, junior judges are typically not assigned: (i) cases in which there is suspected physical or sexual abuse of a young child, (ii) environment cases in which a parent has an intellectual disorder, and (iii) behavior cases in which the need for care largely is based on ADHD or autism.[8] Fortunately, junior judges only make up 3% of my analysis samples and the results are robust to excluding these judges and cases that are typically not assigned to junior judges.

While the exact details vary between courts and over time, staff at the courts in Falun, Malmö, and Stockholm provide similar descriptions of the assignment process and confirm that quasi-random assignment has been used during the two decades covered in my sample.

Upon receiving the petition, the court must offer family members lawyers and hold a hear-

[7] A departmental structure is employed in the four largest courts. Each department has a chief judge and a team of judges. Typically, one department is solely focused on tax cases and the remaining departments are assigned all other cases. There are departments that solely process immigration cases in Stockholm, Gothenburg, and Malmö.

[8] While less applicable to child protection cases, the court guidelines also state that junior judges are typically not to be given a case if it includes a rare or complicated legal matter; is very big; has or can be expected to receive media attention; concerns security issues; or will likely require special experience to not delay proceedings.

ing within 2 weeks. The court administrator decides the date of the hearing based on courtroom availability and the calendars of the lawyers, judge, and law clerk. Judges are expected to be available Monday-Friday during office hours. No hearings are held after office hours or on weekends. When the date is set, the case is randomly assigned three jurors (*nämndemän*) from the pool of available jurors. The judge has no influence over the choice of jurors. The judge and jurors obtain the SWC investigation and written responses from the family a few days before the hearing. The investigation may include which group home or institution the SWC intends to place the child at but rarely includes information about an intended foster family.

The court invites the concerned parties to the hearing. Attendance is not mandatory and whether a party attends should not influence the outcome of the case. The identity of the judge is revealed to all parties before the hearing. However, in contrast to the setting studied in Ash and Nix (2023), there are no public statistics on judge strictness.

The hearing typically lasts for one hour and is the only point at which the judge has direct contact with the family, if at all. Even during the hearing, contact between the judge and the family is very restricted. Family members are only allowed in the courtroom during the hearing, the judge and the family enter the courtroom through separate doors, and the judge only asks direct questions when needed (questions are otherwise asked by the lawyers and SWC workers). Contact between judges, SWC workers, and lawyers is restricted as well to ensure an unbiased judgment.

The judge and three jurors hold deliberations immediately after the hearing. The deliberations usually take less than 15 minutes and end with a vote. Each vote is given equal weight, but the judge holds the tiebreaker. The sole task of the court is to decide whether or not the child is to be placed in out-of-home care. The assigned judge and jurors cannot, for example, decide for how long or in what form care is provided as all other aspects of care are decided by the SWC. Hence, there is only one judiciary outcome.

If the court does not rule in favor of out-of-home placement, the child cannot be removed from home. The SWC must then continue to offer support services (e.g., a support family that can care for the child part-time) but the family can decline such services. The SWC can submit a new petition for removal only if the petition is not based on the same grounds. Of the children whose first petition is denied by the court, 19.5% are removed when their second petition is submitted. The average time between the first and second court cases is 2.5 years.[9]

If the court rules in favor of child removal, the SWC decides where the child should be placed. These decisions by the SWC can be appealed to the court. Appeals are treated as standalone cases and judges are quasi-randomly assigned to such cases, irrespective of previous experience

[9]Calculations are based on the subsample of children with personal identity numbers whose first court case was decided in 2010-2015. Thereby, I can follow the child's future involvement with the court system for at least 4 years.

with the concerned parties. The only exception is appeals in which the SWC denied the family's request for termination of care. Such appeals will only be quasi-randomly assigned to the judge pool leaving out the judge who ordered out-of-home care in the first place.

2.2 Care Conditions

2.2.1 Placement Types and Duration

The most common placement option is foster home. However, half of the sample is placed in a group home or institution at least once during the first 6 months (Table B1).

The former placement type implies living in the private home of a family. Foster homes must be investigated and deemed suitable by the SWC before a child is placed. The investigation must include interviews, home visits, references, and collection of administrative data from the Social Insurance Agency, the Enforcement Authority, and the SWC's own registers. The SWC must also request a criminal record extract. However, crimes are not observable in the criminal record extract after a certain amount of time. For example, if a person is sentenced to prison or forensic psychiatric care, the crime is removed from the extract 10 years after release. There is no rule against recruiting foster parents with debt, criminal histories, or substance use problems.

It is increasingly common that foster homes are run as businesses (Swedish Agency for Health and Care Services Analysis, 2016). Indeed, taking in foster children can generate substantial income. According to the Swedish Association of Local Authorities and Regions (2023), SWCs should pay foster families at least 16,050 SEK per child each month. Depending on the needs of the child, the monthly payment can be over 25,000 SEK. With two foster children, the payment amounts to at least 32,100 SEK, which is close to the median salary in Sweden (34,200 SEK). As there is no limit on the number of children a foster family can take on, the total compensation can be well above the median income in Sweden.

Despite the high compensation, there is a national shortage of foster families in Sweden. Part of the explanation can be the lack of foster family rights and the high level of uncertainty when accepting a foster child. The birth parents still have custody of the child while the child is in out-of-home care and the goal of all placements is family reunification. Even if a child has been placed in the same foster family since birth and the child views the foster family as their only family, the SWC can terminate the placement if the birth parents can offer a suitable home environment. The foster family is obligated to strive for reunification and follow the visitation schedule and general plan set by the SWC, regardless of whether the foster family believes it is in the best interest of the child. Once the placement ends, the foster family has no visitation rights.

In group homes and institutions, multiple children live together while supervised by staff. Group homes are often privately owned and vary in size and orientation. This placement type

is similar to wilderness programs, therapeutic boarding schools, and other forms of residential facilities for 'troubled teens' used in the US and elsewhere. However, in Sweden, all such residential homes, programs, and schools must be authorized by The Health and Social Care Inspectorate and registered as official group homes. The manager and staff members must be suitable for their position, including suitable education and experience. The potential revenue is high. In 2016, the average price charged by private group homes per child and day was 3,600 SEK or 340 USD (Swedish Agency for Health and Care Services Analysis, 2016).

Institutions are secure facilities managed by The National Board of Institutional Care (NBIC) and are akin to juvenile detention centers. In fact, youths who commit serious offenses are almost exclusively sentenced to serve time in the same institutions as children taken into care rather than serve time in an adult prison. These youths are not part of the analysis sample as they enter care through the criminal, rather than the administrative, court system. Staff at institutions have the authority to take coercive measures such as body searches, communication restrictions, solitary confinement, and isolation. These coercive measures are used frequently. For example, 1 in 5 children are put in isolation at least once during their first placement at NBIC. On average, children in isolation spend 33 days isolated from the other children (Helénsdotter, 2025b).

Of court-ordered placements terminated in 2019, 26% ended with family reunification, 24% turned into a voluntary placement, 11% ended with a new involuntary placement, and 39% ended with another outcome (Table B2). Adoption is extremely rare and only allowed if both birth parents agree. The SWC must reassess the need for care every six months. At the latest, placement is terminated when the child turns 18 in environment cases and 21 in behavior cases (NBHW, 2020). As shown in Table B1, the average placement length following court-ordered removal is 25 months. Figure B1 displays the share of children still placed in out-of-home care t months after being removed from their homes.

2.2.2 Healthcare

Treatment of substance abuse is one of the responsibilities of the child protection system in Sweden. Hence, there are well-organized substance abuse treatment programs, actors within the child protection system are educated and trained on how to manage children with substance use problems, and the environment is oftentimes tailored to the needs of substance abusers. However, all other mental and physical illnesses are the responsibility of the regular child and adolescent healthcare system. Therefore, the child protection system is not equipped to provide care for children suffering from mental illnesses other than substance use disorder (Swedish Government, 2002).

Children who suffer from psychiatric disorders, regardless of whether they are in out-of-home care, receive psychiatric treatment in specialized child and adolescent psychiatric units (*Barn-*

och ungdomspsykiatrin) and, if they have a functional impairment, in the child and adolescent habilitation units up until the day they turn 18. If the child is placed outside the area of their original provider, the child must enter a new provider's queue for an appointment. Since the birth parents still have custody, the birth parents, SWC, and foster parents (or staff members) must coordinate to book an appointment. After an appointment is requested, 54% of children (as of June 2024) must wait more than 30 days for their first appointment and 70% must wait more than 60 days for their first treatment (Swedish Association of Local Authorities and Regions, 2024). Hence, lengthy discontinuities in treatment can arise.

Upon turning 18, the young adult must seek treatment at adult healthcare units and start to pay a fee for each visit. Managing the migration from the child to the adult healthcare system as an 18-year-old is challenging in Sweden. At the same time, the majority of children age out of care at 18. Children who age out of care at 21 are often transferred to a new unit within the social welfare system when turning 18 and are assigned a new caseworker. Hence, the support available to the young adult typically changes drastically.

2.2.3 Care Quality

Sweden is regarded as having a quite strong child protection system in terms of the practices employed (FRA, 2015). For example, the SWCs in Sweden are obligated to maintain contact with the child during the placement, conduct follow-ups, and make adjustments when needed. However, according to the Health and Social Care Inspectorate (2017), 40% of SWCs do not meet the required level of placement supervision.

Swedish government agencies have repeatedly found widespread and systemic deficiencies in out-of-home care, including denied or limited access to health and dental care; inadequate provision of schooling; and unlawful use of isolation, communication restrictions, physical restraint, collective punishment, and nude body searches. Deficiencies in the provision of care have been directly linked to deaths (see The Ombudsman for Children, 2010, 2011, 2019, for overviews). In a government report (SOU, 2011:9), the investigators conclude that a large number of children are subject to severe forms of abuse and neglect while placed in out-of-home care. Among the known cases, children abused and neglected in foster families are overrepresented, which might be explained by greater surveillance and training in group homes and institutions.

Another potential explanation is the inadequate vetting of foster families. The Health and Social Care Inspectorate (2017) documents that a majority of SWCs fail to vet foster families before children are assigned to the families. In addition, more than 1 in 4 SWCs do not appropriately match the needs of the children to the foster families' resources.

Severe deficiencies have been found in other placement forms as well. In a recent report, the Health and Social Care Inspectorate (2024) concludes that there are deficiencies in half of the

368 investigated group homes. Physical violence with peer perpetrators was an issue in 11% of the group homes, while physical violence at the hands of staff members occurred in 7% of the group homes. Evidence of sexual violence was found in 3% of the group homes. In a Swedish survey, 25% of children in out-of-home care reported having been abused by personnel or foster parents, and 64% claimed that they had been abused by fellow foster children or someone else (Riksförbundet Attention, 2021).

In line with the findings of these investigations, studies conducted in Western countries document large unmet health needs (e.g., low immunization coverage, untreated dental decay, and underdiagnosis and suboptimal treatment of medical conditions) among children living in out-of-home care (Kaltner and Rissel, 2011; Fontanella et al., 2015; Randsalu and Laurell, 2018). Resource shortages, lack of formal policies to track healthcare delivery, limited access to the child's medical history, and frequent discontinuity of healthcare are some of the identified barriers to healthcare delivery (see Deutsch and Fortin, 2015, for an overview).

