Surviving Childhood: Health and Crime 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 and crime. 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=26,481) 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. For birth parents, I again find an increase in non-narcotic crimes but there is little evidence of adverse health effects. I explore potential explanations for the detrimental effects on child health. Peer victimization, peer-to-peer spillovers, and adverse care home conditions appear to be important channels.

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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 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).[1,2] At the same time, children with experience of out-of-home care in these countries 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). Out-of-home placed children 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 mortality, suicide, and accidental overdose. To further deepen our understanding, I also examine effects on hospitalization related to mental health and substance use, criminal behavior, 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. To obtain credible estimates, a large, longitudinal, and rich data set at the individual level is needed. To overcome this challenge, I collect and process 21,509 Swedish child protection court files from 2001 to 2019 and extract relevant information with scripts, including the personal identity number of each child.[3] Using these identifiers, Statistics Sweden links the children and their parents to rich registry data, including death, patient, and crime registers. To this data set, I add administrative data on judges from the National Courts Administration.

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

[1] I use "child removal" and "out-of-home placement" interchangeably when referring to the intervention of removing a child from their home and placing them in, e.g., a foster or group home. I focus on cases in which a parent or the child contests removal. I refer to these cases as court-ordered or involuntary placements. While only around 30% of children in Swedish out-of-home care are removed 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. There are two key explanations for the large share of voluntary cases. 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 social welfare committee.

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

[3] Personal identity numbers are unique and given to all residents in Sweden, including foreign-born.

children at the margin of placement, i.e. cases that judges 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 as the mean removal rate in all other cases handled by the same judge, leaving out the focal case.[4]

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 decision-making in the focal case. Second, due to Swedish law, the assignment of child protection 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).[5] 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 mortality, mental health, and substance use. For example, removing a child from an abusive or neglectful home 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). In addition, out-of-home placement might facilitate take-up of health and substance abuse treatment among children and parents (Grimon, 2020), and encourage parents to improve the home environment (Baron and Gross, 2022). Yet another potential channel is exposure to better neighborhoods, which has been shown to impact a range of child outcomes (Chyn and Katz, 2021).

At the same time, being separated from one's family may have long-lasting effects on the child's mental health (Astrup et al., 2017). In addition, maltreatment might worsen in out-of-home care. In an international review, Mazzone et al. (2018) conclude that violent victimization by peers during out-of-home placement is a widespread phenomenon. For example, Allroggen et al. (2017) document that 4.5% of German adolescents placed in care facilities experience severe sexual victimization for the first time while placed in such a facility. 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). Moreover, exposure to peers who abuse substance and self-harm may increase in out-of-home care, which can influence own outcomes (Helénsdotter, 2023).

[4]By leaving out the focal case, I ensure that there is no mechanical relationship between the instrument and decision-making in the focal case. My results are robust to alternative judge instruments, including the use of a binary instrument that takes the value 1 if the judge has an above-average removal tendency.

[5]My results are robust to including fixed effects for the SWC in charge of the case.

Using IV analysis, I find that out-of-home placement has significant adverse effects on the mortality of the marginal child.[6] Removal increases the risk of death by the year the child turns 19 by 7 percentage points (relative to a control complier mean of 1.6%). 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. Using the full sample (aged 0 to 19), positive but imprecisely estimated effects on all-cause mortality are found in the 24-month window post-judgment. The results are robust to alternative specifications and samples.

Heterogeneity analysis does not reveal any statistically significant differences in mortality effects along observable characteristics (gender, age, petition grounds, and foreign background). However, the standard errors are large and I cannot rule out economically meaningful differences in effect size.

I also consider effects on child criminality and hospitalization. In light of the diverging findings for overdose and suicide, I examine outcomes related to substance use separately. Removal significantly increases both the risk of being hospitalized for mental illness and the risk of committing a non-narcotic crime within the first year following the court's judgment. An important driver of the latter is an increase in the risk of the marginal child committing a crime against persons (e.g., violent and sexual crimes). Conditional on being removed, almost all of these crimes are committed *during* placement. The increases in hospitalization and crime appear to precede the rise in suicides.

In line with the non-significant effect on overdose during the first two years following the judgment, there is no evidence of an increase in substance use-related hospitalization or narcotic crime in the first year.

Child removal also increases the risk of any birth parent committing a non-narcotic crime and, particularly, a crime against persons. For narcotic crimes, the estimates are not statistically significant. There is little evidence of adverse effects on parental health, and there is no overlap in parent and child deaths during the 24 months post-judgment. There are no statistically significant changes in marriage rates or the probability of having positive labor market earnings during the following calendar year. All in all, effects on birth parents (except, potentially, criminality) appear to be unlikely mediators of the adverse effects on child mortality.

Why do I find such adverse effects on child mortality? First, prior empirical evidence suggests that individuals with a large stock of suicide risk factors (e.g., presence of mental disorders and

[6] I also compute the average treatment effect on all, treated, and untreated children as weighted averages of marginal treatment effects (MTEs). However, the weighted averages should be interpreted with caution as I do not have full common support.

history of adverse childhood experiences) are particularly sensitive to psychosocial stressors (e.g., change and separation), which can trigger an acute risk of suicide (Carballo et al., 2020). Hence, we may expect greater responsiveness to new stressors among children at risk of removal.

Court-ordered child removal may lead to further accumulation of risk factors and exposure to stressors through, for example, family separation and disruption of the child's social and physical environment. To shed some light on this channel, I investigate heterogeneity in effects by the probability of (i) experiencing placement instability and (ii) having to move to another municipality. However, I find little evidence of effect heterogeneity. In contrast, I find suggestive evidence in support of peer victimization, peer-to-peer spillovers, and adverse care home conditions being potentially important channels through which out-of-home placement affects mortality. A critical point appears to be the transition to adulthood: over 20% of the deaths occur during the 2 months after the removed child turns 18 and is legally considered an adult.[7] These deaths cannot be explained by the child aging out of care since the children who died would have aged out of care at 21. I find little support for poor post-placement conditions or the stress of placement exit being major drivers of the adverse mortality effects.

My paper contributes to the literature on the effects of child protection interventions (for a review, see Bald, Doyle, et al., 2022).[8] In Appendix G, 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 health care usage (Drange et al., 2022; Gram Cavalca et al., 2022). By using plausibly exogenous variation in removals to study the effects on overall mortality, suicide, and overdose, I can extend our knowledge on the health effects of child removal. Thereby, 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),

[7] When turning 18, the individual is given a host of rights and responsibilities, which can be both stressful and lead to destructive behaviors. At the same time, the young adult is no longer eligible for certain services and can no longer receive care via the child and adolescence health care system.

[8] 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.

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, health care, schooling, and social security systems (Gilbert et al., 2011), it is imperative to gain knowledge about the effects of child removal in Europe.[9]

I also contribute to our knowledge on family effects of child removal by considering novel parent outcomes (mortality, self-harm, substance use, marriage, 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).

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.[10] 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.[11] These studies 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 age group, welfare practices, and population characteristics.[12]

[9] A key difference between the child protection systems in Europe versus the US is that placement in out-of-home care is rarely coupled with eligibility to other potentially welfare-improving programs (e.g., Medicaid and Head Start) in Europe. I elaborate on differences in institutional features in Section 2.2 and Appendix F.

[10] 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, Kostøl, et al., 2019; Collinson et al., 2022).

[11] The margins studied using the judge versus CPS worker instrument 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.

[12] Another potential reason is differences pertaining to the instrument and the underlying assumptions. 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

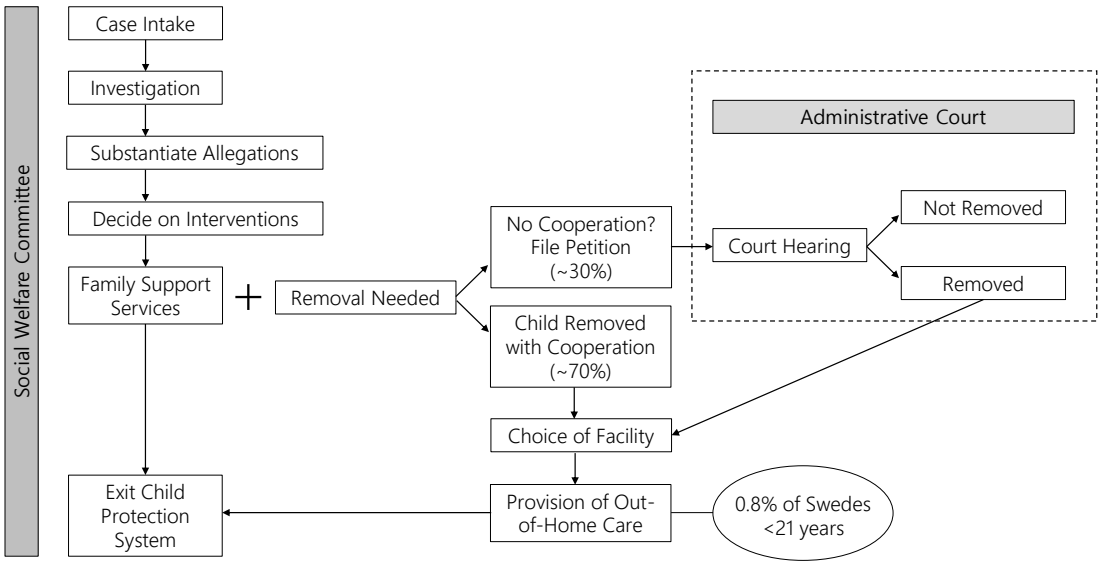
In Appendix F, I elaborate on how the European setting differs from the settings considered in prior studies.

The paper proceeds as follows. Section 2 presents the institutional background and a cross-country comparison. Section 3 describes the data. Section 4 outlines the IV model and discusses the validity of the assumptions. Effects on child mortality are presented in Section 5 while effects on other short-term outcomes are presented in Section 6. Section 7 probes possible mechanisms. Section 8 concludes.

2 Institutional Background

2.1 Child Protection System in Sweden

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, and determines whether the allegations that prompted the investigation are substantiated. The SWC then 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 then 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 (or ages out of) the child protection system.

the family, which issues must be resolved in the family before reunification, or whether the police should be contacted, the worker may affect child outcomes through channels other than the removal decision. While a combined, reduced-form effect can be estimated — which is a policy-relevant effect as well — the exclusion restriction needed to isolate the effect of removal can be challenging to meet. The extent and character of this issue potentially varies between study settings due to local variation in social welfare practices. With the judge instrument, I can avoid this issue since (in my context) the judge only decides whether the child should be removed and has very limited contact with the family. All other decisions are made by the caseworker at the Swedish child protection authorities.

Figure 1 provides a representation of the child protection process in Sweden. The local SWC (*socialnämnden*) is responsible for child protection.[13] This responsibility is broad and encompasses, e.g., preventive work, maltreatment investigations, evaluation of service need, and service provision. However, the SWC does not have the authority to take children into care without the consent of the caregivers and the child.[14] When no consent can be attained, the SWC files a petition with one of Sweden's 12 administrative courts.[15]

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

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.[16] The case is then automatically assigned to a department within the court according to a rotating system.[17] 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.[18] Fortunately, junior judges only make up

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

[14]A key difference between children who are removed with versus without consent is the higher share of unaccompanied minors in the voluntary group: 27% compared to 2% in the involuntary group. In addition, the share of individuals above the age of 18 is higher in the voluntary group (38% compared to 9%) and almost no individuals in voluntary care are placed in institutions (compared to 14% of children in involuntary care; NBHW, 2020). For more descriptive statistics, see Table A1.

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

[16]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.

[17]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. The results are robust to the use of department-by-year FEs.

[18]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.

3% of my analysis samples and the results are robust to excluding these judges and cases that are typically not assigned to junior judges.[19]

Upon receiving the petition, the court must offer family members lawyers and hold an oral hearing within 2 weeks. The date of the hearing is decided by the court administrator 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 of the hearing 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 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 in child protection cases (or any other case group).[20]

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).[21]

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.[22,23] 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

[19]The first-stage estimate and balance test are robust to excluding junior judges and non-junior cases from the analysis samples and the samples used for instrument construction.

[20]The SWC can change their claims at any point before or during the hearing. I use the initial petition (i.e. before judge assignment) to construct background variables such as petition grounds.

[21]Contact between judges, SWC workers, and lawyers is very restricted to ensure that there is no bias.

[22]Some decisions made 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 with the concerned parties, with one exception: termination cases. If a caregiver or child requests termination of care and the SWC denies the request, the caregiver/child can appeal that decision, but such an appeal will only be quasi-randomly assigned to the judge pool leaving out the judge who ordered out-of-home care in the first place.

[23]The SWC takes about 80% of children into emergency care. The SWC must then inform the court within one week and submit a petition for removal within four weeks. 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, usually because of administrative errors made by the SWC (Table A2).

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.[24]

If the court rules in favor of child removal, the SWC decides where the child should be placed. Children removed via court order can be placed together with children who receive care voluntarily. The most common placement option is foster home, followed by group home and institution (Table A2). The former placement type implies living in the private home of a family. Foster families may have children of their own living in the same house.

Group homes and institutions are primarily used for older children with behavioral problems. In such facilities, multiple children live together while supervised by staff. Group homes are often privately owned and vary in size, orientation, treatment portfolio, target group, and staff education. For example, some group homes are located in urban settings and have on-site schools while others are located on farms with horses and other animals. 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 an official group home.

Institutions are secure facilities managed by The National Board of Institutional Care and are akin to juvenile detention centers. Indeed, 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.[25] Staff at institutions has the authority to take coercive measures such as body searches, communication restrictions, solitary confinement, and isolation.

Irrespective of placement type, parents are usually encouraged to have contact with their children while they are placed in out-of-home care and the goal is family reunification.[26] 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 A2, the average placement length following court-ordered removal is 25 months. Figure A1 displays the share of children still placed in out-of-home care t months after being removed from home. After four years, around 10% of children who are taken into care at age 16-19 are still in care, compared to around 50% (70%) of children aged 11-15 (0-10).[27]

[24] 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 rejected, 13.3% are part of a future petition and most (85.4%) are removed in the second case. On average, the time between the first and second petition is almost 2 years.

[25] Youths sentenced to serve time in an institution for committing a serious offense are not part of the analysis samples as they enter care through the criminal, rather than the administrative, court system.

[26] 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 A1).

[27] These calculations are based on a register known to be subject to underreporting (see Section 3.1).

2.2 Cross-Country Comparison of Child Welfare Systems

In terms of child well-being in the general population, Sweden ranks well compared to other OECD countries. In contrast, the US (which is the country in which most credible studies on child removal have been conducted) is found in the bottom tertile (UNICEF Innocenti, 2020). Part of the explanation for Sweden's high level of child well-being can be Sweden's generous family policies, affordable health care, and extensive social security system (Gilbert et al., 2011). In terms of child mortality, the rate of death per 100,000 in Sweden is similar to other Western countries. The US, on the other hand, is an outlier with far higher child death rates (World Health Organization Mortality Database, 2022). During the years 2001-2022, the average rate of death among children (age 0-19) was 27 per 100,000 in Sweden. Among Swedish adolescents (age 10-19), around 4 per 100,000 died each year from suicide during the same period (NBHW, 2023).