3 Data

3.1 Data Description

The primary data source is child protection judgments that I collected from Swedish courts, The Swedish National Archives, and Stockholm City Archive (for data collection details, see Helénsdotter, 2025a). I transcribed these judgments using a mix of automated and manual techniques and manually verified that each document was accurately transcribed. I extracted a number of variables including the personal identity number of the child, whether siblings are part of the same case, petition grounds, whether any child or parent consents to removal, judgment, and judge name and title from the documents using scripts. I also classified whether the case is largely based on concerns for the child's mental health and whether it is a non-junior case type (see Appendix A for details).

I have universal coverage between February 15, 2010, and December 31, 2019. From January 1, 2005, to February 14, 2010, the collection includes all judgments at nine courts and department 6 at the court in Stockholm. Before January 1, 2005, only judgments handed down by department 6 at the court in Stockholm are included. The full court sample consists of 26,579 child-by-case observations spanning 2001 to 2019.

I added administrative data from the National Courts Administration. The data include records (name, year of birth, gender, courthouse, and date of employment by position) of all judges registered at an administrative court. Name was sufficient to uniquely identify each judge except for two pairs of judges. For these pairs, I combined the full name with the courthouse or employment period to identify the judge. Using these data combined with yearly register data containing full

name, year of birth, district of residence, employment, and education at the national level, Statistics Sweden was able to link the deciding judge with the judge's personal identity number for all but 11 child-by-case observations.

I have accurate personal identity numbers on the child for 94.2% of the sample. Missing accurate personal identity numbers is almost always due to not yet having been assigned one because of recent first-time immigration or birth. Using these identifiers, Statistics Sweden matched the children to their parents. From Statistics Sweden, I received data on, e.g., gender, birth date, immigration/emigration dates, foreign background, education, labor income, and marital status of both children and parents (Statistics Sweden, 2022, 2023a, 2023b, 2023c).

Information on all deaths (date and cause) comes from the National Cause of Death Register (1998-2023) kept by the National Board of Health and Welfare (NBHW, 2022a). I also obtained data on all hospitalizations at Swedish hospitals (private and public) from the National In-Patient Register (1998-2023) and all outpatient visits (2013-2023) to a specialist (private and public) from the National Out-Patient Register (NBHW, 2022b). When exploring mechanisms, I make use of placement data from the Register on Service Provision to Children and Young Persons (2000-2022). This register is supposed to include all 24-hour care interventions provided to people under the age of 21 but it suffers from underreporting (NBHW, 2021).[10]

Moreover, I obtained data on all legal proceedings (date of crime, date of decision, and section of the law) for the years 1998-2022 from the National Council for Crime Prevention (NCCP, 2023).[11] Data on all institutional placements at NBIC facilities (2000-2021) comes from Helénsdotter (2025b). SWC quality data comes from NBHW's 2018 quality survey, which was sent to all SWCs in Sweden and has a response rate of 92% (NBHW, 2024). See Appendix A for variable definitions.

3.2 Judge Removal Tendency

As described in Section 4, I use an IV design to isolate exogenous variation in removal decisions by exploiting variation in judges' propensity to remove children from home. I follow standard practice in the literature and calculate judge j's removal tendency in case c as the total number of children judge j removes minus the number of siblings judge j removes in case c divided by

[10]Before 2014, all municipalities reported changes in 24-hour care interventions that occurred during the previous year to the register. Due to administrative changes, the quality and coverage of the data deteriorated during 2014-2021. In each year during this period, 4-13 of Sweden's 290 municipalities failed to submit their data and there were few manual quality checks. No register was created in 2017. I do not use data from this register in the main analysis.

[11]The legal proceedings register includes all crimes in which guilt has been established and includes convictions, penalty orders without a court hearing, and waivers of prosecution.

the total number of children processed by judge j minus the number of siblings in the case:

$$Z_{j(c)} = \frac{1}{n_j - n_{j(c)}} \left(\sum^{n_j} R_{j(i)} - \sum^{n_{j(c)}} R_{j(i)} \right), \tag{1}$$

where $Z_{j(c)}$ is judge j's removal tendency score in case c, n_j is the total number of children processed by judge j during the sample period, $n_{j(c)}$ is the number of siblings in case c, and $R_{j(i)}$ is an indicator taking the value 1 if judge j decides to remove child i from home. By constructing judge removal tendency in this manner, I allow for variation in removal decisions between siblings in the same case. I also rid the measure of a mechanical relationship between removal tendency and decisions in case c by excluding all decisions made in the case.

When I calculate judge removal tendency, I start with all possible cases (even those not included in the analysis sample). To limit measurement error, I drop cases processed by a judge who handles fewer than 25 cases during the sample period. Judge removal tendency (mean: .885, sd: .066) is thus calculated on a sample of 20,538 observations.[12]

3.3 Sample Creation and Descriptive Statistics

This section describes the construction of each analysis subsample, which varies depending on the outcome and availability of register data.

I drop children who I cannot observe in Statistics Sweden's register data (N=1,578), cases with missing information on judge removal tendency (N=5,722), and cases in court-by-year cells containing only one active judge after imposing the previous restrictions (N=94). The final sample (N=19,185) consists of 15,406 unique cases (18,079 unique kids) assigned to one of 250 judges. I use this sample to study all-cause mortality in the months following the court's judgment and refer to it as the 'All Ages Sample'.

When studying the effects of removal on mortality by the year the child turns 19, I further restrict the sample to children who turn 19 by the end of the mortality data (year 2023) whose cases are decided before the year they turn 19. The sample (N=11,103) is referred to as the 'Year 19 Sample'.

Moreover, when studying suicide and overdose during the months following the court's judgment, it is reasonable to exclude children who are too young to self-harm or use harmful substances. The youngest child who died from suicide or overdose within 48 months was 10 at the time of the judgment. Hence, I limit the 'All Ages Sample' to children who were at least 10 years old. This sample (N=11,852) is referred to as the '$\geq$10 y.o. Sample'.

[12] The main instrument is highly correlated with yearly judge removal tendency (the leave-out mean removal rate based on cases processed by the same judge in the same year). Regressing yearly removal tendency on the main instrument (while controlling for court-by-year FEs) yields a point estimate of 0.944 (std. err.: 0.012, p-value$<$0.001).

Table 1 displays statistics at the child and birth parent level (Panel A) and judge level (Panel B). Descriptive statistics are almost identical when taking into account attrition (Table B3). For comparison purposes, the first column shows statistics for the full court sample conditional on being observed in Statistics Sweden's register. The child and parent statistics reported in the first and second columns are similar, but the sample restrictions applied when constructing the 'Year 19' and '$\geq$10 y.o.' samples raise the mean child age. Moreover, the judge characteristics are different in the first column. The reason is that, by restricting the sample to cases assigned to judges who process at least 25 cases, almost all cases handled by junior judges are excluded. Since junior judges are younger and more likely to be female, these statistics are affected as well.[13] However, the average judge removal tendency is unaffected. In fact, judge removal tendency (0.89) is similar across all analysis samples, which is the first piece of evidence supporting random assignment.[14]

Statistics at the court level are provided in Table B4. The composition of courts is largely the same in the national sample when imposing the sample restrictions used to create the 'All Ages', 'Year 19', and '$\geq$10 y.o.' samples.

Figure 2 depicts the average risk of the child being hospitalized (due to mental health or substance use) or committing an offense (non-narcotic or narcotic) around the time of the judgment. The date of the crime, rather than the date of conviction or reporting, is used for crime outcomes. Probabilities for removed and non-removed children are shown separately. For each event, there is a steep rise in the months preceding the judgment, which is expected given that these events can prompt the SWC to file for removal. There is then a sharp drop around the month of the judgment to levels that are more in line with those observed 12 months prior to the judgment. Both the rise and drop are especially prevalent for removed children (i.e. there is selection into removal). This is true for all events except hospitalization for mental health, which is unsurprising since mental illness is not grounds for removal while substance abuse and criminality are.

The drop starts before the judgment, which might be due to incapacitation effects from emergency out-of-home placement or deterrence effects in light of the risk of future removal. After the judgment, event probabilities are fairly similar for removed and non-removed children. All in all, Figure 2 illustrates that it is difficult to use event studies to estimate the causal effects of removal in this context.

[13] The share of female judges is lower in the 'Year 19 Sample' compared to the other analysis samples, which is expected since the share of female judges has increased over time and the 'Year 19 Sample' contains a larger share of children whose cases were handed down at the beginning of the sample period (because they are more likely to turn 19 by the end of the data).

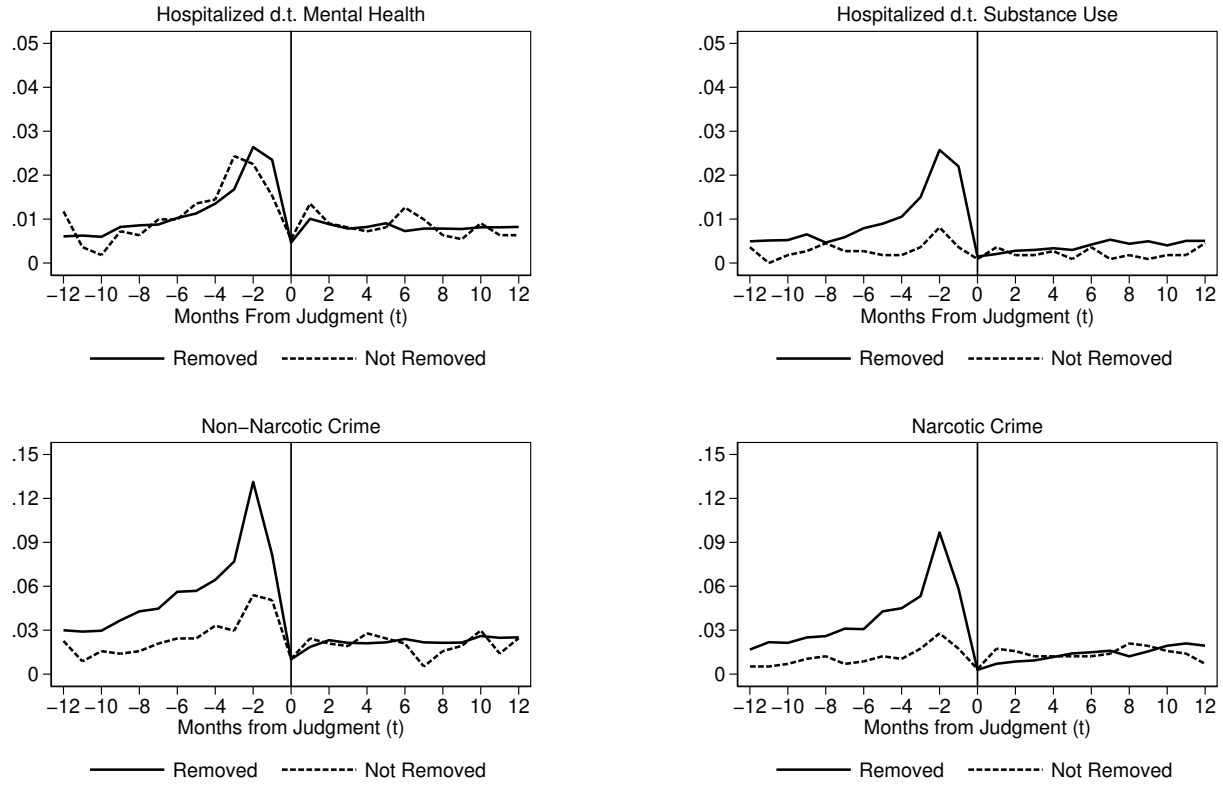
[14] The average judge removal tendency is not comparable with the tendency of child protection caseworkers because, in this setting, the child protection caseworkers have already decided to submit a petition for removal. In the full sample of Swedish child protection investigations, the rate of court-ordered removal is less than 5% (SOU, 2015:71).