For children in need of protection, Sweden is regarded as having a quite strong child protection system in terms of the practices employed (FRA, 2015). Sweden's rate of (voluntary and involuntary) placement has been low relative to other Western countries during the last two decades (Gilbert, 2012). However, it is difficult to compare rates across countries due to differences in reporting. For example, in some countries (including Sweden) voluntary placements in the homes of relatives and private residential facilities are included in the official statistics. In 2019, the total rate of placement (including voluntary placements) was 8.2 per 1,000 Swedes under age 21 while the rate of court-ordered and emergency placements was only 2.5. In the US, official statistics almost exclusively cover court-ordered and emergency placements. Hence, the placement rate in the US of 4.9 per 1,000 should be compared with the rate of 2.5 in Sweden.[28]

The age composition of children in out-of-home care is different in the US: among children in out-of-home care on September 30, 2019, 30% were under the age of 4. In contrast, just 10% of children placed via court order or emergency removal were under the age of 4 in Sweden on November 1, 2019. Moreover, while foster care is the main placement form in both countries, the share of foster placements is larger in the US: 79% compared to 59%. See Table A1 and Appendix F for more comparative statistics and institutional details.

3 Data

3.1 Data Description

The primary data source is child protection judgments that I collect from Swedish courts, The Swedish National Archives, and Stockholm City Archive. I transcribe these judgments using a

[28] Own calculations based on statistics from U.S. Census Bureau, Population Division (2020), Children's Bureau (2020), Statistics Sweden (2019), and NBHW (2020).

mix of automated and manual techniques, and manually verify that each document is accurately transcribed. I extract 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 classify 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 G 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 eight 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 results are robust to excluding years with non-universal coverage. The full court sample consists of 26,481 child-by-case observations spanning 2001 to 2019.

I add 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 is sufficient to uniquely identify each judge except for two pairs of judges. For these pairs, I combine full name with courthouse or employment period to uniquely identify the judge. For 99.3% of the sample, I can match the deciding judge with a judge in the employment records.

I have accurate personal identity numbers on 94.0% of the sample.[29] Using these identifiers, Statistics Sweden matches the children to their parents. From Statistics Sweden, I receive data on, e.g., gender, birth date, immigration/emigration dates, foreign background, labor income, and marital status of both children and parents.

Information on all deaths (date and cause) comes from the National Cause of Death Register (1997-2022) kept by the National Board of Health and Welfare (NBHW). I also obtain data on all hospitalizations at Swedish hospitals (private and public) related to mental health and substance use from the National In-Patient Register (1997-2020). When exploring mechanisms, I make use of placement data from the Register on Service Provision to Children and Young Persons (2000-2020). This register is supposed to include all 24-hour care interventions provided to people under the age of 21 but it suffers from underreporting.[30]

Moreover, I obtain data on all institutional placements from the National Board of Institutional Care (2000-2021) and all legal proceedings (date of crime, date of decision, and section of the

[29] Missing accurate personal identity number is almost always due to (i) not yet having been assigned one because of recent first-time immigration or birth or (ii) protected identity.

[30] Before 2014, all municipalities reported information on 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 my main analysis.

law) from the National Council for Crime Prevention (1997-2021).[31] See Appendix G 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 focal case c as the total number of children judge j removes minus the number of children judge j removes in the focal case divided by the total number of children processed by judge j minus the number of children in the focal 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 focal 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 children 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 children in the same case. By excluding all decisions made in the focal case, I rid the measure of a mechanical relationship between removal tendency and decisions in the focal 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 20,473 observations.[32] The results are robust to changes in instrument construction, including the use of a higher cutoff for the number of cases per judge.

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. Table G1 presents an overview.

First, I drop children that I cannot observe in Statistics Sweden's register data (N=1,576). I also drop cases with missing information on judge removal tendency (N=5,689) and cases in court-by-year cells containing only one active judge (N=80). The final sample (N=19,136) consists of 15,364 unique cases (18,037 unique kids) assigned to one of 249 judges. I use this sample to study

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

[32] 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 0.945 (std. err.: 0.012, p-value$<$0.001).

all-cause mortality in the months following the court's judgment and refer to it as the 'All Ages Sample'.[33]

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 my mortality data (year 2022) whose cases are decided before the year they turn 19. The sample (N=10,200) 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 hospitalized due to self-harm or substance use within the first year was 11 at the time of the judgment. Hence, I limit the 'All Ages Sample' to children who were at least 11 years old.[34] This sample (N=11,205) is referred to as the '$\geq$11 y.o. Sample'.

Table 1 displays descriptive statistics at the child and birth parent level (Panel A) and judge level (Panel B) for each analysis sample.[35] 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 very similar. However, the judge characteristics deviate. 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. However, the average judge removal tendency is unaffected. In fact, judge removal tendency (0.89) is similar across all samples in Table 1, which is the first piece of evidence supporting random assignment.[36,37]

Child and parent characteristics vary between the analysis samples (columns 2-4). Compared to the 'All Ages Sample', the mean age at the time of judgment is higher in the more restrictive samples. As can be expected among an older group of children, the child's own behavior is more likely to be stated as grounds for removal on the SWC's petition, there is a lower share of cases involving siblings, it is more common that parents consent to removal, and there is a higher share of children with histories of crime and mental illness. Naturally, since there are few or no children aged below four in the 'Year 19 Sample' and '$\geq$11 y.o. Sample', the share with missing

[33]Results are robust to only using the first case for each child.

[34]The youngest child to die from suicide (overdose) within the first year was 13 (16) at the time of the judgment.

[35]Descriptive statistics are almost identical when taking into account attrition (Table B2).

[36]The average judge removal tendency is not comparable with the average tendency reported in studies using the decisions of child protection caseworkers (e.g., Doyle, 2007) because, in the current 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).

[37]The share of female judges is somewhat 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 my data).

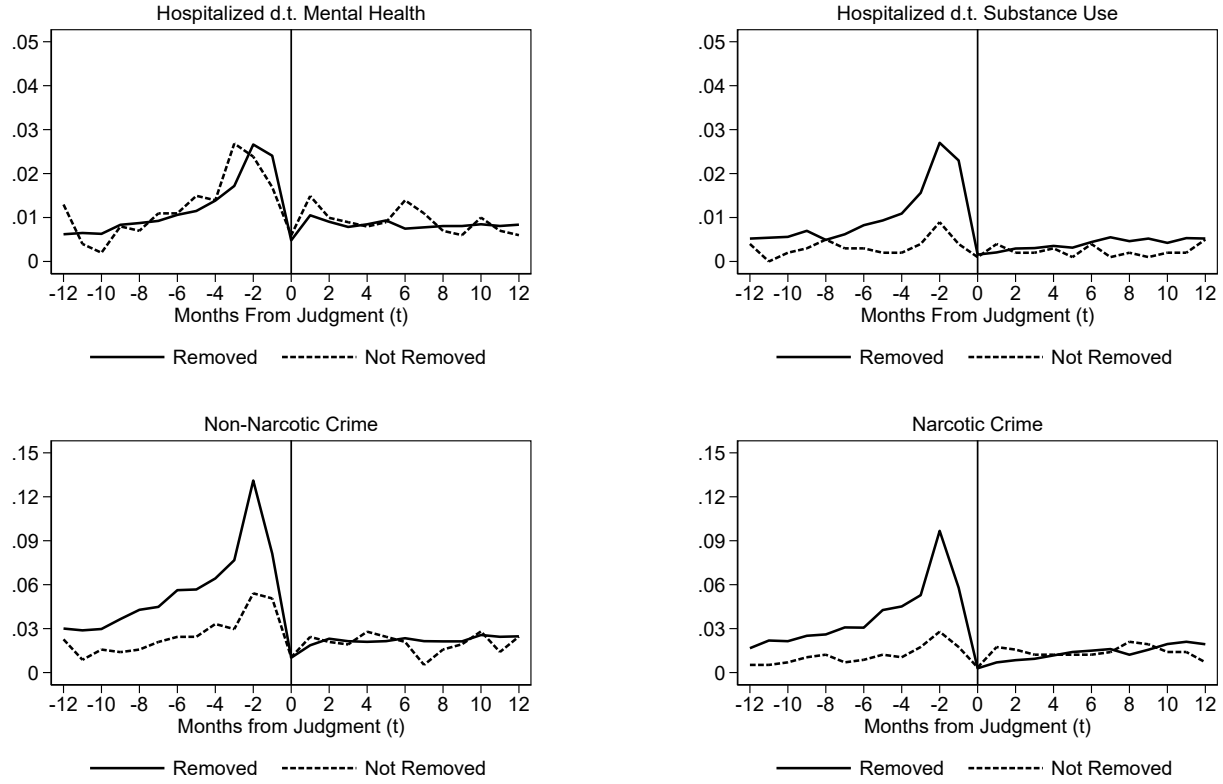
Table 1. *Descriptive Statistics*

	All in Registry	All Ages Sample	Year 19 Sample	≥11 y.o. Sample
A: Child & Parent Characteristics				
Removed	0.89	0.88	0.90	0.91
Girl	0.46	0.47	0.46	0.46
Age at judgment	10.83	10.75	14.49	15.05
Sibling case	0.32	0.33	0.17	0.15
Foreign background	0.38	0.38	0.42	0.42
Behavior petition	0.29	0.28	0.44	0.47
Environment petition	0.61	0.62	0.39	0.35
Double grounds petition	0.10	0.10	0.17	0.17
Child consents to removal	0.57	0.65	0.44	0.48
At least 1 parent consents to removal	0.32	0.36	0.52	0.48
Case largely based on child mental health	0.04	0.04	0.06	0.07
Non-junior case type	0.17	0.17	0.09	0.08
Committed (yrs t-1 to t-3):				
Crime against person	0.09	0.09	0.12	0.13
Narcotic crime	0.09	0.10	0.11	0.14
Other crime	0.11	0.11	0.14	0.16
Hospitalized (yrs t-1 to t-3) due to:				
Mental health	0.06	0.06	0.08	0.09
Substance use	0.05	0.05	0.06	0.07
Missing, yrs t-1 to t-3	0.23	0.24	0.11	0.11
Any birth parent:				
Dead	0.05	0.05	0.06	0.06
<18 y.o. at birth of child	0.02	0.02	0.02	0.03
Married, yr t-1	0.42	0.45	0.49	0.49
No labor income, yr t-1	0.58	0.63	0.56	0.55
Hosp. d.t. mental health, yr t-1	0.07	0.07	0.06	0.05
Hosp. d.t. substance use, yr t-1	0.05	0.05	0.04	0.04
Any crime, yr t-1	0.15	0.16	0.11	0.11
Missing Xs, yr t-1	0.24	0.24	0.28	0.28
B: Judge Characteristics				
Judge removal tendency	0.89	0.89	0.88	0.89
Junior judge	0.15	0.03	0.03	0.03
Female judge	0.53	0.50	0.47	0.49
Judge age	49.77	52.56	52.66	52.50
Unique judges	843	249	249	249
Unique cases	20124	15364	9438	10546
Unique children	23097	18037	9591	10559
Unique birth parents	31542	24853	15323	17036
Observations	24905	19136	10200	11205

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

information in the years t-1 to t-3 is much smaller in these samples.

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.

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.[38] 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 (i.e. there is selection into 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. 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 month, which might be due to incapacitation effects from

[38]The date of the crime, rather than the date of conviction or reporting, is used for crime outcomes. Children can be sentenced to placement in out-of-home care by a district court if they commit a crime punishable by prison. Such placements are not included in this paper.

emergency out-of-home placement or deterrence effects in light of the risk of future removal. After the judgment month, 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.

4 Empirical Methodology

4.1 Instrumental Variable Model

The aim is to estimate the causal effect of removal on child health 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:

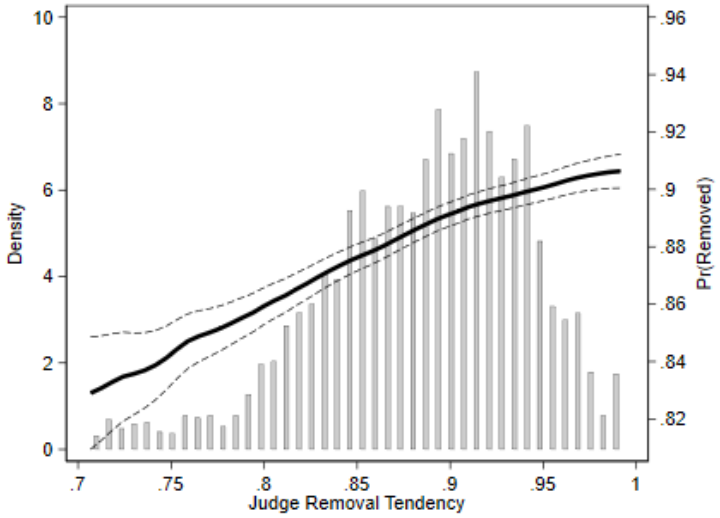
$$R_{i,c,t} = \pi Z_{j(c)} + \alpha_{h,t} + \epsilon_{i,c,t}, \tag{3}$$

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. I demonstrate robustness to the use of other fixed effects: department-by-year FEs as well as court-by-year FEs together with day-of-week FEs and SWC FEs.

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., 2023). I show robustness to alternative levels of clustering.

By using an IV design, I can estimate the local average treatment effect (LATE), i.e. the effect

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



Note: This figure depicts the first-stage relationship between removal in the focal case 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.

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 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, removal tendency 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 2. 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 analysis sample and whether extra controls are added, the estimated coef-

ficient is large, positive, and highly significant with an effective F-statistic around 50-70.[39] The point estimate varies somewhat between the analysis samples, which is unsurprising given that the characteristics of the samples differ. The point estimate of around 0.4 in the 'All Ages Sample' implies that being randomly assigned a judge with a 10 percentage point higher removal rate increases the probability of being removed from home by roughly 4 percentage points.[40]

Table 2. *First-Stage Estimates of Removal on Judge Removal Tendency*

	(1) All Ages Sample	(2) Year 19 Sample	(3) ≥11 y.o. Sample
A: Court-by-Year FEs			
Judge removal tendency	0.4237***	0.4422***	0.3887***
	(0.0550)	(0.0609)	(0.0552)
Effective F-statistic	60.57	53.46	49.70
B: Add Child & Parent Controls			
Judge removal tendency	0.4205***	0.4340***	0.3787***
	(0.0507)	(0.0581)	(0.0521)
Effective F-statistic	70.34	56.40	52.97
Dependent mean	0.88	0.90	0.91
N	19136	10200	11205

Note: In Panel A, estimations include court-by-year FEs. In Panel B, the child and parent characteristics listed in Table 1 are added. I report Olea and Pflueger (2013)'s effective F-statistic. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

In Tables C4-C5, 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 3 provides strong empirical evidence that judges are randomly assigned, conditional on court-by-year FEs. The first column regresses removal on 25 background variables. Important predictors of removal

[39] I obtain similar first-stage results using a probit model.

[40] 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. 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.