Table 1. *Descriptive Statistics*

		All Ages Sample		Year 19 Sample		≥10 y.o. Sample	
	All in Registry	All	Death	All	Death	All	Suicide or Overdose
A: Child & Parent Characteristics							
Removed	0.89	0.88	0.93	0.90	0.89	0.91	0.94
Girl	0.46	0.47	0.34	0.47	0.40	0.46	0.38
Age at judgment	10.83	10.75	15.35	14.21	14.45	14.79	16.58
Sibling case	0.32	0.33	0.06	0.19	0.11	0.18	0.07
Foreign background	0.38	0.38	0.28	0.42	0.26	0.42	0.24
Behavior petition	0.29	0.28	0.75	0.42	0.59	0.45	0.84
Environment petition: Abuse	0.40	0.41	0.09	0.29	0.19	0.27	0.07
Environment petition: Neglect	0.20	0.21	0.08	0.12	0.09	0.11	0.04
Double grounds petition	0.11	0.10	0.08	0.17	0.14	0.16	0.06
Child consents to removal	0.64	0.65	0.35	0.47	0.50	0.50	0.33
At least 1 parent consents to removal	0.36	0.36	0.60	0.50	0.64	0.47	0.67
Case largely based on child mental health	0.04	0.04	0.05	0.06	0.07	0.07	0.05
Non-junior case type	0.16	0.17	0.11	0.09	0.10	0.08	0.08
High school age (16-19 y.o.)	0.27	0.26	0.68	0.39	0.39	0.42	0.76
Eligible for high school, yr t-1	0.28	0.27	0.33	0.25	0.32	0.27	0.35
Committed (yrs t-1 to t-3):							
Crime against person	0.09	0.09	0.25	0.11	0.10	0.12	0.22
Narcotic crime	0.10	0.10	0.30	0.11	0.17	0.13	0.33
Other crime	0.11	0.11	0.32	0.13	0.12	0.15	0.34
Hospitalized (yrs t-1 to t-3) due to:							
Mental health	0.06	0.06	0.14	0.08	0.14	0.09	0.17
Substance use	0.05	0.05	0.17	0.05	0.12	0.06	0.21
Other cause	0.15	0.15	0.26	0.16	0.19	0.16	0.23
Missing, yrs t-1 to t-3	0.23	0.24	0.11	0.11	0.04	0.11	0.05
Any birth parent:							
Dead	0.05	0.05	0.07	0.06	0.06	0.06	0.07
<18 y.o. at birth of child	0.02	0.02	0.01	0.02	0.02	0.02	0.00
Married, yr t-1	0.45	0.45	0.45	0.49	0.44	0.49	0.49
No labor income, yr t-1	0.63	0.63	0.49	0.57	0.61	0.55	0.49
Hosp. d.t. mental health, yr t-1	0.07	0.07	0.08	0.06	0.07	0.05	0.05
Hosp. d.t. substance use, yr t-1	0.06	0.05	0.03	0.04	0.05	0.04	0.03
Hosp. d.t. other cause, yr t-1	0.27	0.28	0.17	0.21	0.18	0.20	0.15
Any crime, yr t-1	0.17	0.17	0.12	0.12	0.23	0.11	0.11
Missing Xs, yr t-1	0.24	0.24	0.23	0.27	0.24	0.27	0.23
B: Judge Characteristics							
Judge removal tendency	0.89	0.89	0.90	0.88	0.91	0.89	0.90
Junior judge	0.15	0.03	0.05	0.03	0.01	0.03	0.06
Female judge	0.53	0.49	0.57	0.47	0.49	0.49	0.56
Judge age	49.74	52.53	51.45	52.63	53.56	52.49	51.21
Unique judges	840	250	102	250	63	250	80
Unique cases	20210	15406	155	10136	80	10979	104
Unique children	23188	18079	144	10436	70	11163	96
Unique birth parents	31672	24913	270	16407	133	17749	179
N	25001	19185	155	11103	80	11852	104

Note: This table presents descriptive statistics on child, parent, and judge characteristics for all children who are observed in Statistics Sweden's register and each analysis sample as described in Section 3.3. Statistics for children who die are presented separately. Statistics are shown for observations with non-missing information.

Figure 2. *Child Event Before and After Month of Judgment*



Note: This figure presents the raw probability of an event (indicated in the subfigure heading) occurring in a given month before or after the month of the judgment. Probabilities are presented separately for removed (black line) and not removed (dashed line) children. The '≥10 y.o. Sample' is used. In the two bottom subfigures, the sample is further restricted to children who had reached the age of criminal responsibility (15) at the time of the judgment.

4 Empirical Methodology

4.1 Instrumental Variable Model

The aim is to estimate the causal effect of removal on child outcomes. Consider the model:

$$Y_{i,c,t} = \beta R_{i,c,t} + X'_{i,c,t}\theta + \eta_{i,c,t}, \tag{2}$$

where $Y_{i,c,t}$ is an outcome measured for child i whose case c is decided in year t, $R_{i,c,t}$ is an indicator variable equal to 1 if the court orders the child to be removed from home, $X'_{i,c,t}$ is a vector of child and parent controls, and $\eta_{i,c,t}$ is an error term.

Even with a rich set of child and parent controls, estimates of β using OLS are likely plagued by omitted variable (OV) bias. Factors that can be difficult to measure and control for, while being correlated with the removal decision, include severity of abuse and addiction. To isolate exogenous variation in removal, judge removal tendency is used as an instrument for removal in

a two-stage least squares (2SLS) procedure. As described in Section 3.2, judge removal tendency is measured as the leave-out mean removal rate. The first-stage equation in the 2SLS model is:

$$R_{i,c,t} = \pi Z_{j(c)} + \alpha_{h,t} + \epsilon_{i,c,t}, \tag{3}$$

where $Z_{j(c)}$ is the removal tendency of judge j in case c, $\alpha_{h,t}$ are court-by-year FEs, and $\epsilon_{i,c,t}$ is an error term. In line with previous studies using judge instruments (e.g., Bhuller et al., 2020), court-by-year FEs are included because case randomization takes place among the pool of judges who are available at the court with jurisdiction. Since the sample includes multiple courts and spans almost two decades, I allow for variation in case characteristics and judge removal tendency across courts and over time. Since judges are assigned to cases (which may contain siblings), I cluster the standard errors at the case level (Abadie et al., 2023; Chyn et al., 2024).

By using an IV design, I can estimate the local average treatment effect (LATE), i.e. the effect of treatment on compliers. Compliers are children who could have been subject to another decision had another judge been assigned to their case. I also estimate marginal treatment effects (MTEs) and construct other parameters of interest as weighted averages of the MTEs.

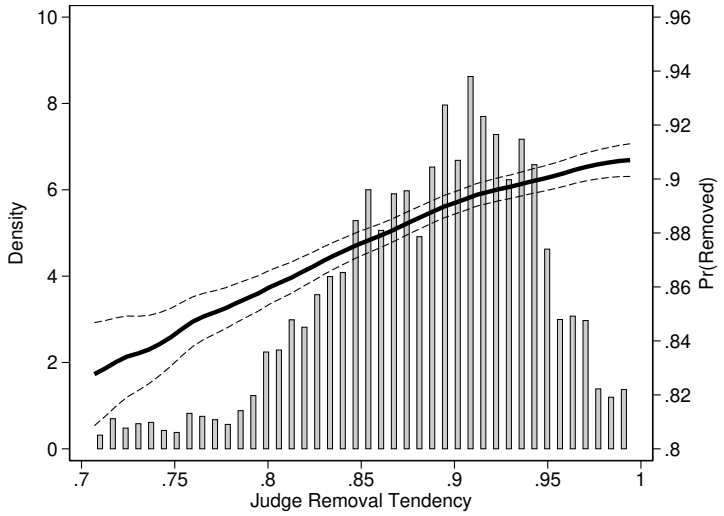
4.2 Instrument Relevance

To identify the effects of removal using judge removal tendency as an instrument, judges' underlying tendencies must be relevant for the removal decision. Figure 3 provides a graphical representation of the identifying variation. The shaded bars depict the distribution of the residualized (using court-by-year FEs) and mean-standardized judge instrument. Even after residualization, there is substantial variation in the instrument (mean: 0.885, std. dev.: 0.059; min: 0.640; max: 1.089), where a judge at the 10th percentile removes 81% of cases and a judge at the 90th percentile removes 95%. To Figure 3, a flexible regression of removal on judge removal tendency is added, showing that the likelihood of being removed is monotonically increasing in the instrument.

To formally assess whether judge removal tendency is a relevant instrument, I regress a dummy for whether the child is removed on judge removal tendency in each analysis sample and present these first-stage estimates in Table B5. In Panel A, I only include court-by-year FEs while in Panel B, I add controls for child and parent characteristics (as listed in Table 1, Panel A). Irrespective of the sample and whether extra controls are added, the coefficient is large, positive, and highly significant with an effective F-statistic around 50-70.[15] The point estimate varies somewhat between the analysis samples, which is unsurprising given that the characteristics of the samples differ. The point estimate of 0.4 in the 'All Ages Sample' implies that being randomly

[15] As noted in Bhuller et al. (2020), the judge 2SLS model has one moment condition and, hence, only one instrument even though there are many judges in the sample.

Figure 3. *First-Stage Graph of Removal on Judge Removal Tendency*



Note: This figure depicts the first-stage relationship between removal in case c and judge removal tendency. The baseline 'All Ages Sample' is used (see Section 3.3). The histogram shows the density of judge removal tendency (leaving out the top and bottom 1%). The solid line shows a Kernel-weighted local polynomial regression of removal on removal tendency. The dashed lines show 90% confidence bands. Removal and judge removal tendency are residualized using court-by-year FEs and mean-standardized. Settings: triangle Kernel, degree 0, and bandwidth 0.1.

assigned a judge with a 10 percentage point higher removal rate increases the probability of being removed from home by 4 percentage points. A first-stage estimate of 0.3-0.5 is common in the decision-maker IV literature (e.g., Doyle, 2008; Bhuller et al., 2020). The estimate is not expected to be 1 since I include covariates and have a limited number of observations per judge. In Table B6, I re-estimate the first stage using various subsamples, specifications, and instrument definitions. Each regression yields a positive, highly significant estimate.

4.3 Random Assignment

The second required assumption is that the instrument is as good as randomly assigned, i.e. uncorrelated with the error term in reduced form where reduced form refers to the regression of the outcome on the instrument.

As described in Section 2, judges are expected to be assigned to cases quasi-randomly (conditional on observable controls) given the features of the institutional setting. Table 2 provides strong empirical evidence that judges are randomly assigned, conditional on court-by-year FEs. The first column regresses removal on 30 background variables. Important predictors of removal are, e.g., petition grounds and whether the case is largely based on concerns for the child's mental health. I then regress judge removal tendency on the same set of characteristics. In line with random assignment, the estimated coefficients are now close to zero, lack individual significance,

Table 2. *Test of Random Assignment of Judge Removal Tendency*

	Removed		Judge Removal Tendency	
	Coeff	Std err	Coeff	Std err
Girl	-0.0044	0.0047	0.0011	0.0009
Age at judgment	0.0023**	0.0009	-0.0002	0.0002
Sibling case	-0.0310***	0.0083	-0.0003	0.0016
Foreign background	0.0273***	0.0067	0.0006	0.0014
Behavior petition	0.0190**	0.0077	0.0016	0.0017
Environment petition: Abuse	-0.0948***	0.0100	-0.0011	0.0020
Environment petition: Neglect	-0.1119***	0.0118	-0.0030	0.0023
Child consents to removal	0.2454***	0.0097	-0.0002	0.0015
At least 1 parent consents to removal	0.0658***	0.0065	-0.0004	0.0014
Missing consent data	0.1460***	0.0221	0.0032	0.0043
Case largely based on child mental health	-0.0380**	0.0157	0.0003	0.0028
Non-junior case type	-0.0121	0.0080	0.0006	0.0015
High school age (16-19 y.o.)	0.0175**	0.0077	0.0003	0.0016
Eligible for high school, yr t-1	-0.0076	0.0087	0.0031	0.0019
Committed (yrs t-1 to t-3):				
Crime against person	0.0117	0.0080	0.0004	0.0020
Narcotic crime	0.0468***	0.0073	0.0004	0.0019
Other crime	0.0059	0.0078	-0.0012	0.0019
Hospitalized (yrs t-1 to t-3) due to:				
Mental health	0.0039	0.0096	0.0014	0.0021
Substance use	0.0074	0.0094	-0.0013	0.0024
Other cause	-0.0195***	0.0066	-0.0009	0.0013
Missing, yrs t-1 to t-3	0.0205***	0.0078	0.0010	0.0016
Any birth parent:				
Dead	0.0291**	0.0128	0.0018	0.0026
<18 y.o. at birth of child	-0.0145	0.0185	-0.0002	0.0037
Married, yr t-1	0.0087	0.0067	-0.0005	0.0014
No labor income, yr t-1	0.0024	0.0068	-0.0001	0.0014
Hosp. d.t. mental health, yr t-1	0.0184	0.0126	-0.0032	0.0026
Hosp. d.t. substance use, yr t-1	0.0041	0.0144	0.0029	0.0027
Hosp. d.t. other cause, yr t-1	0.0074	0.0075	-0.0002	0.0015
Any crime, yr t-1	0.0245***	0.0089	-0.0002	0.0017
Missing Xs, yr t-1	0.0027	0.0094	-0.0007	0.0018
F-statistic	33.14		0.57	
p-value	0.00		0.97	
N	19185		19185	

Note: Test of random assignment of judge removal tendency to cases using the 'All Ages Sample'. Reported *F*-statistic of joint significance is for the displayed variables. All estimations include court-by-year dummies. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

and are not jointly significant (*F*-statistic: 0.57). In other words, child and parent characteristics that predict removal are not correlated with the instrument. For half of the variables, the coefficient from the balance check even has the opposite sign as the direct relationship with removal. Results from additional randomization tests are presented in Table B7. Irrespective of the test I run, I find small *F*-statistics.

4.4 Exclusion Restriction

While random assignment is sufficient to achieve a consistent estimator in reduced form, the estimator of the parameter of interest (β_t) is not necessarily consistent. To achieve the latter, the instrument must satisfy the exclusion restriction, which means that judge removal tendency must exclusively affect child outcomes through the removal decision. If, for example, a judge with a high removal tendency also is inclined to order the parents to complete support programs, and completion of such programs affects child outcomes, the exclusion restriction is violated. Fortunately, as described in Section 2, the assigned judge only makes a single, binary decision in the type of cases I study and has little to no contact with the family.

A test of the exclusion restriction, joint with random assignment and the strong monotonicity condition is provided by Frandsen et al. (2023). In line with the validity of the three assumptions, I cannot reject the null hypothesis for any of the main outcomes (Table B8).

In Table B9, I provide further empirical support for the exclusion restriction by documenting that judge removal tendency is uncorrelated with case and placement characteristics conditional on court-by-year FEs.

4.5 Monotonicity

A standard assumption invoked in heterogeneous IV models has up until recently been Imbens and Angrist (1994) monotonicity, also known as strong monotonicity. The assumption implies that if judge J is overall more likely to remove children from home than judge K, then *every* child removed by judge K would also have been removed by judge J had judge J been assigned the case. This is a strong assumption and its validity in empirical settings has been questioned (Mogstad et al., 2021; Norris et al., 2021; Chan et al., 2022; Frandsen et al., 2023; Sigstad, 2023). In Section 4.4, I apply Frandsen et al. (2023)'s test and find evidence in support of strong monotonicity.

Nevertheless, strong monotonicity is not necessary to ensure that the IV estimand is a weighted sum of non-negative treatment effects. Instead, as shown by Frandsen et al. (2023), a weaker average monotonicity condition is sufficient. This assumption implies that, in each case, judges who decide to remove the child from home do not have a lower removal tendency than judges who decide to leave the child at home. However, as clarified in Sigstad (2023), weak monotonicity does not ensure the identification of MTEs, LATE, or some other meaningful parameter.

If the weak monotonicity assumption holds, the first-stage estimates are nonnegative for all subsamples of children. In Table B10, I slice the sample along observable dimensions and estimate the first stage in each subsample. I also use an alternative definition of removal tendency: the judge's tendency to remove children *outside* the subsample. Regardless, the estimates are large, positive, and significant in each subsample, in line with the weak monotonicity assumption. These

results suggest that judges who are prone to remove children in one subsample (e.g., girls) are also prone to remove children in the complement subsample (e.g., boys).

5 Results for Child Mortality

5.1 Baseline Results

Table 3 presents the estimated effects of court-ordered removal on all-cause and cause-specific mortality by the year the child turns 19 or by month 48 following the court's judgment. Compared to Table 1, the sample sizes are slightly smaller because of attrition stemming from emigration.[16]

A simple OLS regression of death by the year the child turns 19 on removal reveals a negative estimate that lacks statistical significance (column 1). This result may seem counterintuitive since removed children should, to a larger extent, come from deficient home environments and engage in destructive behaviors that threaten their health or development. However, suicides make up over half of all-cause deaths. At the same time, suicidal intent and mental illness are not legal grounds for removal. Nevertheless, some SWC workers attempt to protect children at risk of suicide by submitting a petition for out-of-home care (SOU, 2000:77). This practice is reflected in the overrepresentation of cases that are largely based on the child's mental health among children who are *not* removed by the judge (Table B11). Selection of children with a high risk of suicide into the control group would bias the OLS estimates for all-cause mortality downward.

When adding observable controls, the estimates barely change. However, my capacity to accurately measure factors that influence the risk of suicide and the removal decision (e.g., severity of prior self-harming behavior) is limited.

Using judge removal tendency as an instrument for removal (which addresses the issue of OV bias) reveals very different results. Removal increases the risk of the marginal child dying by the year they turn 19 by 6.8 percentage points (significant at the 5% level). Relative to the mean of 1.8% among non-removed compliers, this increase is striking.[17] I also report the Anderson-Rubin (AR) test and identification-robust confidence sets as recommended by Andrews et al. (2019). Even the lower bound of the AR confidence set implies a large increase in mortality.

[16] See Appendix C for further details on attrition. To test for selective sample attrition, I regress a dummy for missing in each analysis sample on the judge instrument. Selective attrition appears to be negligible (Table C1).

[17] To estimate the power to detect an effect of 6.8 percentage points, I use the sample of non-removed children to estimate the risk of death by the year the child turns 19 with random forest and the full set of child and parent characteristics. I then predict the mortality risk in the removed sample, adjust the prediction upward by the treatment effect, and draw 200 random samples with 11,061 observations. In each sample, I treat observations with a mortality risk above a uniformly distributed random variable as dead and re-estimate the main IV regression. I calculate the power as the fraction of samples in which the null hypothesis can be rejected at the 90% confidence level. This Monte Carlo simulation gives a power estimate of around 30%.

Table 3. *Effect of Removal on Child Mortality*

	Death by Year Child Turns 19				Death by Month 48 Post-Judgment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All-Cause	Suicide & Overdose	Suicide	Overdose	All-Cause	Suicide & Overdose	Suicide	Overdose
OLS (No Controls)								
Removed	-0.0009	-0.0018	-0.0026	0.0008	0.0036**	0.0038	0.0002	0.0036***
	(0.0028)	(0.0026)	(0.0024)	(0.0010)	(0.0017)	(0.0024)	(0.0021)	(0.0011)
OLS (With Full Set of Controls)								
Removed	-0.0040	-0.0043	-0.0045*	0.0002	0.0004	0.0007	-0.0016	0.0023*
	(0.0029)	(0.0028)	(0.0026)	(0.0010)	(0.0018)	(0.0026)	(0.0023)	(0.0013)
OLS (Complier Reweighted)								
Removed	-0.0021	-0.0033	-0.0043	0.0010	-0.0007	-0.0023	-0.0025	0.0002
	(0.0035)	(0.0031)	(0.0029)	(0.0012)	(0.0019)	(0.0028)	(0.0027)	(0.0009)
RF (Only Court-by-Year FEs)								
Judge removal tendency	0.0298**	0.0284**	0.0184**	0.0100	0.0132	0.0255*	0.0197**	0.0058
	(0.0124)	(0.0110)	(0.0088)	(0.0066)	(0.0096)	(0.0134)	(0.0095)	(0.0095)
IV (Only Court-by-Year FEs)								
Removed	0.0683**	0.0652**	0.0422**	0.0229	0.0310	0.0608*	0.0470*	0.0138
	(0.0301)	(0.0270)	(0.0212)	(0.0155)	(0.0229)	(0.0335)	(0.0241)	(0.0226)
IV (With Full Set of Controls)								
Removed	0.0673**	0.0630**	0.0405*	0.0225	0.0290	0.0577*	0.0461*	0.0116
	(0.0305)	(0.0273)	(0.0215)	(0.0157)	(0.0231)	(0.0342)	(0.0245)	(0.0234)
Sample	Year 19	Year 19	Year 19	Year 19	All Ages	≥10 y.o.	≥10 y.o.	≥10 y.o.
AR *p*-value	0.0166	0.0101	0.0369	0.1327	0.1666	0.0571	0.0382	0.5407
AR confidence set (95%)	[.015,.134]	[.017,.124]	[.003,.088]	[-.006,.056]	[-.012,.079]	[-.002,.134]	[.004,.1]	[-.03,.06]
Dependent mean	0.0072	0.0055	0.0039	0.0016	0.0081	0.0088	0.0047	0.0041
Complier mean if not removed	0.0177	0.0148	0.0110	0.0039	0.0113	0.0065	0.0027	0.0038
N	11061	11061	11061	11061	19083	11811	11811	11811

Note: Columns 1-4, 5, and 6-8 use the 'Year 19 Sample', 'All Ages Sample', and '≥10 y.o. Sample', respectively. Each sample is described in Section 3.3. All estimations except *OLS (No Controls)* include court-by-year FEs. *OLS (With Full Set of Controls)*, *OLS (Complier Reweighted)*, and *IV (With Full Set of Controls)* also control for the child and parent characteristics listed in Table 1. Reported AR *p*-values and confidence sets are for *IV (Only Court-by-Year FEs)*. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

The effect on mortality is primarily driven by suicides. The IV estimate in column 3 implies that removal increases the risk of suicide by year 19 by over 4 percentage points (significant at the 5-10% level). The point estimate for accidental overdoses is positive but not statistically significant at conventional levels.