Table 3. *Test of Random Assignment of Judge Removal Tendency*

	Removed		Judge Removal Tendency	
	Coeff	Std err	Coeff	Std err
Girl	-0.0043	0.0048	0.0011	0.0009
Age at judgment	0.0034***	0.0008	-0.0001	0.0002
Sibling case	-0.0292***	0.0082	0.0000	0.0016
Foreign background	0.0304***	0.0066	0.0008	0.0014
Behavior petition	0.0205***	0.0076	0.0019	0.0017
Environment petition	-0.0982***	0.0095	-0.0012	0.0019
Child consents to removal	0.2454***	0.0096	-0.0002	0.0015
At least 1 parent consents to removal	0.0658***	0.0065	-0.0004	0.0014
Missing consent data	0.1445***	0.0221	0.0032	0.0043
Case largely based on child mental health	-0.0432***	0.0154	-0.0004	0.0027
Non-junior case type	-0.0069	0.0079	0.0011	0.0015
Committed (yrs t-1 to t-3):				
Crime against person	0.0140*	0.0079	0.0002	0.0020
Narcotic crime	0.0491***	0.0072	0.0009	0.0019
Other crime	0.0086	0.0076	-0.0012	0.0018
Hospitalized (yrs t-1 to t-3) due to:				
Mental health	0.0015	0.0097	0.0015	0.0021
Substance use	0.0080	0.0093	-0.0016	0.0024
Missing, yrs t-1 to t-3	0.0238***	0.0077	0.0011	0.0016
Any birth parent:				
Dead	0.0294**	0.0125	0.0021	0.0025
<18 y.o. at birth of child	-0.0143	0.0185	-0.0002	0.0037
Married, yr t-1	0.0096	0.0068	-0.0004	0.0014
No labor income, yr t-1	0.0023	0.0068	-0.0004	0.0014
Hosp. d.t. mental health, yr t-1	0.0158	0.0128	-0.0031	0.0026
Hosp. d.t. substance use, yr t-1	0.0044	0.0144	0.0028	0.0027
Any crime, yr t-1	0.0272***	0.0090	-0.0000	0.0017
Missing Xs, yr t-1	0.0004	0.0094	-0.0009	0.0018
F-statistic	38.98		0.50	
p-value	0.00		0.98	
N	19136		19136	

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. * $p < .1$. ** $p < .05$. *** $p < .01$.

are, e.g., petition grounds, whether the case is largely based on concerns for the child's mental health, foreign background, whether the child or any parent consents to removal, and the criminal history of the child and parents. 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, and are not jointly significant (*F*-statistic: 0.50). 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 Tables C2-C3. I vary the sample, specification, and instrument used when performing the randomization test. I also test for random assignment using other judge characteristics (judge gender, age, and junior position) in Table C1. 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. In criminal cases, the judge must typically decide on guilt and a host of possible sanctions. This multifaceted nature of judgments in criminal cases poses a threat to the exclusion restriction (see, e.g., Bhuller et al., 2020). 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 formal test of the exclusion restriction, joint with random assignment and the strong monotonicity condition (see Section 4.5) is provided by Frandsen et al. (2023).[41] I apply the test for the main outcomes as well as hospitalization and crime outcomes while varying the settings (Table C6). In line with the validity of the three assumptions, I cannot reject the null hypothesis for any of the main outcomes.[42]

In Table C7, 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. First, I regress judge removal tendency on case processing time, whether the SWC decided to place the child in emergency care before the court hearing, and an indicator for the court rejecting the emergency care decision. Second, I use the subset of removed children and regress judge removal tendency on various placement characteristics (placement type, length of stay, placement switches, across-municipality moves, and within-country moves). In line with the exclusion restriction, the estimated coefficients are close to zero and lack statistical significance (F-statistic for joint significance: 0.53-0.87).

[41] Frandsen et al. (2023)'s test essentially tests an implication of the three assumptions: outcomes averaged at the judge level should fit a continuous function with bounded slope of judge treatment propensity.

[42] For hospitalization and crime outcomes, the test rejects the null only when few knots are used.

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. In this setting, 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 very strong assumption and its validity in empirical settings has been questioned in recent papers (Mogstad et al., 2021; Norris et al., 2021; Chan et al., 2022; Frandsen et al., 2023; Sigstad, 2023). As I note 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 individual treatment effects (Frandsen et al., 2023). 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 overall removal tendency than judges who decide to leave the child at home. However, as clarified in Sigstad (2023), while weak monotonicity is sufficient to identify some proper weighted average, it does not ensure 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. Hence, whether the weak monotonicity assumption is credible can be investigated by slicing the sample along observable dimensions and rerunning the first stage for each subsample. Table C8 presents such estimates when I split the sample by petitions grounds, age, foreign background, and gender. In each subsample, the estimates are large, positive, and significant. I also rerun the first stage using an alternative definition of removal tendency: the judge's tendency to remove children *outside* the subsample. Again, the estimates are large, positive, and significant in each subsample (Panel B). 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), which further supports the validity of the monotonicity assumption.

5 Results for Child Mortality

5.1 Baseline Results

Table 4 presents the estimated effects of court-ordered removal on all-cause and cause-specific mortality measured by the year the child turns 19 or by month 24 following the court's judgment. Compared to Table 1, the sample sizes are slightly smaller because of sample attrition stemming from emigration.[43]

[43]See Appendix B 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 B1).

Table 4. *Effect of Removal on Child Mortality*

	Death by Year Child Turns 19			Death by Month 24 Post-Judgment		
	(1) All-Cause	(2) Suicide	(3) Overdose	(4) All-Cause	(5) Suicide	(6) Overdose
OLS (No Controls)						
Removed	-0.0009	-0.0027	0.0009	-0.0000	-0.0003	0.0014***
	(0.0029)	(0.0023)	(0.0011)	(0.0013)	(0.0015)	(0.0004)
OLS (With Full Set of Controls)						
Removed	-0.0035	-0.0045*	0.0002	-0.0009	-0.0019	0.0011**
	(0.0031)	(0.0024)	(0.0011)	(0.0014)	(0.0017)	(0.0005)
OLS (Complier Reweighted)						
Removed	-0.0042	-0.0043*	-0.0004	-0.0002	-0.0009	0.0010*
	(0.0034)	(0.0025)	(0.0014)	(0.0013)	(0.0010)	(0.0005)
RF (Only Court-by-Year FEs)						
Judge removal tendency	0.0321**	0.0156**	0.0134	0.0066	0.0149***	-0.0067
	(0.0131)	(0.0073)	(0.0081)	(0.0055)	(0.0054)	(0.0058)
IV (Only Court-by-Year FEs)						
Removed	0.0719**	0.0350**	0.0299	0.0154	0.0383**	-0.0173
	(0.0312)	(0.0173)	(0.0187)	(0.0131)	(0.0150)	(0.0150)
IV (With Full Set of Controls)						
Removed	0.0721**	0.0337*	0.0301	0.0144	0.0383**	-0.0184
	(0.0316)	(0.0174)	(0.0191)	(0.0132)	(0.0152)	(0.0154)
Sample	Year 19	Year 19	Year 19	All Ages	≥11 y.o.	≥11 y.o.
AR p-value	0.0157	0.0427	0.1050	0.2674	0.0065	0.2263
AR confidence set (95%)	[.016,.141]	[.001,.072]	[-.005,.07]	[-.011,.041]	[.011,.073]	[-.051,.012]
Dependent mean	0.0071	0.0026	0.0018	0.0031	0.0017	0.0013
Complier mean if not removed	0.0156	0.0023	0.0083	0.0006	0.0000	0.0000
N	10168	10168	10168	19089	11189	11189

Note: Columns 1-2, 3, and 4-5 use the 'Year 19 Sample', 'All Ages Sample', and '≥11 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. * $p < .1$. ** $p < .05$. *** $p < .01$.

As shown in the last column of Table 4, naive OLS analysis reveals that the risk of overdose by month 24 is 0.14 percentage points higher among removed children (conditional on being at least 11 years old at the time of the judgment). This result is unsurprising since drug and alcohol addiction is grounds for removal. Hence, the removed group likely has a higher underlying risk of overdose.

When controls for child and parent characteristics are added, the point estimate is reduced. As I cannot observe and control for all variables that influence the removal decision and the risk of overdose (e.g., addiction severity), the OLS results are still likely plagued by (positive) OV bias.

Nevertheless, I conduct an exercise in which removed attriters are assigned the best outcome (e.g., survival by month 24) and non-removed attriters are assigned the worst outcome (e.g., death by month 24). The results are essentially the same (Table B3).

When using IV analysis (which addresses the issue of OV bias), the estimate is reduced to the point that it even switches signs. However, due to large standard errors, the IV estimate is 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 A3). 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.[44]

In contrast to the effect on overdoses by month 24, the IV estimated effect on overdose by the year the child turns 19 (column 3) is positive but still imprecisely estimated.

While addiction is both a major predictor of overdose and a legal ground for removal, the main predictor of suicide, mental illness (Beautrais, 2000; Bostwick et al., 2016), is not a legal ground. Nevertheless, some SWC workers attempt to protect children at risk of suicide by trying to place them in 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 assigned judge (Table A3). Hence, it is plausible that the counterfactual suicide rate is higher among non-removed children. In turn, suicides make up over one-third of all-cause deaths. This implies that a selection of children with a high risk of suicide into the control group would also bias the OLS estimates for all-cause mortality downward, which may explain why naive OLS analysis reveals slightly negative estimates for mortality outcomes that include suicides (columns 1-2, 4-5).

When adding observable controls and reweighting the sample using complier weights, the estimates barely change. However, as for overdoses, 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 reveals very different results compared to OLS. As shown in the first column of Table 4, removal increases the risk of the marginal child dying by the year they turn 19 by over 7 percentage points (significant at the 5%

[44]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 LASSO to obtain a measure of the risk of removal based on court-by-year dummies and the child and parent characteristics listed in Table 1. 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.

level). This holds both with and without child and parent controls. In relation to the mean of 1.6% among compliers if not removed, this increase is striking.[45] The effect is primarily driven by suicides. The IV estimate in column 2 implies that removal increases the risk of suicide by year 19 by over 3 percentage points (significant at the 5-10% level). I also report the Anderson-Rubin (AR) test and identification-robust confidence sets as recommended by Andrews et al. (2019). Even the lower bounds of the AR confidence sets imply large increases in mortality.[46]

Such large effects suggest caution in interpretation. Recall that the effects are estimated for cases that judges disagree about, which only make up around 14% of the analysis samples.[47] 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 legal guidance and consensus on involuntary placement of such children.[48] 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 A3).

In addition, the instrument typically only takes on values between 0.7 and 1 (see Figure 3) and the first-stage coefficient is around 0.4.[49] However, the IV estimate extrapolates the induced

[45]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 (353) 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 the general Swedish population during the years 2001-2020 (NBHW, 2023).

[46]In Appendix F, I discuss reasons for why my findings contrast with recent findings reported in studies conducted in the US.

[47]Consider the effect on all-cause mortality by the year the child turns 19. The risk of death, P(Y), can be decomposed: P(Y)=P(Y|NC)*P(NC)+P(Y|C)*P(C), where C defines complier and NC defines non-complier. In turn, the risk of death among compliers can be decomposed: P(Y|C)=P(Y|C,NT)*(1-P(T|C))+P(Y|C,T)*P(T|C), where T defines treated and NT defines control. Using that P(Y|C,NT)=0.0156 and P(Y|C, T)=0.0875, we get P(Y|C)=0.0156+0.0719P(T|C). I estimate that the share of compliers is around 13.55%, while the mean risk of death is 0.71%. Hence, 0.0071=P(Y|NC)*(1-0.1355)+(0.0156+0.0719P(T|C))*0.1355. Suppose 20% of compliers are removed from home. If so, the probability of death among non-compliers (always- and never-takers) must be around 0.35%. In total, 72 children die by the year they turn 19, of which 44 die from suicide or accidental overdose. Under the assumption that 20% of compliers are removed, there are (0.0156*0.8+0.0875*0.2)*0.1356*10168≈41 deaths among compliers and 31 deaths among non-compliers. According to my point estimates, only 0.0156*0.1356*10168≈22 compliers would die if none of the compliers are removed from home and child removal causes an extra 19 deaths. Using instead the lower end of the AR confidence set (0.016) yields (under the same assumption that 20% of compliers are removed) 26 deaths among compliers, of which only 4 deaths are attributable to child removal. If 80% of compliers are removed, there are 39 deaths among compliers and 17 of these deaths are attributable to child removal.

[48]The legal mandate to place children with mental health problems 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 illness can be removed if they engage in socially destructive behavior provided that the behavior is not a *symptom* of the child's underlying 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). Even if a child engages in socially destructive behavior that would warrant removal, the best treatment given the child's needs might be offered in the home environment by various specialists and health care professionals.

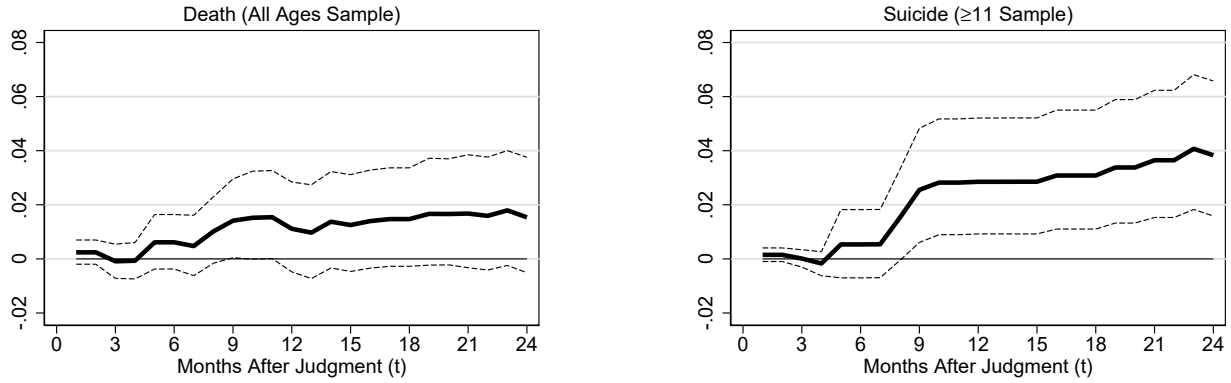
[49]The range of variation in the instrument in Aizer and Doyle (2015) is 12 percentage points, while it is around 25

change in the likelihood of removal to a binary change in removal from 0 to 1, which can result in large point estimates and standard errors.

Table 4 also provides reduced-form (RF) estimates.[50] The relationship between (actual and predicted) child mortality and judge removal tendency is further explored in Figure D1. In line with conditional randomization, predicted child mortality (using child and parent background characteristics) appears unrelated to the instrument. In contrast, actual mortality by the year the child turns 19 increases approximately linearly with the instrument.

Turning to all-cause mortality by month 24 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 at conventional levels (Table 4, Column 4). Instead, limiting the sample to children who are at least 11 years old at the time of the judgment reveals a significant increase (5% level) in suicides by month 24.[51]

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



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.

Figure 4 graphically presents IV estimates of the effects of child removal on cumulative all-cause mortality and cumulative risk of suicide by month t after the court's judgment (with 90% AR confidence intervals). The point estimates 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 '≥11 y.o. Sample', the estimates become significant (5% level) already by month 9 and remain steady for the subsequent months.

percentage points in Bhuller et al. (2020).

[50] A probit model yields similar reduced-form estimates.

[51] In the 'All Ages Sample', 60 children die by month 24, while 19 (14) children die from suicide (overdose) in the '≥ 11 y.o. Sample' by month 24.

5.2 Heterogeneity

5.2.1 By Observable Characteristics

In light of prior research documenting that boys are particularly responsive to childhood conditions (Bertrand and Pan, 2013; Autor, Figlio, et al., 2019), I first split the sample by gender. Responsiveness (as well as needs, care home conditions, and treatment length) may also vary by petition grounds, foreign background, and age. Therefore, I split the sample along these dimensions too. Moreover, the existence of close, trusting, and supportive relationships has been identified as a protective factor against mental illness (McLaughlin and Lambert, 2017). Hence, being placed together with a sibling could have a shielding effect against adverse outcomes. While I do not observe whether siblings are placed together, I can split the sample by whether siblings are part of the same court case.

Results by subgroups are presented in Table 5. Since the samples are sliced along several dimensions, the effects are often imprecisely estimated. While Wald tests of equality reveal no statistically significant differences, I cannot rule out economically significant differences. Nevertheless, there is no evidence of decreased mortality as a result of child removal in any subgroup.