Since IV estimation captures the treatment effect for compliers, not the average treatment effect, discrepancies between OLS and IV estimates could be driven by effect heterogeneity rather than selection bias. In fact, the complier groups deviate from the analysis samples along several observable dimensions (Table B11). Nevertheless, reweighting the sample using complier weights yields a similar OLS estimate, which suggests that the difference in estimates is not driven by effect heterogeneity.[18]

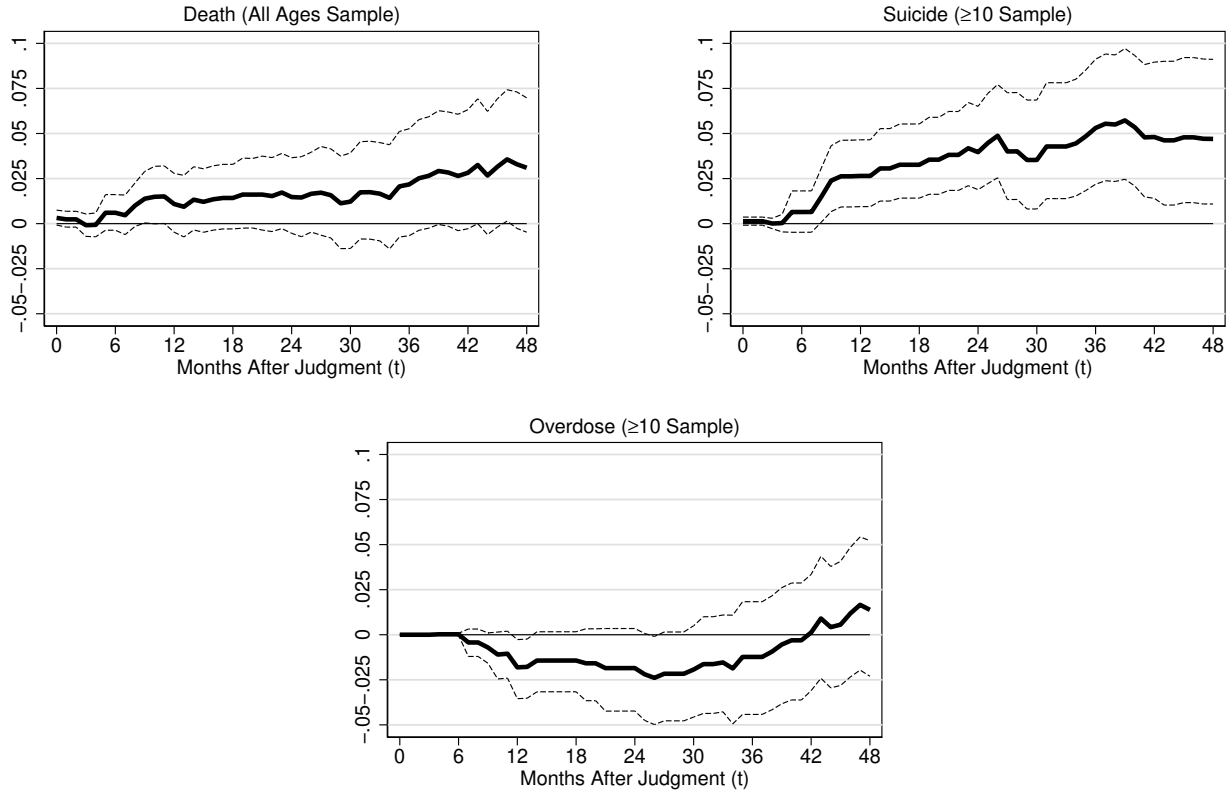
Turning to all-cause mortality by month 48 following the court's judgment, the full sample of children aged 0 to 19 can be used. Since a meaningful share of these children are not old enough to engage in self-harm and substance use, it is unsurprising that the estimated effect is not statistically significant (Table 3, column 5).[19] Instead, limiting the sample to children who are at least 10 years old at the time of the judgment reveals a significant increase (10% level) in suicides by month 48.

Figure 4 graphically presents IV estimates of the effects of child removal on the cumulative risk of all-cause mortality, suicide, and accidental overdose by month t after the court's judgment (with 90% AR confidence intervals). The point estimates for all-cause death and suicide quickly turn positive and stay non-negative for the subsequent months. For all-cause mortality using the 'All Ages Sample', the intervals are wide and only a few estimates are statistically significant at the 10% level. In contrast, for suicides in the '$\geq$10 y.o. Sample', the estimates become significant (5% level) already by month 9 and remain steady for the subsequent months. For overdoses, the estimates are weakly negative before month 42 but are rarely statistically significant. Among removed children who died (regardless of cause) within 9 months of the judgment, all were continuously placed in out-of-home care up to and including the month they died.

[18] To obtain complier reweighted samples, I adopt the procedure employed in, e.g., Dahl et al. (2014), Bhuller et al. (2020), Dobbie, Goldin, and Yang (2018), and Baron and Gross (2022). First, I identify the least and most stringent judges, defined as the bottom and top 1 percentiles. I then calculate the overall proportion of compliers in each analysis sample as the difference in the first stage between children assigned the most stringent and least stringent judges. I then create subgroups that capture important heterogeneity. Specifically, I use random forest to obtain a measure of the risk of removal based on the child and parent characteristics listed in Table 1 and court-by-year dummies. I then split the analysis sample into quartiles depending on the child's risk score and follow the same procedure as for the full analysis sample to compute the share of compliers within each risk quartile. Finally, I retrieve the relative likelihood of a complier belonging to a risk quartile by dividing the share of compliers in the risk quartile by the total share of compliers. These relative likelihoods are the complier weights.

[19] The yearly death rate among the sampled children is much higher than the rate observed in the general Swedish population. In the 12 months following the court's judgment, the death rate is 63 (352) per 100,000 children in the 'All Ages' Sample aged 10-14 (15-19) compared to an average of 10 (27) per 100,000 children aged 10-14 (15-19) in Sweden during the years 2001-2020 (NBHW, 2023).

Figure 4. *Effect of Removal on All-Cause Mortality, Suicide, and Overdose*



Note: Black lines show IV estimates of the effect of removal on the cumulative probability of the child dying by month t post-judgment. The relevant outcome and sample are stated in the subfigure heading. Dashed lines show 90% AR confidence bands. All specifications condition on being in Sweden during month t or later.

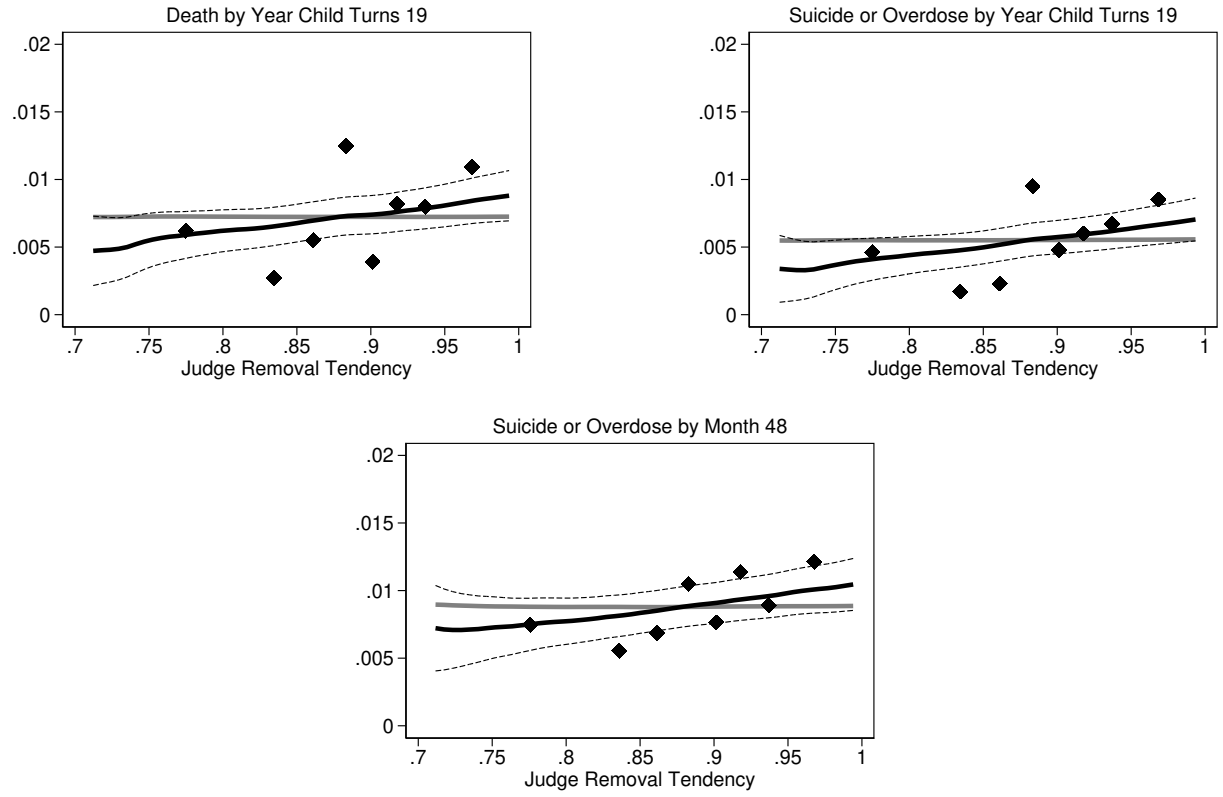
The large point estimates in the IV analysis suggest caution in interpretation. Recall that the effects are estimated for cases that judges may disagree about, which only make up around 13% of the analysis samples. This group might be more responsive to placement in terms of increased mortality than the average child because, for example, it likely contains a higher share of children with underlying mental health problems given that there is a lack of consensus on the legal mandate to remove such children via a court order (see Section 2.1). Indeed, empirically, I find that cases that are largely based on the child's mental health are more than twice as common in the complier group as in the full 'Year 19 Sample' (Table B11).