I do not present results for overdoses since the outcome, both by year 19 and by month 24, is frequently null in individual subgroups. Overdoses are concentrated among boys with behavioral problems taken into care as teenagers.

5.2.2 MTEs and Other Parameters of Interest

Heterogeneity in treatment effects can also be explored by estimating MTEs. Figure E1 traces out MTE curves over the unobserved resistance to treatment. The MTEs are attained by fitting a quadratic polynomial model using the local IV approach. 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% from the common support.

For each outcome except all-cause death by month 24, the MTE curves tend to be flat or somewhat upward-sloping. An upward slope means that the adverse effect on mortality is largest for children that have high unobserved resistance to treatment (i.e. children who have unobservable characteristics that make them unlikely to be removed).

Table E3 presents approximations of ATE, ATT, and ATUT based on MTEs obtained using various parametric models. The results reveal no evidence that child removal significantly improves mortality for the average child. However, as the common support is very limited, the parameter approximations should be interpreted with caution.

Table 5. *Heterogeneity of Effects on Child Mortality*

	Gender		Petition grounds		Background		Sibling Case		Age at Judgment		
	Girl	Boy	Behavior	Environ.	Foreign	Native	Yes	No	16-20 yrs	11-15 yrs	0-10 yrs
A: Death by Year Child Turns 19											
Removed	0.0655	0.0806*	0.1412	0.0577**	0.0478	0.0834**	0.0443	0.0911**	0.0922	0.0566	0.1001*
	(0.0452)	(0.0430)	(0.1294)	(0.0266)	(0.0555)	(0.0395)	(0.0432)	(0.0414)	(0.0594)	(0.0437)	(0.0596)
Dependent mean	0.0057	0.0082	0.0096	0.0046	0.0042	0.0092	0.0034	0.0079	0.0069	0.0071	0.0066
N	4705	5460	4483	3910	4272	5890	1751	8407	4352	4900	906
B: Death by Year Child Turns 19 (Suicide)											
Removed	0.0312	0.0387**	0.1104	0.0093	0.0115	0.0482**	0.0132	0.0438*	0.0643*	0.0173	0.0374
	(0.0330)	(0.0173)	(0.0791)	(0.0143)	(0.0308)	(0.0227)	(0.0176)	(0.0229)	(0.0365)	(0.0231)	(0.0331)
Dependent mean	0.0034	0.0018	0.0038	0.0015	0.0014	0.0034	0.0006	0.0030	0.0023	0.0027	0.0033
N	4705	5460	4483	3910	4272	5890	1751	8407	4352	4900	906
C: Death by Month 24 Post-Judgment											
Removed	0.0240	0.0101	0.0417	0.0084	0.0009	0.0258	0.0072	0.0214	0.0103	0.0398**	0.0028
	(0.0151)	(0.0216)	(0.0903)	(0.0072)	(0.0187)	(0.0173)	(0.0054)	(0.0187)	(0.0575)	(0.0172)	(0.0106)
Dependent mean	0.0022	0.0039	0.0081	0.0012	0.0018	0.0040	0.0006	0.0043	0.0082	0.0015	0.0013
N	8909	10178	5306	11828	7307	11778	6202	12887	4998	6198	7882
D: Death by Month 24 Post-Judgment (Suicide)											
Removed	0.0451*	0.0402**	0.0966*	0.0116	0.0331	0.0405**	0.0215	0.0467**	0.0581*	0.0282*	
	(0.0270)	(0.0177)	(0.0549)	(0.0092)	(0.0279)	(0.0179)	(0.0255)	(0.0193)	(0.0329)	(0.0145)	
Dependent mean	0.0017	0.0017	0.0028	0.0005	0.0011	0.0022	0.0006	0.0019	0.0026	0.0010	
N	5167	6018	5301	3949	4744	6438	1699	9480	4993	6193	

Note: This table presents IV estimates of removal on child mortality. The 'Year 19 Sample' is used in Panels A-B, the 'All Ages Sample' is used in Panel C, and the '$\geq$11 y.o. Sample' is used in Panel D (see Section 3.3). I limit the samples to the subgroup specified at the top of each column. All estimations only include court-by-year FEs. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

5.3 Robustness Checks

I present robustness checks related to sample, specification, and instrument construction decisions in Tables D1-D2. The main results are robust to dropping each court.

Baseline results are provided in Table D1, Panel A for comparison. The results are robust to limiting the sample to only include years with universal coverage of child protection cases (cases determined after February 15, 2010); cases handled by non-junior judges; cases that are randomized to any judge irrespective of position at the court; the first case per child; cases determined 24 or more months before the outbreak of Covid-19 in February 2020; cases in court-by-year cells containing at least 10 observations; cases processed by judges who handle at least 30 cases; and cases processed by judges with tendencies that are not in the top or bottom 1% of the distribution. I also show robustness to three-way clustering on judge, child, and case level; replacing court-by-year FEs with department-by-year FEs; and adding FEs for judgment day of the week and SWC in charge.

Table D2 demonstrates robustness to how judge removal tendency is measured by using three-year specific judge removal tendency; leave-out same-family judge removal tendency; judge removal tendency calculated on the subsample of first-time cases, cases handled as a non-junior judge, and cases that are randomized to any judge at the court irrespective of position; an indicator for above-average judge removal tendency; and judge removal tendency calculated by first residualizing the removal decision using court-by-year FEs (in line with Dobbie, Goldin, and Yang, 2018). I also demonstrate robustness to using a full set of judge dummies as instruments, jackknife instrumental variable estimation, and limited-information maximum likelihood.

6 Effects on Other Outcomes

6.1 Effects on Other Child Outcomes

Given that the adverse effects on mortality occur quickly (the effect on suicide is significant at the 5% level by month 9), it is valuable to examine the effects on other short-term outcomes. Next, I consider the effects on child criminality and hospitalization due to mental illness and substance use during the first year following the court's judgment. In light of the diverging effects on suicide and overdose by month 24, I present results separately for outcomes related to substance use. As these outcomes are not relevant for very young children, I use the '$\geq$11 y.o. Sample' for hospitalization outcomes. For crime outcomes, I only include children who are at least 15 years old at the time of the judgment since the minimum age of criminal responsibility in Sweden is 15.

As shown in the first column of Table 6, removal increases the risk of the marginal child being

Table 6. *Effect of Removal on Child Hospitalization & Crime, Month 1-12*

	Not Substance Use-Related			Substance Use-Related	
	(1) Hosp. d.t. Mental Health	(2) Non-Narcotic Crime	(3) Crime Against Person	(4) Hosp. d.t. Substance Use	(5) Narcotic Crime
IV (Only Court-by-Year FEs)					
Removed	0.2086**	0.5276**	0.3509*	0.0514	-0.1173
	(0.0980)	(0.2488)	(0.1919)	(0.0777)	(0.2082)
IV (With Full Set of Controls)					
Removed	0.1769*	0.5584**	0.3831**	0.0350	-0.1041
	(0.0961)	(0.2513)	(0.1938)	(0.0782)	(0.2001)
Sample	≥11 y.o.	≥15 y.o.	≥15 y.o.	≥11 y.o.	≥15 y.o.
AR p-value	0.0553	0.0151	0.0344	0.6541	0.6039
AR confidence set (95%)	[-.002,.386]	[.111,1.165]	[.038,.836]	[-.117,.193]	[-.524,.3]
Dependent mean	0.0630	0.1967	0.1136	0.0382	0.1389
Complier mean if not removed	0.0353	0.1803	0.0522	0.0556	0.1853
N	11139	7025	7025	11139	7025

Note: The '≥11 y.o. Sample' is used in columns 1 and 4 (see Section 3.3). In columns 2-3 and 5, I further limit the sample to children who had reached the age of criminal responsibility (15) at the time of the judgment. All estimations include court-by-year FEs. *IV (With Full Set of Controls)* also controls 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. OLS estimates are provided in Table D3. * $p < .1$. ** $p < .05$. *** $p < .01$.

hospitalized for mental illness within the first year by around 20 percentage points (significant at the 5-10% level).[52] Removal also increases the risk of the marginal child committing a non-narcotic crime within the first year by around 50 percentage points (5% significance level).[53] 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 91% are committed while the removed children are still placed in out-of-home care.

Again, these large estimates should be interpreted with caution (see the discussion in Section 5.1), especially in light of the large confidence sets.[54]

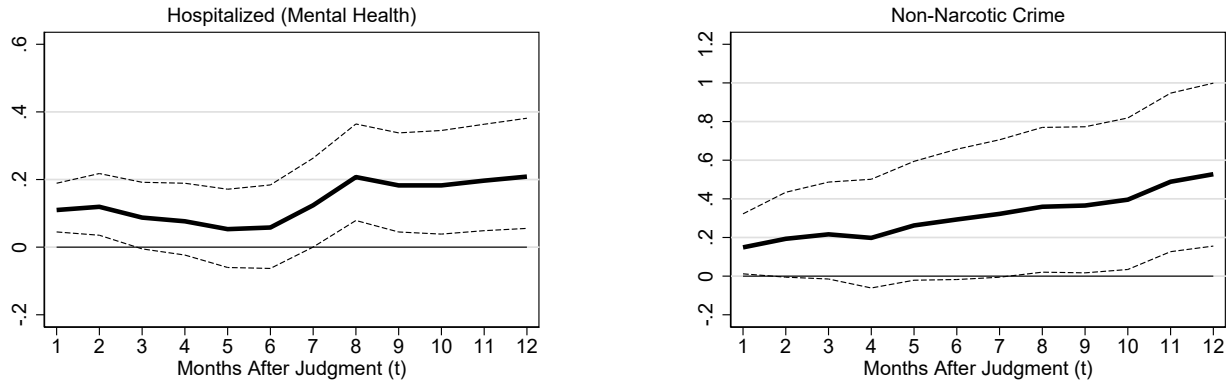
[52]OLS and reduced-form estimates are provided in Table D3. I also present estimated effects on the likelihood of committing a minor versus non-minor crime. I define minor crimes as those that do not result in a criminal trial. Note that all non-minor crimes must be processed in a trial even if the perpetrator admits guilt. Examples of minor crimes are traffic offenses and petty theft.

[53]Since I use the date of the crime, rather than the date of conviction or date of reporting, the rise in crime cannot be attributed to crimes committed prior to removal. If the crime spans several days, I use the first date when determining which month the crime occurred.

[54]During the months 1-12 following child removal, 702 of 11,139 children are hospitalized for mental illness. In the "≥11 y.o. Sample", I estimate that about 12% are compliers, while the point estimated effect of removal is 0.2086 and the control complier mean is 0.0353. Under the assumption that 20% of compliers are removed from home, the number of children who are hospitalized among compliers is estimated to be (0.0353*0.8+0.2439*0.2)*0.1191*11139≈102. If instead no compliers are removed, only 47 children would be hospitalized in the complier group. In other words, my estimates imply that child removal causes 55 additional children to be hospitalized for mental illness. However, the uncertainty in these approximations is very high. This is also the case when examining the implied change

Turning to substance use-related outcomes (columns 4-5 of Table 6), the IV estimates are not statistically significant, which is in line with the non-significant effect found on overdose by month 24.

Figure 5. *Effect of Removal on Child Hospitalization & Crime*



Note: Black lines show IV estimates of the effect of removal on the cumulative probability of the child being hospitalized due to their mental health and committing a non-narcotic crime by month t post-judgment. The relevant outcome is stated in the subfigure heading. Dashed lines show 90% AR confidence bands. The '$\geq$11 y.o. Sample' is used. For non-narcotic crime, I further limit the 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.

The outcomes used in Table 6 condition on the child surviving and never emigrating during the first year. Figure 5 shows the estimated effects of removal on the cumulative probability of (i) hospitalization due to mental health and (ii) non-narcotic crime by calendar month t post-judgment. The effects are significant at the 5-10% level already by the first calendar month following the court's judgment and the point estimates remain positive in the 12-month window. The effect on non-narcotic crime, but not hospitalization, increases fairly steadily. For hospitalization due to mental health, the effect increases after month 6, which coincides with the first review of the child's case.[55] The next kink at month 8 coincides with the steep rise in suicides (which results in these children exiting the sample).

Estimates by subsamples are presented in Table E2. The effects on non-narcotic crime and crime against persons are concentrated among youths who are 16 or older at the time of the judgment. For both outcomes, the differences in effects for children aged 15 versus 16 or older are significant at the 5% level (p-values: .014 and .032, respectively). Part of the reason can be

in the number of children who commit non-narcotic crimes. In the sample used to estimate the effect of child removal on crime, I estimate that 10.4% are compliers, while the complier mean is 0.1803 and the point estimated effect of removal is 0.7079. Under the assumption that 20% of compliers are removed, the estimates imply that (0.1803*0.8+0.7079*0.2)*0.1042*7025$\approx$209 youths commit non-narcotic crimes in the complier group. If none of the compliers are removed, the estimates suggest that only 132 of these youths would commit non-narcotic crimes during the 1-12 months following the court's judgment.

[55]The SWC must reassess the need for out-of-home placement every 6 months.

Sweden's particularly lenient treatment of offenders who are 15 as opposed to 16 or older at the time of the crime (The Prosecutor-General of Sweden, 2006).

The estimated effects of removal on crime and hospitalization 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). The focus on hospitalizations, rather than total health care usage, likely mitigates this issue. Physicians only hospitalize patients with severe injuries or illnesses that cannot wait or be treated within the Swedish primary care 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 my results. Regarding criminality, 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). Moreover, having a criminal record is an important outcome even if there is no change in actual criminality. For example, it is common among Swedish employers to conduct criminal background checks. Hence, a criminal record can adversely affect the individual's outcomes (Agan and Starr, 2018).

6.2 The Role of Parent Outcomes

In Sweden, parents typically have extensive contact with their children while they are placed in out-of-home care via phone or visits. Hence, parent outcomes may impact child outcomes already during out-of-home placement.

Tables D4-D5 present IV estimated effects of child removal on birth parents using the 'All Ages Sample'. In sharp contrast to the results for children, I find little evidence that removal impairs birth parents' health as measured by mortality and hospitalizations. In addition, none of the children of parents who died within 2 years died themselves. All in all, deteriorated parent health is not a likely mediator of the adverse effects found for child mortality.

Turning to criminal behavior, removal increases the probability of any parent committing a non-narcotic crime within the first year by around 17 percentage points (10% significance level). This increase is primarily driven by an increase in crimes against persons (13 percentage point increase; 10% significance level). For narcotic crime, the estimates are negative and not statistically significant. Likewise, there are no significant effects on family composition as measured by marriage rates or the probability of any parent having positive labor income during the year after child removal.

7 Mechanisms

My 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. In this section, I tentatively explore potential mechanisms through which removal might affect child mortality and especially suicide.

7.1 Drivers of Suicide

Stahl et al. (2021) offer an overview of the existing knowledge about the drivers of suicide. The empirical evidence suggests that suicide may be driven by the accumulation of and interaction between biological, psychological, and environmental risk factors (McFeeters et al., 2015). Such risk factors include psychiatric disorders, substance abuse, physical health conditions, personality traits, genetics, low social support, high barriers to effective health care, and adverse childhood experiences (ACEs). ACE is a concept used in the medical literature and describes a key childhood event that harms the child's health and development (Kalmakis and Chandler, 2015). ACEs are, e.g., abuse, neglect, family separation, and growing up with a mentally ill or substance-abusing family member.[56]

The evidence base indicates that individuals with a large stock of underlying risk factors react more strongly to psychosocial stressors (e.g., loss, conflict, change, and bullying) which can lead to an acute risk of suicide (Turecki and Brent, 2016; Carballo et al., 2020).[57,58] Naturally, ACEs, substance abuse, and other suicide risk factors are common among children at risk of out-of-home placement. Hence, we should expect greater responsiveness to emotionally stressful events in this group compared to children who do not interact with the child welfare system, which may partly explain why I find such large effects of court-ordered placement on suicide.

Next, I explore how court-ordered out-of-home placement can affect the accumulation of suicide risk factors and exposure to stressors.

[56] A large body of literature documents that exposure to multiple ACEs is a major risk factor for a wide variety of adverse health outcomes (for a meta-analysis, see Hughes et al., 2017). For example, the risk of a suicide attempt is estimated to be around 4-5 times higher among children who experience at least four ACEs compared to children who experience one ACE (Petruccelli et al., 2019).

[57] Empirical studies in medicine provide a biological explanation for the greater responsiveness (in terms of increased risk of suicide) to stressors among individuals who have experienced early-life adversities (for a review, see Van Heeringen and Mann, 2014).

[58] Studies in economics document evidence that further supports the notion that disadvantaged children are particularly sensitive to adverse events, including parental death (Adda et al., 2011), parental job loss (Oreopoulos et al., 2008), and parental incarceration (Dobbie, Grönqvist, et al., 2018).

7.2 Separation and Disruption of the Child's Environment

An important driver of the observed effects of court-ordered placement on child mortality can be family separation and disruption of the child's social and physical environment. First, the family separation event can be a deeply traumatic experience (Cohen and Mannarino, 2019).[59] Second, moving to a new home can be a psychologically stressful event and may involve both school and neighborhood change. Greater residential mobility during childhood has been linked with increased prevalence of depression, drug use, and other adverse outcomes (see Jelleyman and Spencer, 2008, for a meta-analysis).

Out-of-home placement can also disrupt the child's support system and social bonds with primary caretakers, teachers, relatives, friends, and other important individuals in the child's life through geographical relocation and implementation of visitation and communication restrictions. Such disruptions can lead to feelings of isolation, detachment, and loss and have long-lasting adverse effects on the child's health and development (Goldsmith et al., 2004; Astrup et al., 2017).

The extent of these disruptions is likely larger if the child must move far from their original home. To shed some 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.[60] As I cannot observe where non-removed children would have been placed had the court ordered removal, I use the child and parent characteristics listed in Table 1, court-by-year dummies, and SWC dummies to predict across-municipality moves. Prediction is done with LASSO. I then split the sample by whether the child has an above- or below-median risk of having to move and re-estimate the main IV specification in each subsample. The results are presented in Table E1. The point estimates are positive in both subsamples and tend to be marginally larger for children with *low* probability of having to move across municipalities. This suggests that large disruptions to the child's social and physical environment do not drive the effects of removal on mortality.

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. I then apply the same procedure as for across-municipality moves described above. No statistically significant differences are found for children with low versus high probability of

[59] Adverse effects of family separation have been documented in other contexts as well. For example, forced separation of migrant families is associated with trauma symptoms (see Lovato et al., 2018, for a review).

[60] Because there is a significant effect on the risk of suicide already by month 9, I focus on events that occur by month 6 when exploring mechanisms. Figure A2 depicts the distribution of placement switches and across-municipality moves by month 6.

placement instability (Table E1).[61,62]

All in all, I find little evidence that large or frequent disruptions are the main drivers of the adverse effects found for child mortality. However, caution is advised due to incomplete register data on placement characteristics (see Section 3.1). In addition, my measures of long-distance moves and placement instability might not accurately capture important disruptions in the child's life. Hence, it is still possible that disruptions adversely affect mortality.

7.3 Peers

As I show in Section 6, out-of-home placement has a large effect on the likelihood of youths committing crimes against other persons. These crimes are almost exclusively committed while the removed youth is still placed in out-of-home care. If the victims are other children in care, the increase in crime may mediate the adverse effect on mortality. Indeed, children did die from violent crimes committed by other children placed in the same home during my sample period (e.g., Hellman, 2019). Crimes against persons in the same home can also adversely affect child mental health and thereby increase the risk of suicide. Prior research shows that there are adverse effects of victimization on a range of outcomes, including mental health and suicide (Dustmann and Fasani, 2016; Nikolaou, 2017; Bharadwaj et al., 2021).

The adverse effects on child mortality can also be driven by increased exposure to peers who engage in harmful behaviors if peer-to-peer spillovers exist. In Helénsdotter (2023), 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 youths 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. I find that greater exposure to peers with a history of self-harm increases the risk of future self-harming behavior among youths with own history of self-harm. A similar, reinforcing effect is found for substance abuse.

7.4 Care Conditions

Swedish government agencies have repeatedly found widespread and oftentimes 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

[61] I try several definitions of environment instability, including an indicator for more than the median number of moves within the country during the first 6 months following removal. Regardless of the definition, I find no evidence of environment instability being an important mechanism.

[62] In Table D6, I regress the probability of death by the year the child turns 19 on child and placement characteristics among the subset of removed children. Having to move across municipalities is weakly associated with a lower likelihood of death, while the point estimate for experiencing more than one placement change is close to zero.

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.

On the other hand, there are characteristics of non-family facilities that may make such placements particularly harmful. For example, developing a secure attachment to a parent figure can be difficult in a non-family facility. Table E1 presents estimated effects of child removal on mortality by the probability of ever being placed in an institution during the first 6 months following the court's judgment. The point estimates are consistently larger for children with a high probability of institutional placement, but only the difference in estimates for suicide by month 24 is marginally significant (p-value=0.088).[63] Nevertheless, it should be noted that of the removed children who die in out-of-home care, over 60% die in group homes and institutions.

To further explore if poor care conditions can explain the adverse effects on mortality, I collect news stories from *Mediearkivet* on children who died during the years 2000-2022 while being involuntarily placed in Swedish out-of-home care.[64] I identify 26 cases in which (i) a child died from suicide and (ii) a government agency conducted an investigation and found that deficiencies in the provided care contributed to the child's suicide. Physical and sexual abuse in out-of-home care are identified as contributors to a handful of deaths, while severe neglect of the child's medical and emotional needs are identified as contributors in almost all cases. Examples of such neglect are failure to seek or facilitate psychiatric treatment and refusal to monitor or seek medical care when children express acute suicidal intent or attempt suicide. In addition, I identify 7 cases in which the child was murdered or died from a physical injury or illness attributable to neglect.

In line with the findings of these government 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 health care delivery, limited access to the child's medical history, and frequent discontinuity of health care are some of the identified barriers to health care delivery (see Deutsch and Fortin, 2015, for an overview).

Why do I find an adverse effect on the risk of dying from suicide but not overdose? Treatment of substance abuse is one of the responsibilities of the child protection system in Sweden. Hence,

[63] Estimates are based on data provided by the National Board of Institutional Care. No statistically significant differences are observed when comparing effects among children with low versus high probability of being placed in a non-family facility (group home or institution) using the incomplete register data covering all placement types. Caseworkers at the SWC, not the assigned judge, determine where the child should be placed.

[64] *Mediearkivet* is Scandinavia's largest media archive and contains stories from newspapers, radio, and television.

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 physical environment is oftentimes tailored to the needs of substance abusers. All other mental and physical illnesses are the responsibility of the child and adolescent health care 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).

7.5 Placement Exit and Transition to Adulthood

The adverse effects on mortality can be driven by poor post-placement conditions or the emotional stress of having to exit care. Hence, I examine when adverse events occur: during or after out-of-home placement. I find that for each mortality outcome, the overwhelming majority of deaths occur while the child is still placed in out-of-home care (conditional on being removed). For example, 81% of suicides by the year the removed child turns 19 occur while placed in out-of-home care. Given the issue of underreporting in the placement data, the share of children still in care at the time of death is likely even higher.

The high share of deaths during out-of-home placement speaks against poor post-placement conditions and the stress of placement exit being major drivers of my findings. On the other hand, children might end their lives before placement exit 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 18^{th} 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 18^{th} birthday or within 6 months before.

Nevertheless, 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, more than 20% 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 age out of care when they turn 21.[65] Figure A3 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

[65] Children removed due to their own behavior cannot leave care even when they turn 18 unless the SWC decides that care is no longer needed.

online, and drink alcohol. In addition, the 18-year-old must manage all contact with the school, bank, health care 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 psychosocial stressor among children struggling with mental or physical illness can be the automatic termination of their treatment within the child and adolescent health care system on the day of their 18th birthday.[66] Upon turning 18, the young adult must seek treatment at an adult unit and start to pay a fee for each visit. Typically, 18-year-olds are also transferred to a new unit within the social welfare system and are assigned a new caseworker.

8 Conclusion

This paper studies the effects of the court-ordered placement of children in out-of-home care on health and crime outcomes, including all-cause mortality, suicide, and accidental overdose. Causal effects are identified by exploiting 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 out-of-home placement adversely affects the health of children on the margin of placement. Court-ordered removal strongly increases the risk of death by the year the child turns 19 years old. This effect is primarily driven by suicides. I also trace out the effects over the months following the judgment. For children who are old enough to self-harm and use harmful substances, there is a large and significant increase in the risk of suicide already by month 9. In contrast, the point estimate is negative and not statistically significant for overdose in the 24-month window post-judgment. When using the full sample (aged 0 to 19) a positive but imprecisely estimated effect on all-cause mortality is found.

There are no statistically significant differences in treatment effects by child characteristics (gender, petition grounds, foreign background, or age). While all point estimates are positive, economically significant differences in effect magnitude cannot be ruled out.

I also examine the effects of removal on crime and hospitalization due to mental illness and substance use during the year following the court's judgment. Significant increases in the risks of (i) being hospitalized for mental illness and (ii) committing a non-narcotic crime are found already by the first month after the judgment month and the estimates remain positive for the full 12-month window. An important driver of the effect on non-narcotic crime is an increase in crimes against persons (e.g., violent and sexual crimes). These crimes are almost exclusively committed while the removed child still is in out-of-home care. In line with the non-significant

[66] Children in Sweden, irrespective of whether they are placed 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.

effect on overdoses, no effect is found on narcotic crimes or the risk of being hospitalized due to substance use during the first year.

Among birth parents, child removal causes a significant increase in non-narcotic crimes and, in particular, crimes committed against other persons during the year following the court's judgment. Other parent outcomes, such as mortality, hospitalization, family composition, and labor income are not significantly affected at conventional levels.

I explore possible mechanisms. I find suggestive evidence in favor of peer victimization, peer-to-peer spillovers, and adverse care home conditions being potentially important drivers of the effects on child mortality. In addition, the transition to adulthood appears to be a critical point with 20% of deaths occurring during the 2 months after the child turns 18. These deaths cannot be explained by poor post-placement conditions as the children who die would not age out of care until they turn 21. Indeed, the clear majority of deaths among removed children occur while the child still is placed in out-of-home care.

In this paper, I only study court-ordered placements (i.e. cases in which a parent or the child does not consent to removal). Court-ordered placements only make up around 30% of Swedish out-of-home placements on a given day. Hence, focusing solely on court-ordered placements is a limitation of the paper. However, from a policy perspective, studying the effects of court-ordered removal is particularly relevant as it involves taking a government action that intervenes with citizens' private lives. The effects of voluntary and involuntary removal are potentially different. In the future, it would be interesting to quantify and compare the effects.

Another limitation is the set of considered outcomes. There can be positive effects on other health-related outcomes (e.g., nutrition and routine health visits). Such outcomes can have important long-term effects, which might eventually switch the effect on mortality. Hence, future studies on other health (and non-health) outcomes are needed.

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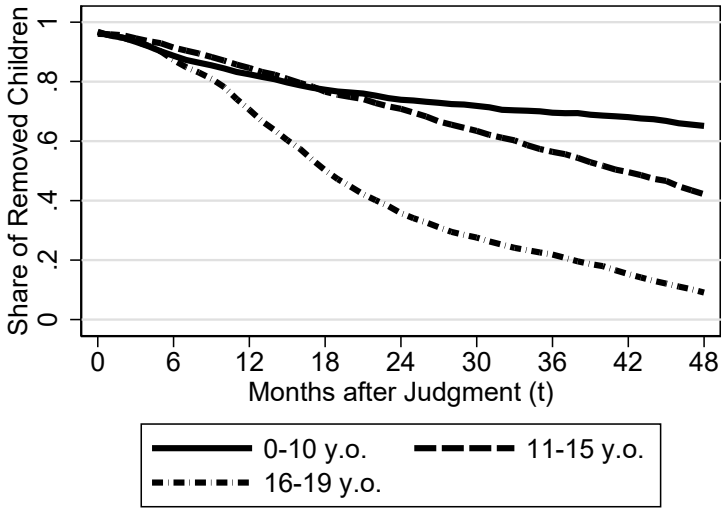
Appendix

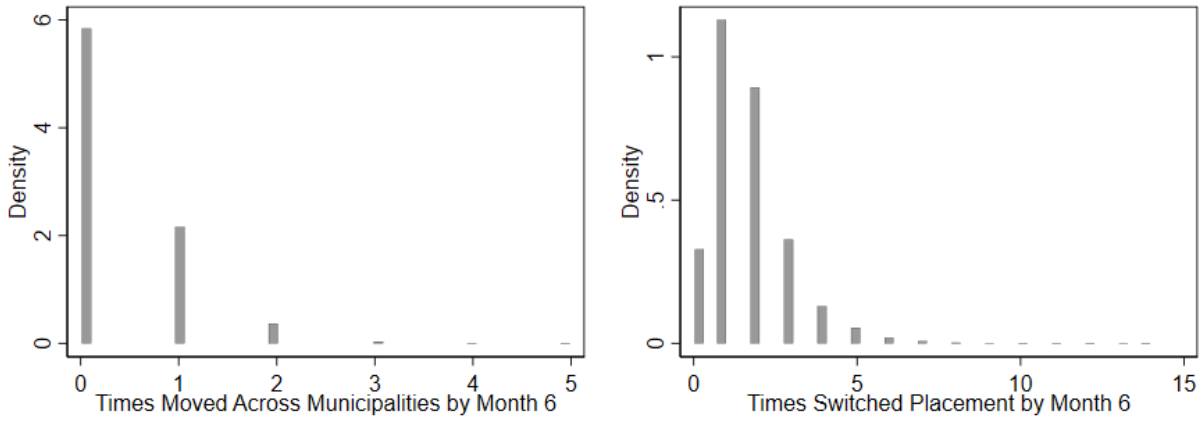
A Descriptive Statistics

Figure A1. *Share of Removed Children Still in Out-of-Home Care*



Note: This figure shows the share of children (by age at judgment) still in out-of-home care for any number of days during month t post-judgment in the 'All Ages Sample' conditional on the child (i) being removed from home and (ii) existing in the placement data on any day in the judgment month ± 1 month. This is a selective sample since the placement data is known to suffer from underreporting.

Figure A2. *Distribution of Across-Municipality Moves and Placement Switches*



Note: The left-hand subfigure gives the distribution of across-municipality moves while the right-hand subfigure gives the distribution of placement switches by month 6 following the court's judgment. I use the subsample of children in the 'All Ages Sample' who are removed. In the left-hand subfigure, I further restrict the sample to children who are observable in the placement data on any day in the judgment month ± 1 month.