In addition, because the variation in the instrument is limited and the first-stage estimate is 0.42, the instrument only induces variation in the probability of removal from 0.78 to 0.93. While this range of induced variation is common in the judge IV literature (e.g., Aizer and Doyle, 2015; Bhuller et al., 2020), interpreting the IV estimate as the effect of a binary change in removal from 0 to 1 requires linear extrapolation. Without extrapolation beyond the support, the IV estimate in column 1 of Table 3 implies that increasing the likelihood of removal from 0.78 to 0.93 increases the probability of death by the year the child turns 19 by 1 percentage point, or 58% relative to

the control complier mean.

Is it reasonable to extrapolate the induced change in the likelihood of removal to a binary change? The IV estimator is essentially the ratio of the reduced form to the first stage. Hence, it is useful to probe the first-stage and reduced-form graphs. As shown in Figure 3, the probability of removal increases approximately linearly with the instrument. The reduced-form relationship between (actual and predicted) child mortality and judge removal tendency is explored in Figure 5. In line with conditional randomization, predicted child mortality (using child and parent background characteristics) appears unrelated to the instrument. In contrast, the relationship between actual mortality by the year the child turns 19 and the instrument is positive and appears linear. All in all, linear extrapolation can be reasonable in the current setting.

Figure 5. *Reduced-Form Graphs*



Note: The sample is the 'Year 19 Sample' in the top figures and the '≥10 y.o. Sample' in the bottom figure. Each solid black line shows a Kernel-weighted local polynomial regression of the mortality outcome (stated at the top) on judge removal tendency and the dashed lines show 90% confidence bands. The black squares indicate mean mortality among cases assigned judges with removal tendencies that fall within the same bin (8 bins of equal size). The solid gray lines show Kernel-weighted local polynomial regressions of predicted mortality (using the child and parent characteristics in Table 1) on judge removal tendency. Child outcomes and judge removal tendency are residualized using court-by-year FEs and mean-standardized. Settings: triangle Kernel, degree 0, and bandwidth 0.15.

5.1.1 Implied Additional Deaths

In total, 80 children die by the year they turn 19, of which 61 die from suicide or accidental overdose. How many of these deaths can be attributed to the removal of compliers? I estimate that the number of compliers is 1,453 of 11,061 observations. Using the control complier mean in Table 3, column 1, 0.0177*1453≈26 compliers would die if none of the compliers is removed from their home. The point estimated effect of removal on death is 0.0683 for compliers. If all judges were strict, all compliers would be removed from their homes and 0.086*1453≈125 would die.

The total number of deaths among non-removed and removed compliers is given by: [0.0177*(1-P(T|C))+(0.0177+0.0683)*P(T|C)]*1453, where T defines treated and C defines complier. The probability of treatment in the complier group, P(T|C), can be deduced using the size of the untreated group and the share of never-takers. Reasonably, there should be close to no never-takers (i.e. children who would never be removed by *any* judge) since cases are not heard in court unless a caseworker with a bachelor's degree in social work, their supervisor, and a majority in the deciding body at the SWC agree that there are legal grounds to place the child in care. If there are no never-takers, the entire untreated group is comprised of compliers, i.e. 1,123 out of 11,061 observations. The remaining 330 compliers must be in the treated group. Hence, the share of treated in the complier group is 22.71%. Then there are [0.0177*0.7729 + 0.086*0.2271]*1453≈48 deaths among compliers and 32 deaths among non-compliers, which implies that child removal causes an extra 22 deaths among compliers.

Using instead the lower end of the AR confidence set (0.0146) yields 31 deaths among compliers, of which 5 deaths are attributable to child removal. Alternatively, assuming that 2% of the sample are never-takers, the total number of deaths in the complier group rises to 63. As many as 37 of these deaths would be caused by the removal of compliers.

5.2 Heterogeneity

Table D1 presents separate effects by child characteristics (gender, siblings in the same case, foreign background, petition grounds, and age). For all subgroups, the point estimated effect of removal on death is positive, but sometimes imprecisely estimated. Wald tests of equality reveal no statistically significant differences in effect size.

Heterogeneity in treatment effects can also be explored by estimating MTEs. Figure D1 traces out MTE curves over the unobserved resistance to treatment. I also show the propensity score distribution (the probability of removal given judge removal tendency and court-by-year FEs) for removed and non-removed children in the 'Year 19 Sample'. The common support is around 0.70 to 0.98 after trimming the bottom and top 1%. The MTE curves are flat for all outcomes except all-cause death by month 48 (then it is slightly downward sloping). A downward slope means

that the adverse effect on mortality is largest for children who have low unobserved resistance to treatment (i.e. children who have unobservable characteristics that make them likely to be removed).

Table D2 presents approximations of ATE, ATT, and ATUT based on MTEs. The results reveal no evidence that child removal significantly improves mortality for the average child. However, as the common support is limited, the parameter approximations should be interpreted with caution.

5.3 Robustness Checks

In the main analysis, I employ classical 2SLS, in which linear models are used in the first and second stages. However, the treatment and outcome variables (mortality and removal) are binary and imbalanced. In such settings, alternative approaches such as nonlinear two-stage residual inclusion (SRI; Terza et al., 2008) might be more appropriate. On the other hand, Angrist (2001), among others, argue that simple 2SLS may still be preferable. Indeed, Basu et al. (2018) show that 2SLS produces consistent estimates of the LATE even when the outcome and treatment variables are binary and highly imbalanced. In Table D3, I present estimated ATEs using 2SRI with probit first and second stages. The estimated ATEs are highly significant and slightly larger compared to the LATEs in my main analysis.

Tables D4-D5 present robustness checks in which sample, specification, and instrument construction decisions are varied. The results are also robust to dropping each court.

6 Mechanisms

The analysis reveals that court-ordered removal of the marginal child from home decreases their chances of surviving childhood, with particularly large effects on the risk of suicide. I now explore four groups of mechanisms (disruption, care conditions, peers, and parents) through which removal might affect mortality. I start with a qualitative investigation of the circumstances of the deaths.

6.1 Qualitative Investigation

The SWC keeps extensive records in child protection cases. Hence, even if the caseworker is no longer employed at the SWC, the SWC usually has in-depth knowledge about the circumstances of deaths that occur among children they serve. If a child who receives services from the SWC dies and there is a risk that a deficiency in the services contributed to their death, the suspected deficiency must – according to law – be investigated, documented, and corrected. If the investigation shows that a serious deficiency exists, the Health and Social Care Inspectorate must notified. The Health and Social Care Inspectorate can then launch an external investigation.

For each unique child in the 'Year 19 Sample' who (i) was removed (N=65) and (ii) died from an unnatural cause (N=61), I reached out to the SWC in charge and inquired about the circumstances of the death in May-June 2024. I obtained information, including investigations and case notes, about 56 out of the deaths. When possible, I conducted an in-depth interview with the caseworker in charge of the case. I also collected investigations conducted by the Health and Social Care Inspectorate.

The majority of children (35 out of 56) died while placed in out-of-home care. An additional 2 died within 2 weeks after leaving out-of-home care. Harmful exposure to peers and violent victimization by peers living in the same home directly contributed to at least 20% of the deaths, including murders, rapes, and peer effects in substance abuse.

Moreover, there is evidence that quality deficiencies contributed to 55% of the deaths. The most common quality deficiency identified as a contributor is inadequate psychiatric healthcare (25 out of 56 deaths), including failure to facilitate appropriate psychiatric treatment, lack of cooperation with the healthcare system, wrongful medication, as well as long and frequent disruptions in treatment that arise when the child moves or turns 18. Other common quality deficiencies are low supervision and failure to meet the child's emotional needs.

6.2 Disruption of the Child's Environment

Removing a child from their home inherently disrupts the child's social and physical environment. The extent of these disruptions is likely larger if the child must move far from their home. To shed light on this mechanism, I create an indicator that takes the value 1 if the child moves at least once across municipalities within the first 6 months following the court's judgment. As I cannot observe where non-removed children would have been placed had the court ordered removal, I use the child and parent characteristics in Table 1, court-by-year dummies, and SWC dummies to predict across-municipality moves. Prediction is done with random forest. I then interact removal and judge removal tendency with indicators for above- and below-median risk of moving across municipalities. I use the interactions with judge removal tendency as instruments for the interactions with removal. The results are presented in Table D6, Panel A. The point estimates are positive in both groups and there is no statistically significant difference.

To further explore the role of disruptions, I exploit data on placement changes and create an indicator for whether the child experiences more than one placement change within the first 6 months. Figure D2 depicts the distribution of across-municipality moves and placement switches by month 6. I then apply the same procedure as for across-municipality moves described above. No statistically significant difference is found for children with low versus high probability of placement instability (Table D6, Panel B).

Taking stock, I find little evidence that large or frequent disruptions are the main drivers

of the adverse effects found for child mortality. However, my measures of long-distance moves and placement instability might not accurately capture important disruptions in the child's life. Hence, I cannot rule out that disruption of the child's environment is an important channel.

6.3 Care Conditions

6.3.1 The Role of Placement Type

Half of the children in the 'All Ages Sample' are placed in a group home or institution at least once within 6 months of being removed from home (Table B1). However, prior empirical evidence suggests that placements in such non-family facilities are particularly harmful (Li et al., 2019). It is therefore useful to investigate whether the effects are driven by such placements.

Panels C-D in Table D6 present estimated effects of child removal on mortality by the probability of ever being placed in a non-family facility or institution during the first 6 months following the court's judgment. The point estimate is larger for children with a high probability of non-family and institutional placement, but the differences in estimates are not statistically significant. In fact, among children with a *low* risk of non-family placement, removal increases the risk of death by the year the child turns 19 by 6.9 percentage points (5% significance level). Hence, the effects do not appear to be driven by a certain placement type.