Table A1. Placement Statistics in the US and Sweden

	USA	Sweden	
	(1)	(2) Involuntary	(3) Voluntary
Panel A. Age Composition of Out-of-Home Placed Children			
0 – 3 years	30%	10%	4%
4 – 6 years	16%	12%	5%
7 – 9 years	14%	14%	6%
10 – 12 years	13%	16%	8%
13 – 14 years	9%	12%	9%
15 – 17 years	16%	27%	29%
18 – 20 years	3%	9%	38%
Panel B. Children in Out-of-Home Care per 1,000			
0 – 3 years	8,2	1,4	1,4
4 – 6 years	5,8	2,1	2,0
7 – 9 years	4,7	2,4	2,5
10 – 12 years	4,3	2,7	3,3
13 – 14 years	4,4	3,1	5,4
15 – 17 years	5,3	4,8	12,2
18 – 20 years	1,0	1,6	16,0
Total	4,9	2,5	5,8
Panel C. Placement Composition			
Foster Family Home	79%	59%	58%
Group Home	4%	21%	23%
Institution	6%	11%	0%
Other	11%	9%	19%
Panel D. Living Situation After Care Termination			
Adopted by foster parents		0%	0%
Both parents		9%	10%
Foster parents given custody		7%	2%
Father		4%	4%
Involuntary care		11%	5%
Mother		13%	16%
Other		16%	27%
Own home		2%	18%
Unknown		13%	7%
Voluntary care		24%	11%

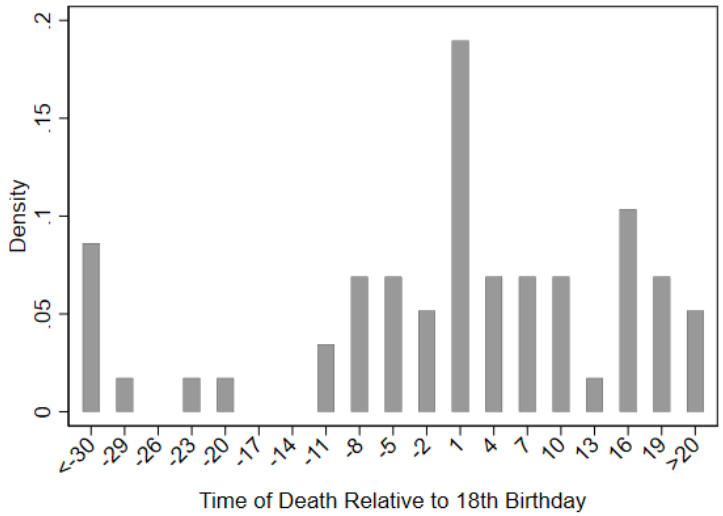
Note: This table reports placement statistics based on the children in out-of-home care on September 30, 2019, in the US (column 1) or on November 1, 2019, in Sweden (columns 2-3). Column 2 is restricted to court-ordered and emergency placements while column 3 is restricted to voluntary placements. Panel A gives the age composition, Panel B gives the number of children in out-of-home care per 1,000, and Panel C gives the most recent placement composition. Panel D reports the composition of living arrangements for children whose care ended in 2019 in Sweden. Based on statistics reported by Children's Bureau (2020) and NBHW (2020).

Table A2. *Case & Placement Characteristics*

	All Ages Sample	Year 19 Sample	≥11 y.o. Sample
A: Case Characteristics			
Months from case intake to judgment	1.67	1.60	1.58
The SWC removed the child immediately	0.78	0.80	0.80
The court rejects the immediate removal decison	0.01	0.00	0.00
Observations	19136	10200	11205
B: Placement Characteristics			
Months in out-of-home care	25.17	25.16	21.50
First placement type:			
Foster care	0.42	0.32	0.26
Group home (private)	0.21	0.24	0.27
Group home (public)	0.06	0.06	0.06
Institutional care	0.23	0.30	0.35
Kinship care	0.04	0.03	0.02
Other facility	0.04	0.04	0.03
Ever placed in by month 6:			
Congregate care	0.50	0.68	0.70
Institutional care	0.28	0.44	0.46
Kinship care	0.05	0.04	0.03
Observations	15307	8469	9296

Note: This table presents case and placement characteristics in the 'All Ages Sample', 'Year 19 Sample', and '≥11 y.o. Sample'. Placement characteristics (Panel B) are shown for the first placement spell or during the first 6 months after court-ordered removal conditional on the child (i) being removed from home and (ii) existing in the placement data on any day in the judgment month ±1 month.

Figure A3. *Distribution of Months Between 18th Birthday and Death*



Note: This figure depicts the distribution of months between the month of the child's 18th birthday and the month of death. The subsample of children who die from any cause by the year they turn 19 is used. Deaths that occur within the period 30 months before and 20 months after the month of the child's 18th birthday are grouped into bins that represent 3 months. The month of the child's birthday is included in the 0-2 month bin labeled '1'.

Table A3. *Descriptive Statistics for All, Removed and Compliers*

	All Ages Sample				Year 19 Sample				≥11 y.o. Sample			
	All	Removed	Not Removed	Complier	All	Removed	Not Removed	Complier	All	Removed	Not Removed	Complier
Girl	0.47	0.46	0.49	0.53	0.46	0.46	0.49	0.45	0.46	0.46	0.50	0.47
<11 yrs	0.41	0.40	0.55	0.47	0.09	0.08	0.16	0.16				
11-15 yrs	0.32	0.33	0.28	0.32	0.48	0.48	0.49	0.52	0.55	0.55	0.63	0.60
>15 yrs	0.26	0.27	0.17	0.21	0.43	0.44	0.34	0.35	0.45	0.45	0.37	0.39
Sibling case	0.33	0.31	0.44	0.28	0.17	0.16	0.30	0.19	0.15	0.14	0.27	0.15
Foreign background	0.38	0.39	0.32	0.36	0.42	0.42	0.38	0.32	0.42	0.43	0.39	0.33
Behavior petition	0.28	0.30	0.14	0.13	0.44	0.46	0.27	0.20	0.47	0.49	0.31	0.24
Environment petition	0.62	0.60	0.79	0.77	0.39	0.36	0.59	0.61	0.35	0.33	0.55	0.52
Double grounds petition	0.10	0.11	0.07	0.09	0.17	0.18	0.14	0.17	0.17	0.18	0.14	0.16
Child consents to removal	0.65	0.69	0.35	0.58	0.44	0.46	0.22	0.38	0.48	0.50	0.22	0.42
At least 1 parent consents to removal	0.36	0.38	0.17	0.12	0.52	0.54	0.24	0.07	0.48	0.51	0.22	0.06
Case largely based on child mental health	0.04	0.04	0.07	0.08	0.06	0.06	0.12	0.14	0.07	0.06	0.16	0.15
Non-junior case type	0.17	0.16	0.20	0.15	0.09	0.09	0.11	0.08	0.08	0.08	0.11	0.09
Any birth parent:												
Dead	0.05	0.05	0.04	0.06	0.06	0.06	0.06	0.07	0.06	0.06	0.07	0.08
<18 y.o. at birth of child	0.02	0.02	0.02	0.06	0.02	0.03	0.02	0.07	0.03	0.03	0.02	0.05
Married, yr t-1	0.45	0.46	0.42	0.45	0.49	0.49	0.49	0.46	0.49	0.49	0.48	0.49
No labor income, yr t-1	0.63	0.62	0.68	0.61	0.56	0.56	0.61	0.67	0.55	0.54	0.60	0.66
Hosp. d.t. mental health, yr t-1	0.07	0.07	0.07	0.11	0.06	0.06	0.05	0.10	0.05	0.05	0.04	0.05
Hosp. d.t. substance use, yr t-1	0.05	0.05	0.05	0.07	0.04	0.04	0.04	0.04	0.04	0.04	0.03	0.05
Any crime, yr t-1	0.16	0.17	0.15	0.25	0.11	0.11	0.11	0.04	0.11	0.10	0.11	0.00
Observations	19136	16910	2226	19136	10200	9187	1013	10200	11205	10197	1008	11205

Note: This table presents descriptive statistics on child and parent characteristics for all children, removed children, and compliers within each analysis sample. To characterize the subpopulation of compliers within each estimation sample, 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 (1st and 99th percentiles). I then calculate the overall proportion of compliers in each estimation sample as the difference in the first-stage coefficient between children assigned the most stringent and least stringent judges. I then follow the same procedure to compute the share of compliers within each characteristic subgroup. Then, by dividing the share of compliers in each subgroup by the total share of compliers, I can retrieve the relative likelihood of a complier belonging to a characteristic subgroup. Finally, I multiply the original probability of an observation belonging to a characteristic subgroup with the computed relative likelihoods.

B Attrition

When studying mortality, I treat children as attrited if they emigrate from Sweden during the specified period (e.g., months 0-24) and do not return by the end of the latest available year (2022). In practice, there are no other meaningful sources of attrition than emigration. Even if a person changes their name or goes missing, they would most likely be identified if they die in Sweden. When an unknown person dies in Sweden, the National Board of Forensic Medicine (NBFM) investigates their identity. Since Sweden has free dental care for residents up until the year they turn 24 and well-documented dental care, most unknown individuals are identified using dental X-rays. During the last 4 years, the identity could not be confirmed in only about 10 cases.

When studying hospitalization and crime during the months following the court's judgment, I treat children as attrited if they die or ever emigrate from Sweden during the specified period (e.g., months 1-12). In contact with the health care and judicial systems, the individual must provide their personal identity number and support their identity (e.g., using a physical or digital identification card). All individuals in my analysis samples have accurate personal identity numbers. Even if a person obtains protected identity status or changes their personal identity number, all hospitalizations and legal proceedings in Sweden would be linked to their person if they identified themselves.[67] It is possible that hospitalizations and legal proceedings are not accurately registered if the person refuses to identify themselves or uses someone else's identity. However, there are strong motives against failing to identify oneself. First, it is a crime to use someone else's identity and health care personnel can report suspected illegal identity use to the police. In addition, Swedish residents pay nothing or a small fee for health care, but if health care professionals cannot verify the patient's identity or suspect illegal identity use, they can require that the patient pays for the care in full.

Table B1. *Test of Selective Attrition*

		Missing Information on...				
	(1) No Personal Identity Number	(2) Death by Year 19	(3) Death by Month 24	(4) Death by Month 24	(5) Hospital-ization Months 1-12	(6) Crime Months 1-12
Judge removal tendency	0.0346	-0.0075	0.0013	-0.0134*	-0.0063	-0.0098
	(0.0335)	(0.0111)	(0.0072)	(0.0070)	(0.0120)	(0.0174)
Sample		Year 19	All Ages	≥11 y.o.	≥11 y.o.	≥15 y.o.
Dependent mean	0.0603	0.0031	0.0025	0.0014	0.0059	0.0069
N	20471	10200	19136	11205	11205	7074

Note: Column 1 regresses an indicator for missing personal identity number on judge removal tendency using observations with non-missing removal tendency in court-by-year cells containing more than 1 judge. In columns 2-6, I regress an indicator for missing information on child death, hospitalization, or crime within the sample specified at the bottom of the table. Sample attrition can occur because of emigration or (in columns 5-6) death. All regressions include court-by-year FEs. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

[67] If a person cannot provide a conventional form of identification such as a driver's license, the identity can be supported by, for example, providing a transcript from the Swedish Tax Authorities.

Table B2. *Descriptive Statistics Excluding Attriters*

	Year 19	Month 24		Month 12	
	Mortality	Mortality	Mortality	Hospital-ization	Crime
A: Child & Parent Characteristics					
Removed	0.90	0.88	0.91	0.91	0.92
Girl	0.46	0.47	0.46	0.46	0.42
Age at judgment	14.50	10.75	15.05	15.04	16.31
Sibling case	0.17	0.32	0.15	0.15	0.06
Foreign background	0.42	0.38	0.42	0.42	0.42
Behavior petition	0.44	0.28	0.47	0.47	0.64
Environment petition	0.38	0.62	0.35	0.35	0.20
Double grounds petition	0.17	0.10	0.17	0.17	0.17
Child consents to removal	0.44	0.65	0.48	0.48	0.28
At least 1 parent consents to removal	0.52	0.36	0.48	0.49	0.57
Case largely based on child mental health	0.06	0.04	0.07	0.07	0.05
Non-junior case type	0.09	0.17	0.08	0.08	0.09
Committed (yrs t-1 to t-3):					
Crime against person	0.12	0.09	0.13	0.13	0.20
Narcotic crime	0.11	0.10	0.14	0.14	0.22
Other crime	0.14	0.11	0.16	0.16	0.25
Hospitalized (yrs t-1 to t-3) due to:					
Mental health	0.08	0.06	0.09	0.09	0.12
Substance use	0.06	0.05	0.07	0.07	0.10
Missing, yrs t-1 to t-3	0.11	0.24	0.11	0.11	0.11
Any birth parent:					
Dead	0.06	0.05	0.06	0.06	0.06
$<$18 y.o. at birth of child	0.02	0.02	0.03	0.03	0.02
Married, yr t-1	0.49	0.45	0.49	0.49	0.50
No labor income, yr t-1	0.56	0.63	0.55	0.55	0.50
Hosp. d.t. mental health, yr t-1	0.05	0.07	0.05	0.05	0.04
Hosp. d.t. substance use, yr t-1	0.04	0.05	0.04	0.04	0.03
Any crime, yr t-1	0.11	0.17	0.11	0.11	0.09
Missing Xs, yr t-1	0.28	0.24	0.28	0.27	0.28
B: Judge Characteristics					
Judge removal tendency	0.88	0.89	0.89	0.89	0.88
Junior judge	0.03	0.03	0.03	0.03	0.02
Female judge	0.47	0.50	0.49	0.49	0.48
Judge age	52.65	52.56	52.50	52.50	52.53
Sample	Year 19	All Ages	$\geq$11 y.o.	$\geq$11 y.o.	$\geq$15 y.o.
Unique judges	249	249	249	249	249
Unique cases	9412	15332	10532	10487	6947
Unique children	9560	17992	10544	10498	6723
Unique birth parents	15283	24803	17013	16955	11504
Observations	10168	19089	11189	11139	7025

Note: This table presents descriptive statistics on child, parent, and judge characteristics for each analysis sample used to study mortality (see Section 3.3) but excluding children who attrited by the year the child turns 19 or by month 24 following the court's judgment. I also present descriptive statistics for children who never attrited during the 1-12 months after the court's judgment in the '$\geq$11 y.o.' and '$\geq$15 y.o.' samples. Statistics are shown for observations with non-missing information.

Table B3. *Effect of Removal on Child Mortality Incl. Attriters*

	Death by Year Child Turns 19			Death by Month 24 Post-Judgment		
	(1) All-Cause	(2) Suicide	(3) Overdose	(4) All-Cause	(5) Suicide	(6) Overdose
OLS (No Controls)						
Removed	-0.0029	-0.0046***	-0.0011	-0.0018	-0.0003	0.0014
	(0.0028)	(0.0017)	(0.0015)	(0.0013)	(0.0014)	(0.0012)
OLS (With Full Set of Controls)						
Removed	-0.0055	-0.0065**	-0.0018	-0.0030	-0.0019	0.0011**
	(0.0037)	(0.0031)	(0.0023)	(0.0019)	(0.0017)	(0.0005)
OLS (Complier Reweighted)						
Removed	-0.0071	-0.0072*	-0.0033	-0.0019	-0.0009	0.0010*
	(0.0044)	(0.0038)	(0.0032)	(0.0016)	(0.0010)	(0.0005)
RF (Only Court-by-Year FEs)						
Judge removal tendency	0.0350***	0.0184**	0.0161*	0.0096	0.0150***	-0.0067
	(0.0133)	(0.0078)	(0.0086)	(0.0065)	(0.0054)	(0.0058)
IV (Only Court-by-Year FEs)						
Removed	0.0791**	0.0417**	0.0364*	0.0227	0.0387**	-0.0173
	(0.0324)	(0.0189)	(0.0202)	(0.0156)	(0.0150)	(0.0151)
IV (With Full Set of Controls)						
Removed	0.0791**	0.0404**	0.0366*	0.0221	0.0387**	-0.0184
	(0.0328)	(0.0190)	(0.0204)	(0.0158)	(0.0152)	(0.0155)
Sample	Year 19	Year 19	Year 19	All Ages	≥11 y.o.	≥11 y.o.
AR *p*-value	0.0098	0.0241	0.0635	0.1510	0.0060	0.2268
AR confidence set (95%)	[.021,.151]	[.007,.082]	[-.001,.081]	[-.007,.055]	[.012,.073]	[-.051,.012]
Dependent mean	0.0073	0.0027	0.0020	0.0033	0.0017	0.0012
Complier mean if not removed	0.0156	0.0023	0.0083	0.0006	0.0000	0.0000
N	10200	10200	10200	19136	11205	11205

Note: This table reproduces Table 4 while including attriters. To provide a conservative measure, non-removed attriters are assumed to have the worst outcomes (e.g., suicide) while removed attriters have the best outcomes. AR *p*-values and confidence sets are for *IV (Only Court-by-Year FEs)*. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

C Tests of Assumptions

Table C1. *Tests of Random Assignment of Other Judge Characteristics*

	Female Judge	Judge Age	Non-Junior Judge
F-statistic	1.05	1.28	1.05
p-value	0.40	0.16	0.39
N	19136	19136	19136

Note: Test of random assignment of judge gender, age, and junior position using the 'All Ages Sample'. All estimations include the child and parent characteristics listed in Table 1 and court-by-year FEs. Reported *F*-statistic of joint significance is for the child and parent characteristics only. Standard errors are clustered at the case level.

Table C2. *Tests of Random Assignment: Sample Decisions*

	All Ages Sample	≥11 y.o. Sample	Year 19 Sample
A: Baseline			
F-statistic	0.50	0.55	0.58
p-value	0.98	0.97	0.95
N	19136	11205	10200
B: Sample With National Coverage			
F-statistic	0.73	0.73	0.77
p-value	0.83	0.84	0.78
N	17373	9996	8723
C: Excluding Non-Junior Cases			
F-statistic	0.48	0.52	0.64
p-value	0.99	0.97	0.91
N	15971	10289	9299
D: First-Time Cases			
F-statistic	0.56	0.61	0.67
p-value	0.96	0.93	0.89
N	17752	10209	9408
E: Cases Determined ≥24 Months Before Covid-19			
F-statistic	0.57	0.76	0.72
p-value	0.96	0.80	0.85
N	15358	9095	9074
*F: Cases in Court*Year Cells With ≥10 obs*			
F-statistic	0.50	0.56	0.60
p-value	0.98	0.96	0.94
N	19094	11122	10141
G: Same Sample as in Table 4			
F-statistic	0.52	0.56	0.56
p-value	0.98	0.96	0.96
N	19089	11189	10168
H: Non-Junior Judges			
F-statistic	0.45	0.54	0.65
p-value	0.99	0.97	0.91
N	18490	10818	9832
I: Each Judge Handles ≥30 Cases			
F-statistic	0.44	0.52	0.57
p-value	0.99	0.98	0.96
N	18369	10745	9825
J: Excluding Judges With Top or Bottom 1% Residualized Tendency			
F-statistic	0.65	0.76	0.86
p-value	0.91	0.80	0.66
N	18746	10986	9976

Note: In these randomization tests, I limit the baseline samples to years with universal coverage (Panel B), cases that are randomly assigned to any judge within the judge pool irrespective of the judge's seniority (Panel C), the first case for each child (Panel D), cases decided ≥24 months before February 2020 (Panel E), cases in court-by-year cells with at least 10 observations (Panel F), the samples (excluding attriters) used in Table 4 (Panel G), cases processed by non-junior judges (Panel H), judges who handle at least 30 cases during the sample period (Panel I) and judges whose residualized (using court-by-year FEs) removal tendency is between the 1st and 99th percentiles of the distribution (Panel J). All estimations include the child and parent characteristics listed in Table 1 and court-by-year FEs. Reported *F*-statistic of joint significance is for the child and parent characteristics only. Standard errors are clustered at the case level.

Table C3. *Tests of Random Assignment: Specification and Instrument Decisions*

	All Ages Sample	≥11 y.o. Sample	Year 19 Sample
A: Three-Way Cluster at Case, Child, and Judge Level			
F-statistic	0.64	0.65	0.75
p-value	0.90	0.90	0.81
N	19136	11205	10200
B: Court-by-Year FEs Replaced With Department-by-Year FEs			
F-statistic	0.64	0.72	0.62
p-value	0.91	0.84	0.93
N	19111	11173	10174
C: Add Day-of-Week and Social Welfare Committee FEs			
F-statistic	0.48	0.50	0.51
p-value	0.99	0.98	0.98
N	19127	11191	10188
D: Three-Year Specific Judge Removal Tendency			
F-statistic	0.79	0.75	0.96
p-value	0.76	0.81	0.53
N	12834	7455	6524
E: Leave-Out Same-Family Judge Removal Tendency			
F-statistic	0.49	0.54	0.58
p-value	0.98	0.97	0.95
N	19136	11205	10200
F: Judge Removal Tendency Excl. Return Children			
F-statistic	0.50	0.66	0.67
p-value	0.98	0.90	0.89
N	17752	10209	9408
G: Judge Removal Tendency Excl. Cases Handled as Junior			
F-statistic	0.47	0.54	0.63
p-value	0.99	0.97	0.92
N	18637	10913	9946
H: Judge Removal Tendency Excl. Non-Junior Cases			
F-statistic	0.57	0.61	0.65
p-value	0.95	0.93	0.91
N	15971	10289	9299
I: Indicator for Judge Removal Tendency Above Mean			
F-statistic	0.87	0.73	0.73
p-value	0.65	0.83	0.83
N	19136	11205	10200
J: Judge Removal Tendency Calculated Following Dobbie et al. (2018)			
F-statistic	0.50	0.55	0.58
p-value	0.98	0.97	0.95
N	19136	11205	10200

Note: Panel A clusters the standard errors on the case, judge, and child level. Panel B replaces court-by-year FEs with department-by-year FEs. Panel C adds FEs for judgment day of the week and SWC. Panel D redefines the instrument as the judge's removal rate among cases handed down in the same 3-year period. Panels E-H redefine the instrument as the judge's removal rate excluding cases involving the same child or parent as in the focal case; children who have been part of a case before; cases handled while the judge held a junior position; and non-junior cases. Panel I replaces the instrument with an indicator for above-mean removal tendency. In Panel J, judge removal tendency is calculated by first residualizing removal using court-by-year FEs (see Dobbie, Goldin, and Yang, 2018). All estimations include child and parent characteristics (Table 1) and court-by-year FEs. Reported *F*-statistic of joint significance is for the child and parent characteristics. Standard errors are clustered at the case level.

Table C4. *Additional First-Stage Estimates: Sample Decisions*

	All Ages Sample		Year 19 Sample		≥11 y.o. Sample	
	Coeff	Std err	Coeff	Std err	Coeff	Std err
A: Baseline						
Judge removal tendency	0.4237***	0.0550	0.4422***	0.0609	0.3887***	0.0552
Effective *F*-statistic	60.57		53.46		49.70	
N	19136		10200		11205	
B: Sample With National Coverage						
Judge removal tendency	0.4563***	0.0576	0.4525***	0.0650	0.3907***	0.0585
Effective *F*-statistic	63.86		48.59		44.77	
N	17373		8723		9996	
C: Excluding Non-Junior Cases						
Judge removal tendency	0.4322***	0.0566	0.4642***	0.0633	0.3974***	0.0565
Effective *F*-statistic	59.52		54.60		49.63	
N	15971		9299		10289	
D: First-Time Cases						
Judge removal tendency	0.4105***	0.0570	0.4433***	0.0634	0.3952***	0.0576
Effective *F*-statistic	52.96		49.60		47.13	
N	17752		9408		10209	
E: Cases Determined ≥24 Months Before Covid-19						
Judge removal tendency	0.4215***	0.0597	0.4691***	0.0652	0.4245***	0.0615
Effective *F*-statistic	51.33		52.66		47.81	
N	15358		9074		9095	
*F: Cases in Court*Year Cells With ≥10 obs*						
Judge removal tendency	0.4242***	0.0550	0.4470***	0.0609	0.3865***	0.0553
Effective *F*-statistic	60.63		54.59		48.96	
N	19094		10141		11122	
G: Same Sample as in Table 4						
Judge removal tendency	0.4277***	0.0550	0.4466***	0.0611	0.3900***	0.0553
Effective *F*-statistic	60.57		53.46		49.70	
N	19089		10168		11189	
H: Non-Junior Judges						
Judge removal tendency	0.4150***	0.0571	0.4214***	0.0636	0.3713***	0.0576
Effective *F*-statistic	53.86		44.65		41.65	
N	18490		9832		10818	
I: Each Judge Handles ≥30 Cases						
Judge removal tendency	0.4327***	0.0584	0.4435***	0.0638	0.3863***	0.0586
Effective *F*-statistic	56.16		48.99		43.55	
N	18369		9825		10745	
J: Excluding Judges With Top or Bottom 1% Residualized Tendency						
Judge removal tendency	0.4041***	0.0593	0.4047***	0.0644	0.3798***	0.0580
Effective *F*-statistic	47.52		40.27		43.06	
N	18746		9976		10986	

Note: I limit the baseline samples to years with universal coverage (Panel B), cases that are randomly assigned to any judge within the judge pool irrespective of seniority (Panel C), the first case for each child (Panel D), cases decided ≥24 months before February 2020 (Panel E), cases in court-by-year cells with at least 10 observations (Panel F), the samples (excluding attriters) used in Table 4 (Panel G), cases processed by non-junior judges (Panel H), judges who handle at least 30 cases during the sample period (Panel I), and judges whose residualized (using court-by-year FEs) removal tendency is between the 1st and 99th percentiles of the distribution (Panel J). All estimations include court-by-year FEs. I report Olea and Pflueger (2013)'s effective *F*-statistic. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table C5. *Additional First-Stage Estimates: Specification and Instrument Decisions*

	All Ages Sample		Year 19 Sample		≥11 y.o. Sample	
	Coeff	Std err	Coeff	Std err	Coeff	Std err
A: Three-Way Cluster at Case, Child, and Judge Level						
Instrument	0.4237***	0.0719	0.4422***	0.0721	0.3887***	0.0583
Effective *F*-statistic	60.61		53.50		49.32	
N	19136		10200		11205	
B: Court-by-Year FEs Replaced With Department-by-Year FEs						
Instrument	0.3648***	0.0577	0.3702***	0.0638	0.3313***	0.0583
Effective *F*-statistic	40.77		34.29		32.28	
N	19111		10174		11173	
C: Add Day-of-Week and Social Welfare Committee FEs						
Instrument	0.4286***	0.0540	0.4445***	0.0607	0.3844***	0.0547
Effective *F*-statistic	64.18		54.21		49.32	
N	19127		10188		11191	
D: Three-Year Judge Removal Tendency						
Instrument	0.2697***	0.0631	0.2416***	0.0737	0.2846***	0.0664
Effective *F*-statistic	19.00		10.70		18.42	
N	12834		6524		7455	
E: Leave-Out Same-Family Judge Removal Tendency						
Instrument	0.4160***	0.0550	0.4408***	0.0609	0.3882***	0.0553
Effective *F*-statistic	58.27		53.12		49.51	
N	19136		10200		11205	
F: Judge Removal Tendency Excl. Return Children						
Instrument	0.3727***	0.0534	0.4002***	0.0589	0.3581***	0.0536
Effective *F*-statistic	49.71		46.83		44.76	
N	17752		9408		10209	
G: Judge Removal Tendency Excl. Cases Handled as Junior						
Instrument	0.3993***	0.0559	0.4115***	0.0635	0.3625***	0.0574
Effective *F*-statistic	52.17		42.68		39.97	
N	18637		9946		10913	
H: Judge Removal Tendency Excl. Non-Junior Cases						
Instrument	0.4140***	0.0543	0.4358***	0.0601	0.3789***	0.0540
Effective *F*-statistic	59.22		53.18		49.26	
N	15971		9299		10289	
I: Indicator for Judge Removal Tendency Above Mean						
Instrument	0.0408***	0.0070	0.0428***	0.0079	0.0370***	0.0070
Effective *F*-statistic	34.26		30.22		27.88	
N	19136		10200		11205	
J: Judge Removal Tendency Calculated Following Dobbie et al. (2018)						
Instrument	0.4237***	0.0550	0.4422***	0.0609	0.3886***	0.0552
Effective *F*-statistic	60.55		53.45		49.66	
N	19136		10200		11205	

Note: Panel A clusters the standard errors on the case, judge, and child level. Panel B replaces court-by-year FEs with department-by-year FEs. Panel C adds FEs for judgment day of the week and SWC. Panel D redefines the instrument as the judge's removal rate among cases handed down in the same 3-year period. Panels E-H redefine the instrument as the judge's removal rate excluding cases involving the same child or parent as in the focal case; children who have been part of a case before; cases handled while the judge held a junior position; and non-junior cases. Panel I replaces the instrument with an indicator for above-mean removal tendency. In Panel J, judge removal tendency is calculated by first residualizing removal using court-by-year FEs (see Dobbie, Goldin, and Yang, 2018). All estimations include court-by-year FEs. I report Olea and Pflueger (2013)'s effective *F*-statistic. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table C6. *Frandsen et al. (2023)'s Test*

	5 knots	10 knots	15 knots	20 knots
A: Death by Year Child Turns 19				
Test statistic	70	66	61	57
p-value	[1.000]	[1.000]	[1.000]	[1.000]
B: Death by Year Child Turns 19 (Suicide)				
Test statistic	26	24	22	22
p-value	[1.000]	[1.000]	[1.000]	[1.000]
C: Death by Year Child Turns 19 (Overdose)				
Test statistic	18	18	18	17
p-value	[1.000]	[1.000]	[1.000]	[1.000]
D: Death by Month 24				
Test statistic	61	57	53	66
p-value	[1.000]	[1.000]	[1.000]	[1.000]
E: Death by Month 24 (Suicide)				
Test statistic	19	19	47	21
p-value	[1.000]	[1.000]	[1.000]	[1.000]
F: Death by Month 24 (Overdose)				
Test statistic	14	14	14	13
p-value	[1.000]	[1.000]	[1.000]	[1.000]
G: Hosp. d.t. Mental Illness, Months 1-12				
Test statistic	284	214	176	155
p-value	[0.009]	[0.689]	[0.987]	[0.999]
H: Non-Narcotic Crime, Months 1-12				
Test statistic	381	248	252	230
p-value	[0.000]	[0.138]	[0.067]	[0.234]
I: Crime Against Person, Months 1-12				
Test statistic	326	234	184	194
p-value	[0.000]	[0.321]	[0.964]	[0.846]
J: Hosp. d.t. Substance Use, Months 1-12				
Test statistic	373	298	253	158
p-value	[0.000]	[0.001]	[0.061]	[0.999]
K: Narcotic Crime, Months 1-12				
Test statistic	305	221	203	150
p-value	[0.001]	[0.569]	[0.789]	[1.000]
d.f.	230	225	220	215

Note: Application of Frandsen et al. (2023)'s test of random assignment, exclusion restriction, and strong monotonicity using the 'Year 19 Sample' (Panels A-C), the 'All Ages Sample' (Panel D), and the '$\geq$11 y.o. Sample' (Panels E-K). In Panels H-I and K, I further limit the sample to children who had reached the age of criminal responsibility by the judgment date. Each panel gives the test statistic and *p*-value associated with a separate test. The outcome is indicated in the panel heading. The number of knots used in the spline function is indicated at the top of the table, while degrees of freedom are shown at the bottom. Failure to reject the null hypothesis implies that I cannot reject the null that random assignment, exclusion restriction, and strong monotonicity jointly hold.