6.3.2 SWC Quality

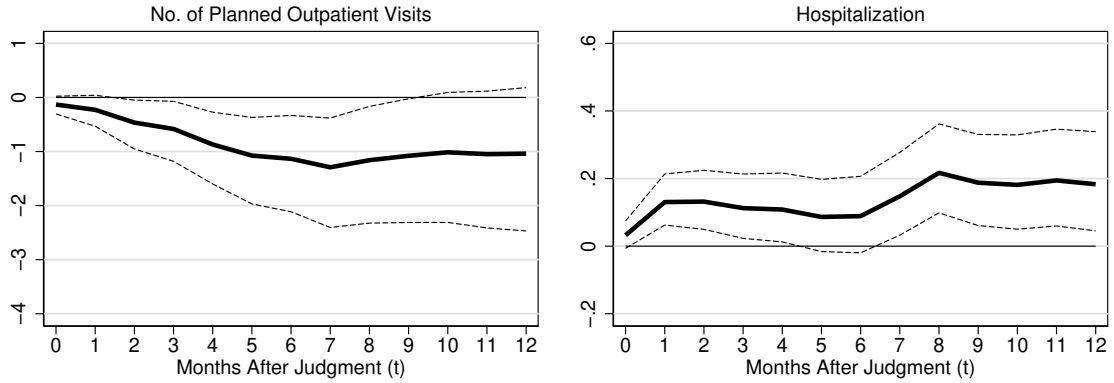
The practices employed by SWCs vary greatly. For example, in NBHW's 2018 survey, only 48% of SWCs provide information to children about their rights and how they can contact their caseworker, 51% have an agreement on collaboration with the child and adolescent psychiatry, and 37% offer psychosocial treatment. I use the survey responses to construct a quality score and investigate heterogeneity in treatment effects by whether the child's SWC is of low or high quality, where high quality is defined as a score above the 75^{th} percentile (see Appendix A). Table D6, Panel E, displays the IV results. The effect of removal on suicide and overdose by the year the child turns 19 is 7.8 percentage points larger (sig. at 10%) in SWCs with low quality compared to SWCs with high quality. In SWCs with high quality, the point estimate is close to zero (0.0006).

6.3.3 Healthcare

The qualitative investigation in Section 6.1 suggests that inadequate psychiatric treatment might be important for the adverse effects on removed children. Hence, I examine the effects of child removal on mental health-related care utilization during the first year.

Figure 6 shows the estimated effects of removal on two forms of mental health-related care: (i) the cumulative number of planned outpatient specialist visits and (ii) the cumulative probability

Figure 6. *Effect of Removal on Mental Health-Related Healthcare Utilization*



Note: Black lines show IV estimates of the effect of removal on the cumulative (i) number of planned outpatient specialist visits and (ii) probability of the child being hospitalized due to their mental health by month t post-judgment. The relevant outcome is stated in the subfigure heading. Dashed lines show 90% AR confidence bands. The '$\geq$10 y.o. Sample' is used. All specifications condition on being alive and in Sweden during months 0-t.

of hospitalization by calendar month t post-judgment. In line with the findings of my qualitative investigation, there is a reduction in the marginal child's number of planned outpatient visits for mental illness before the steep rise in suicides (and exit of these children from the sample) at month 9. The point estimate at month 8 implies that child removal decreases the number of planned outpatient visits among compliers by 1.2 visits (AR p-value: 0.054; dep. mean=0.5).

The reduction in planned outpatient visits can be driven by improved mental health or higher barriers to treatment. If child removal leads to improved mental health, we should see a decline in mental health-related hospitalizations.

There is a statistically significant (1% level) increase in the risk of being hospitalized already by the first calendar month following the court's judgment. The almost immediate jump in hospitalizations can be viewed as supporting the disruption channel discussed in Section 6.2.

The effect on mental health-related hospitalizations appears to decrease up to month 6, which is when the SWC must conduct its first review of the child's case. After the first review, the effect increases rapidly. At month 8, there is a kink that coincides with the rise in suicides. The point estimate implies that child removal increases the risk of the marginal child being hospitalized for mental illness within 8 months by 21.7 percentage points (AR p-value: 0.003; dep. mean=0.05). However, the large estimates should be interpreted with caution (see the discussion in Section 5.1).

The diverging effects for planned outpatient care and hospitalization suggest that the reduction in outpatient care is driven by higher barriers to treatment rather than improved mental health. In turn, it is plausible that the reduction in planned outpatient care contributes to the increased risk of hospitalization and death, in line with the findings presented in Section 6.1.

OLS, reduced-form, and IV estimates by month 12 are provided in Table D7. The IV estimate of 18.32 percentage points in column 1 implies that child removal causes 76 of the 121 mental health-related hospitalizations by month 12 among compliers, under the assumption that all non-treated observations (1,103) are compliers.[20]

I also offer IV estimated effects of child removal on additional healthcare outcomes (all-cause, acute, physical, and substance use) in Figures D4-D5. The estimates for all-cause and acute hospitalizations follow those for mental health-related hospitalizations over the first year, with peaks at month 8 (sig. at 5% level). There are no statistically significant effects on hospitalizations for physical illnesses or substance use. However, there is a significant increase in the number of planned outpatient visits for substance use by month 12 (5% level).

The estimated effects of removal on healthcare usage are subject to the caveat that there may be under- or over-reporting. For example, foster parents may be more likely to bring a child to the hospital than birth parents for the same level of injury (or the other way around). It is plausible that the results for hospitalizations are less affected by this issue. Physicians only hospitalize patients with severe injuries or illnesses that cannot wait or be treated within the outpatient system. Hence, if someone brings a child to the hospital when it is unnecessary the child would not be hospitalized and, thereby, such overuse would not affect the estimated effect on hospitalizations.

Further evidence in support of the healthcare channel is offered in Table D6, Panel F. The adverse effect of removal on suicides and overdoses is concentrated among children whose SWC has a low level of collaboration with the healthcare and education system. The difference in effect sizes is statistically significant at the 5% level.

6.3.4 Education

One of the many deficiencies reported in The Ombudsman for Children (2010, 2011, 2019)'s overviews is inadequate provision of schooling. This critique is mostly directed towards NBIC, which provides onsite schooling for institutionalized children. Court-ordered out-of-home placement can also affect schooling if the child must transfer schools, does not get enough help with homework, or is too distressed to focus on school. In turn, poor academic performance has been linked to mental illness and self-harm (Richardson et al., 2005). Table D7 presents the estimated effect of child removal on the likelihood of (i) becoming eligible for high school and (ii) graduating high school by the year the child turns 19, which is when Swedes usually graduate high school. The IV analysis reveals negative but imprecise point estimates.

Nevertheless, the school may play an important role: 1 in 4 deaths by the year the child turns 19 occurs during July and August, i.e. during the Swedish summer break. However, I cannot

[20] The share of compliers in the '$\geq$10 y.o. Sample' is estimated to be 12.85%.

disentangle the role of the summer break from the general effect of summer.

6.3.5 Transition to Adulthood

The adverse effects on mortality can be driven by poor post-placement conditions or the stress of having to leave care. The high share of deaths during out-of-home placement (see Section 6.1) speaks against poor post-placement conditions and the stress of care termination being major drivers of my findings. On the other hand, children might end their lives before care termination in anticipation of stress and poor post-placement conditions. To explore this channel, I examine how old the children are at the time of death.

Children who are involuntarily placed in care based on deficiencies in the home age out of care when they turn 18. Hence, a spike in deaths right before their 18th birthday could be driven by anticipation. However, none of the children in the 'Year 19 Sample' who are removed based on deficiencies in the home die in the month of their 18th birthday or within 6 months before.

There is a clustering of deaths, but among the children in the 'Year 19 Sample' who are removed (solely or partly) based on their own behavior. Specifically, 17% of the children who die by the year they turn 19 die within 2 months *after* they turn 18. It is unlikely that this pattern is driven by anticipation of having to leave care because children who are removed based on their own behavior cannot leave care before they turn 21 unless the SWC decides that care is no longer needed. Figure D3 depicts the distribution of months between the month the child turns 18 and the month of death among all children in the 'Year 19 Sample' who die by the year they turn 19.

The spike in deaths in the months right after turning 18 can be driven by several factors. When a person turns 18, they are legally considered an adult in Sweden which means that they become responsible for their own finances and can enter contracts, take out loans, gamble, shop online, and drink alcohol. In addition, the 18-year-old must manage all contact with the school, bank, healthcare system, police, and other authorities. The sudden increase in responsibility and freedom can be stressful and lead to destructive behaviors that increase the risk of suicide. A particularly salient event among children struggling with mental or physical illness can be the automatic termination of their treatment within the child and adolescent healthcare system on the day of their 18th birthday (see Section 2.2.2).

6.4 Peers

6.4.1 Peer-to-Peer Spillovers

The adverse effects on child mortality can be driven by increased exposure to peers who engage in harmful behaviors and peer-to-peer spillovers. In Helénsdotter (2025b), I shed light on this channel using data on the universe of youths placed in Swedish institutional care from 2000 to

2020. To address the issue of non-random assignment of children to facilities, I include facility-by-year fixed effects and estimate peer effects using only temporal variation in peer composition within each facility and year. A test of the identifying assumption is offered in Table D8.

In Table D9, I reproduce the main results of Helénsdotter (2025b) while limiting the sample to children who are removed from home by an administrative court, i.e. the population considered in this paper. The results are effectively the same as in Helénsdotter (2025b). Columns 1-3 suggest that there are reinforcing and persistent peer effects in substance abuse. Relative to the average risk of a future substance abuse-related event (e.g., hospitalization) among children with a history of substance abuse (50.9%), the estimate of 0.15 in column 1 implies that a 1-standard deviation increase in peer exposure (0.21) increases the risk of experiencing a serious substance abuse-related event within 12 months of being discharged from NBIC by 6%.

Similarly, columns 4-6 suggest that there are reinforcing and persistent peer effects in self-harm. Children with a history of self-harm face an average risk of being hospitalized or dying from self-harm of 14.8%. Hence, the estimate of 0.30 in column 4 implies that a 1-standard deviation rise in peer exposure (0.09) increases the risk of being hospitalized or dying from self-harm within 12 months of discharge by 19% for youths at the mean. For youths without histories of substance abuse and self-harm, the estimated peer effects are positive but not statistically significant. These results support the notion that children placed in out-of-home care are exposed to harmful peer effects.

6.4.2 Peer Victimization

In Table D10, I use the same sample and empirical strategy as in Section 6.4.1 to estimate the effect of exposure to peers with a criminal background on assault-related healthcare within 12 months of entering the facility and death by the year the child turns 19. I present separate effects for institutionalized boys and girls since (i) boys are often grouped with other boys and (ii) boys are overrepresented in crimes against other persons. As the focus is on peer victimization, I exclude assaults known to have been perpetrated by someone other than a peer (see Appendix A).

The point estimate in column 1 implies that a 1-standard deviation increase in exposure to criminal peers (0.11) in the same home raises the number of times boys seek healthcare (out- and inpatient care) for injuries caused by assault within 12 months of entering the home with 37% at the mean. The estimates are positive for girls but are only marginally significant in column 3.

Increased exposure to criminal peers raises the risk of being hospitalized for assault within 12 months for both boys and girls. The point estimates in column 4 imply that a 1-standard deviation increase in exposure (boys: 0.11, girls: 0.18) raises the risk of hospitalization by 0.3 percentage points for both boys and girls. Exposure to criminal peers also raises the risk of death by the year the child turns 19, but only for boys. The difference is statistically significant at the 10% level.

Boys who experience a 1-standard deviation higher exposure to criminal peers are 0.5 percentage points more likely to die by the year they turn 19. This effect is driven by an increase in suicides (0.4 percentage point increase from a 1-standard deviation increase).

Table 4. *Effect of Removal on Healthcare for Assault by Month 12*

	No. of Healthcare Visits		Hospitalization	
	(1)	(2)	(3)	(4)
Male*Removed	0.1144*	0.1172	0.0192**	0.0183*
	(0.0695)	(0.0715)	(0.0096)	(0.0095)
Female*Removed	0.0266	0.0208	0.0111	0.0104
	(0.0812)	(0.0796)	(0.0071)	(0.0069)
Dependent mean if male	0.0202	0.0202	0.0020	0.0020
Dependent mean if female	0.0320	0.0320	0.0008	0.0008
Test of equality (p-value)	0.3194	0.2733	0.4529	0.4562
Child & parent controls	No	Yes	No	Yes
N	13044	13044	19085	19085

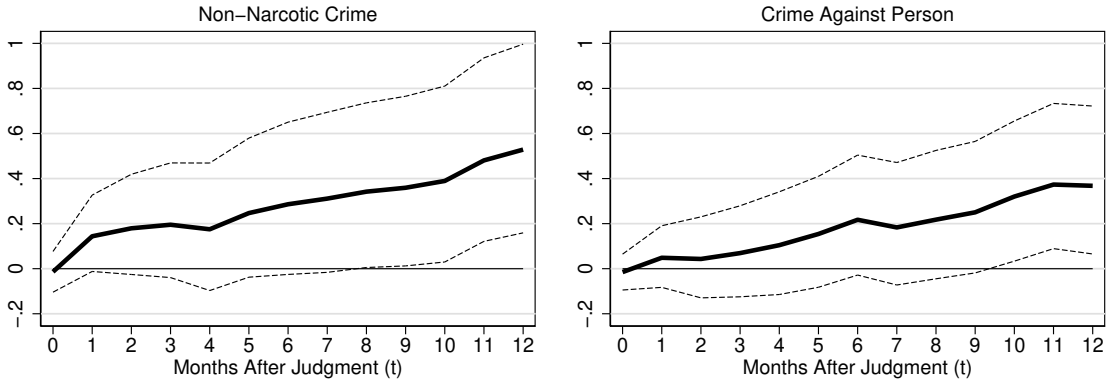
Note: The dependent variable in columns 1-2 is the sum of in- and outpatient care visits due to assault, while the dependent variable in columns 3-4 is an indicator taking the value 1 if the child is ever hospitalized due to assault by month 12. The 'All Ages Sample' is used. In columns 1-2, the sample is smaller since I do not observe outpatient care before 2013. Only IV results are presented. Male*Judge removal tendency and Female*Judge removal tendency are used as instruments. The child and parent characteristics listed in Table 1 are included as controls in columns 2 and 4. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

The results in Table D10 imply that exposing children who are placed in out-of-home care to criminal peers increases their risk of being assaulted and dying. But does *child removal* increase the risk of being assaulted? Columns 1-2 of Table 4 present the IV estimated effects of removal on the number of times the marginal child receives healthcare for injuries sustained through assault within 12 months of the court's judgment. All point estimates are positive but the estimates are only marginally significant for boys. Turning to hospitalizations, columns 3-4 show that child removal increases the risk of the marginal boy being hospitalized for assault within 12 months of the court's judgment by around 1.9 percentage points.

Healthcare data do not include all victimization events since victims might not seek healthcare for their injuries. As a complement, I also present IV estimated effects of removal on non-narcotic crime and crime against other persons over the first year (Figure 7). Since I use the date of the crime, rather than the date of conviction, the steady rise in crime over the year cannot be attributed to crimes committed before removal. If the crime spans several days, I use the first date when determining which month the crime occurred.

Table D11 presents OLS, reduced-form, and IV estimates by month 12. I also present estimated effects on the likelihood of committing a narcotic, minor, and non-minor crime. Child removal increases the risk of the marginal child committing a non-narcotic crime within the first year

Figure 7. *Effect of Removal on Crime*



Note: Black lines show IV estimates of the effect of removal on the cumulative probability of the child committing a non-narcotic crime or crime against persons by month t post-judgment. Dashed lines show 90% AR confidence bands. I limit the '$\geq$10 y.o. Sample' to children who had reached the age of criminal responsibility (15) at the time of the judgment. All specifications condition on being alive and in Sweden during months 0-t.

by 52.9 percentage points (5% significance level). Under the assumption that all 569 non-treated observations (out of 7,046) are compliers, this estimate implies that child removal causes an extra 92 compliers to commit non-narcotic crimes by month 12.[21] The effect on non-narcotic crimes is primarily driven by a large increase in the risk of committing a crime against persons, of which at least 87% are committed while the removed children are still placed in out-of-home care.

The results are again subject to the caveat that there may be under- or over-reporting. The risk of being found guilty might be higher when a child commits a crime while placed in out-of-home care due to increased supervision. On the other hand, prosecutors are encouraged to drop cases against children who are placed in institutions (The Prosecutor-General of Sweden, 2006). Nevertheless, having a criminal record can adversely affect the individual's outcomes (Agan and Starr, 2018). Hence, recorded crime is an important outcome even if there is no change in criminality.

6.5 The Role of Parent Outcomes

Table D12 presents IV estimated effects of child removal on birth parents. In sharp contrast to the results for children, I find little evidence that removal impairs birth parents' health as measured by mortality, hospitalization, and outpatient care. In addition, none of the children of parents who died within 2 years died themselves, and there are no significant effects on crime, marriage rate, or the probability of a parent having positive labor income during the year after child removal.

[21] The share of compliers in the '$\geq$15 y.o. Sample' is estimated to be 10.54%. The number of compliers (non-compliers) who commit crimes within 12 months is estimated to be 230 (1,205).

7 Comparison

The adverse effects that I find are in line with Doyle (2007, 2008, 2013) and Warburton et al. (2014), but contrast with the positive or null findings in Roberts (2018), Bald, Chyn, et al. (2022), Baron and Gross (2022), and Gross and Baron (2022). These studies are all conducted in North America but not in the same state or period. My study is conducted in a European country after 2000. Hence, my findings should be interpreted in light of the high level of child well-being in Europe relative to the US (UNICEF Innocenti, 2020). In particular, children rarely die from injuries in Northern and Western Europe. Depending on the age group, the rates of general and injury-related deaths among children in the US are usually twice as large as the rates in Northern and Western Europe (World Health Organization Mortality Database, 2022).

Part of the explanation can be the generous family policies, affordable healthcare, and extensive social security systems that are common in Northern and Western Europe. For example, Sweden offers a general child allowance, free school meals, lengthy parental leave, compensation for days caring for a sick child, as well as free or heavily subsidized child care, education, and dental, physical, and psychiatric healthcare (Robila, 2014). Residents who fall ill, have a disability, or struggle financially receive economic benefits via Sweden's strong social security system. Families in need are offered extensive services, such as a support family that can care for the child part-time, help with housekeeping, parent training, and treatment programs. If needed, children can be provided free tutoring and tailored education. All in all, the care provided to children who are *not* removed might be particularly good in European countries.

Few European countries grant children in out-of-home care priority access to healthcare or other services (Vinnerljung and Hjern, 2018). In the US, out-of-home placement makes the child eligible for a host of services. The bundle of additional resources varies by state and over time. During the last decades, there have been reforms that further strengthen the support to children in out-of-home care (Dworsky et al., 2013; Palmer et al., 2017). In Michigan, which is the setting studied in Baron and Gross (2022) and Gross and Baron (2022), children who enter out-of-home care are eligible for, e.g., Head Start (an early childhood program), free school meals, Medicaid (a program providing healthcare coverage), and compensation for tuition, education, and training expenses. It is plausible that the estimates reported in the US-based studies capture, to a varying extent, the positive effects of access to services. Since eligibility for support services stays constant in my setting, my estimates do not pick up such effects.

Differences in placement rates may also contribute to the mixed results. However, Sweden's rate of court-ordered and emergency placements was only 2.5 per 1,000 in 2019, compared to 4.9 per 1,000 in the US. Nevertheless, the composition of children might be important: 30% of US children in out-of-home care are under 4, relative to 10% in Sweden. In addition, almost half of

the 'Year 19 Sample' are taken into care because of their behavior, which is rare in the US. The placement composition is also different: a third of children in the US stay with a relative, but only 4% stay with a relative in Sweden. Moreover, placement in non-family homes is common throughout Europe and it is three times as common in Sweden as in the US (Children's Bureau, 2020; NBHW, 2020; Whittaker et al., 2022).

8 Conclusion

This paper studies the effects of the court-ordered placement of children in out-of-home care on mortality, healthcare usage, education, and crime. Causal effects are identified by exploiting the quasi-random assignment of judges together with across-judge variation in the tendency to remove children in an IV framework.

I find that court-ordered removal strongly increases the risk of death by the year the child on the margin of placement turns 19 years old. This effect is primarily driven by suicides that occur while the removed children are still placed in out-of-home care. Critical points in time are the summer months and the transition to adulthood.

The empirical evidence suggests that adverse care conditions and peer exposure are channels through which removal affects child mortality. High barriers to healthcare appear to be especially important. Court-ordered removal *decreases* the number of mental health-related outpatient specialist visits but *increases* the risk of hospitalization for mental illness before the steep rise in suicides at month 9. Court-ordered removal also increases the risk of being hospitalized for injuries sustained through assault, and the risk of committing non-narcotic crimes (especially crimes against other persons) within 12 months of the court's judgment.

The results of this paper do not imply that we should stop intervening when children live in deficient homes or engage in destructive behavior. Rather, the findings should encourage quality improvements (e.g., increased supervision and strengthened collaboration with the healthcare system) and efforts to mitigate harmful effects from peers. The findings should also encourage decision-makers to not only evaluate whether there are legal grounds to remove the child from their home but also whether out-of-home placement is in the child's best interest. In some cases, providing extensive services in the home environment might yield better outcomes, even if the legal grounds for removal are met. Lowering the removal rate of children who suffer from mental illness might be particularly advantageous, but more research on the topic is needed.

A limitation of the paper is that I only study court-ordered placements, which make up 30% of Swedish out-of-home placements. The effects of voluntary placements are potentially different since the children and placements are different. For example, children placed in out-of-home care voluntarily are often unaccompanied minors: 27% compared to 2% in the court-ordered group. In

addition, they tend to be older, placements in institutions are very rare, and they are three times as likely to be placed in a relative's home as a child removed via a court order (NBHW, 2020).

Moreover, I estimate the effect of removal on children who could have received another judgment had a different judge been assigned the case using 'Year 19' and '$\geq$10 y.o.' samples. The restrictions used to construct these samples raise the mean age of the children, which affects other characteristics as well. In addition, some characteristics, such as mental illness, are overrepresented in the complier group (Table B11). All in all, the variation in child and placement characteristics mentioned here and in Section 7 threaten the external validity of the results. At the same time, I find little evidence of effect heterogeneity by child characteristics and placement types. Rather, the quality of the SWC appears to be important. Future studies on the role of individual welfare practices, such as training programs for foster families, are needed to further guide policy.

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Supplementary Data

Supplementary data are available at *Review of Economic Studies* online.

Data Availability Statement

The replication package for this article is available at https://doi.org/10.5281/zenodo.14293584.

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