Table C7. *Test of Implications of the Exclusion Restriction*

	(1) Judge Removal Tendency	(2) Judge Removal Tendency
Months from case intake to judgment	0.0004	
	(0.0007)	
The SWC removed the child immediately	0.0015	
	(0.0014)	
The court rejects the immediate removal decison	-0.0030	
	(0.0088)	
First placement type:		
Foster care		-0.0052
		(0.0057)
Group home		-0.0034
		(0.0057)
Institutional care		-0.0046
		(0.0058)
Kinship care		-0.0079
		(0.0070)
Missing first placement type		-0.0022
		(0.0057)
Months in out-of-home care		0.0000
		(0.0000)
Missing service length		-0.0004
		(0.0014)
No. of placement switches by month 6		0.0004
		(0.0004)
No. of across-municipality moves by month 6		-0.0021
		(0.0017)
No. of within-country moves by month 6		0.0002
		(0.0014)
F-statistic	0.53	0.87
p-value	0.66	0.56
Dependent mean	0.89	0.89
N	18909	15285

Note: Column 1 reports the results from a regression of judge removal tendency on the number of months from case intake to the judgment is announced, an indicator for the SWC placing the child in emergency care before the court hearing, and an indicator for the court rejecting the decision to place the child in emergency care before the court hearing. Column 1 uses the 'All Ages Sample' (see Section 3.3) excluding observations with missing case processing time (N=227). Column 2 reports the results from a regression of judge removal tendency on the characteristics of the first placement spell. The omitted placement type is "Other facility". Column 2 uses the 'All Ages Sample' but restricted to children who are (i) removed and (ii) observable in the placement data on any day in the judgment month ± 1 month. All regressions include court-by-year FEs. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table C8. *First-Stage Estimates of Removal on Judge Removal Tendency in Subsamples*

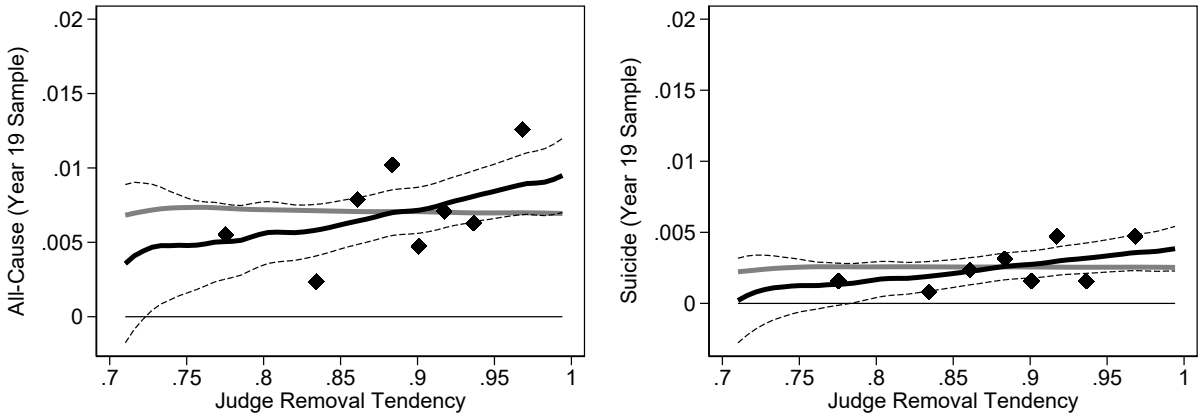
	(1) Behavior	(2) Environment	(3) 0-10 years	(4) 11-15 years	(5) 16-20 years	(6) Native Background	(7) Foreign Background	(8) Girl	(9) Boy
A: Main Measure of Judge Removal Tendency									
Judge removal tendency	0.2039***	0.5172***	0.4728***	0.4238***	0.3386***	0.4568***	0.3871***	0.4733***	0.3954***
	(0.0580)	(0.0799)	(0.0956)	(0.0781)	(0.0703)	(0.0720)	(0.0811)	(0.0737)	(0.0660)
Dependent mean	0.94	0.85	0.85	0.90	0.92	0.87	0.90	0.88	0.89
Effective *F*-statistic	12.50	42.74	25.06	29.50	23.30	40.78	23.79	42.30	36.27
N	5312	11865	7913	6206	5006	11800	7332	8931	10203
B: Reverse-Sample Judge Removal Tendency									
Judge removal tendency	0.1107**	0.3667***	0.5803***	0.3574***	0.2406***	0.2887***	0.3627***	0.3672***	0.2601***
	(0.0440)	(0.0905)	(0.1034)	(0.0689)	(0.0577)	(0.0532)	(0.0683)	(0.0660)	(0.0500)
Dependent mean	0.94	0.85	0.85	0.90	0.92	0.87	0.90	0.88	0.89
Effective *F*-statistic	6.44	16.67	31.74	26.94	17.44	29.46	29.78	31.56	27.19
N	5312	11865	7913	6206	5006	11800	7332	8931	10203

Note: First-stage estimates in subsamples of the baseline 'All Ages Sample' using the main measure of judge removal tendency (Panel A) and reverse-sample judge removal tendency (Panel B). Reverse-sample judge removal tendency is defined as the judge's removal tendency for cases outside of the subsample. The subsample used when re-estimating the first stage is indicated in the column heading. All estimations include court-by-year FEs. Standard errors are clustered at the case level. Olea and Pflueger (2013)'s effective *F*-statistic of joint significance is for judge removal tendency. * $p < .1$. ** $p < .05$. *** $p < .01$.

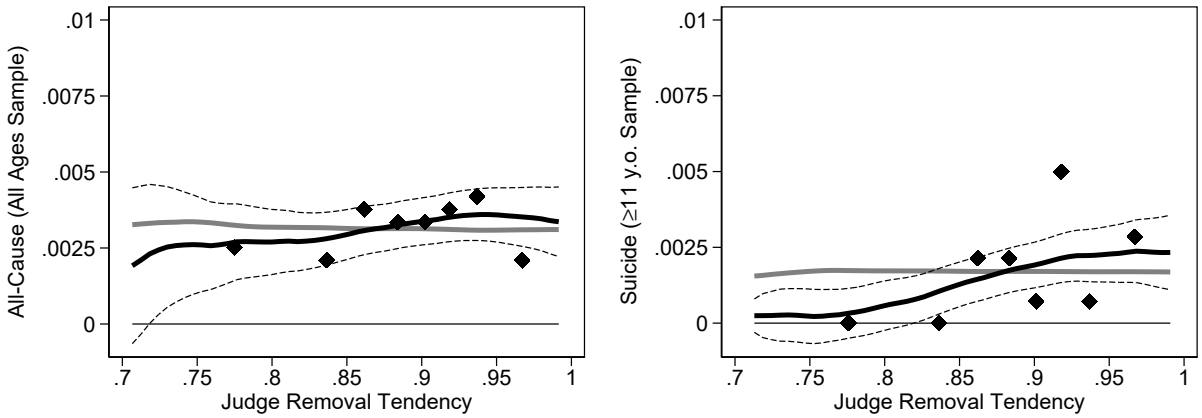
D Results

Figure D1. *Child Mortality vs Judge Removal Tendency*

Panel A. Death by Year Child Turns 19



Panel B. Death by Month 24 Post-Judgment



Note: Each solid black line shows a Kernel-weighted local polynomial regression of the mortality outcome (as indicated on the y-axis) 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 background characteristics listed in Table 1) on judge removal tendency. The sample used is indicated on the y-axis title (see Section 3.3 for details). Child outcomes and judge removal tendency are residualized using court-by-year FEs and mean-standardized. Settings: triangle Kernel, degree 0, and bandwidth 0.10.

Table D1. *Robustness Checks of Effects on Child Mortality I*

	Death by Year Child Turns 19				Death by Month 24 Post-Judgment			
	All-Cause		Suicide		All-Cause		Suicide	
	Coeff	Std err	Coeff	Std err	Coeff	Std err	Coeff	Std err
A: Baseline								
Removed	0.0719**	0.0312	0.0350**	0.0173	0.0154	0.0131	0.0383**	0.0150
Observations	10168		10168		19089		11189	
B: Sample With National Coverage								
Removed	0.0824**	0.0343	0.0364*	0.0190	0.0168	0.0130	0.0444***	0.0165
Observations	8698		8698		17328		9982	
C: Cases Handled by Non-Junior Judges								
Removed	0.0842**	0.0350	0.0375*	0.0192	0.0166	0.0142	0.0405**	0.0167
Observations	9800		9800		18444		10802	
D: Excluding Non-Junior Cases								
Removed	0.0607**	0.0304	0.0353**	0.0179	0.0122	0.0146	0.0413**	0.0162
Observations	9269		9269		15937		10274	
E: First-Time Cases								
Removed	0.0753**	0.0322	0.0354**	0.0175	0.0124	0.0128	0.0382**	0.0160
Observations	9377		9377		17707		10194	
F: Cases Determined $\geq$24 Months Before Covid-19								
Removed	0.0584*	0.0301	0.0271*	0.0162	0.0184	0.0141	0.0315**	0.0141
Observations	9044		9044		15322		9082	
*G: Cases in Court*Year Cells With $\geq$10 obs*								
Removed	0.0724**	0.0309	0.0347**	0.0171	0.0154	0.0131	0.0387**	0.0152
Observations	10109		10109		19047		11106	
H: Each Judge Handles $\geq$30 Cases								
Removed	0.0733**	0.0327	0.0361*	0.0185	0.0185	0.0138	0.0400**	0.0165
Observations	9793		9793		18323		10729	
I: Excluding Judges With Top or Bottom 1% Residualized Tendency								
Removed	0.0816**	0.0390	0.0368*	0.0208	0.0175	0.0157	0.0377**	0.0157
Observations	9944		9944		18699		10970	
J: Three-Way Cluster at Case, Child, and Judge Level								
Removed	0.0719**	0.0307	0.0350*	0.0186	0.0154	0.0131	0.0383**	0.0150
Observations	10168		10168		19089		11189	
K: Court-by-Year FEs Replaced With Department-by-Year FEs								
Removed	0.0821**	0.0408	0.0433*	0.0237	0.0251	0.0177	0.0469**	0.0220
Observations	10142		10142		19064		11157	
L: Add Day-of-Week and Social Welfare Committee FEs								
Removed	0.0747**	0.0317	0.0372**	0.0167	0.0145	0.0134	0.0375**	0.0152
Observations	10156		10156		19080		11175	

Note: Panels B-I limit the baseline analysis samples to years with universal coverage (Panel B), cases handled by non-junior judges (Panel C), cases that are randomly assigned to any judge within the judge pool irrespective of the judge's seniority (Panel D), the first case for each child (Panel E), cases decided $\geq$24 months before February 2020 (Panel F), cases in court-by-year cells with at least 10 observations (Panel G), and cases handled by a judge who handles at least 30 cases during the sample period (Panel H). Panel I excludes cases handled by judges whose residualized (using court-by-year FEs) removal tendency is in the top or bottom 1% of the distribution. Panel J clusters the standard errors on the case, judge, and child level. Panel K replaces court-by-year FEs with department-by-year FEs. Panel L adds FEs for judgment day of the week and SWC. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D2. *Robustness Checks of Effects on Child Mortality II*

	Death by Year Child Turns 19				Death by Month 24 Post-Judgment			
	All-Cause		Suicide		All-Cause		Suicide	
	Coeff	Std err	Coeff	Std err	Coeff	Std err	Coeff	Std err
A: Three-Year Specific Judge Removal Tendency								
Removed	0.1797**	0.0755	0.0500	0.0321	0.0412*	0.0211	0.0423**	0.0211
Observations	6505		6505		12805		7446	
B: Leave-Out Same-Family Judge Removal Tendency								
Removed	0.0721**	0.0313	0.0350**	0.0173	0.0155	0.0133	0.0384**	0.0150
Observations	10168		10168		19089		11189	
C: Judge Removal Tendency Excl. Return Children								
Removed	0.0643*	0.0343	0.0297	0.0193	0.0150	0.0153	0.0395**	0.0166
Observations	9377		9377		17707		10194	
D: Judge Removal Tendency Excl. Cases Handled as Junior								
Removed	0.0774**	0.0349	0.0348*	0.0192	0.0112	0.0149	0.0392**	0.0167
Observations	9914		9914		18591		10897	
E: Judge Removal Tendency Excl. Non-Junior Cases								
Removed	0.0607**	0.0304	0.0404**	0.0167	0.0116	0.0137	0.0347**	0.0148
Observations	9269		9269		15937		10274	
F: Indicator for Judge Removal Tendency Above Mean								
Removed	0.0914**	0.0455	0.0576**	0.0267	0.0118	0.0191	0.0488**	0.0212
Observations	10168		10168		19089		11189	
G: Judge Removal Tendency Calculated Following Dobbie et al. (2018)								
Removed	0.0718**	0.0312	0.0348**	0.0172	0.0153	0.0131	0.0383**	0.0150
Observations	10168		10168		19089		11189	
H: Full Set of Judge Fixed Effects								
Removed	0.0381***	0.0125	0.0157**	0.0069	0.0061	0.0057	0.0132**	0.0054
Observations	10168		10168		19089		11189	
I: Estimated Using Jackknife Instrumental Variable Estimation								
Removed	0.0839***	0.0265	0.0352**	0.0143	0.0099	0.0088	0.0285**	0.0113
Observations	10168		10168		19089		11189	
J: Estimated Using Limited-Information Maximum Likelihood								
Removed	0.0719**	0.0312	0.0350**	0.0173	0.0154	0.0131	0.0383**	0.0150
Observations	10168		10168		19089		11189	

Note: Panel A defines the instrument as the judge's mean removal rate among cases handed down during the same 3-year period. Panels B-D redefine the instrument as the judge's mean removal rate excluding cases involving the same child or parent as in the focal case (Panel B); children who have been part of a case before (Panel C); cases handled while the judge held a junior position (Panel D); and non-junior cases (Panel E). Panel F replaces the instrument with an indicator for above-mean removal tendency. In Panel G, judge removal tendency is calculated by first residualizing the removal decision using court-by-year FEs (see Dobbie, Goldin, and Yang, 2018). Panel H uses a full set of judge dummies as instruments. Panel I uses jackknife instrumental variable estimation, after residualizing the outcome, removal, and the judge dummies using court-by-year FEs. Panel J uses limited-information maximum likelihood. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D3. *Effect of Removal on Child Hospitalization & Crime*

	Not Substance Use-Related			Substance Use-Related		Severity of Crime	
	(1) Hosp. d.t. Mental Health	(2) Non-Narcotic Crime	(3) Crime Against Person	(4) Hosp. d.t. Substance Use	(5) Narcotic Crime	(6) Non-Minor	(7) Minor
OLS (No Controls)							
Removed	0.0001	0.0343**	0.0146	0.0157***	0.0058	0.0308*	-0.0085
	(0.0081)	(0.0164)	(0.0132)	(0.0052)	(0.0149)	(0.0176)	(0.0137)
OLS (With Full Set of Controls)							
Removed	-0.0062	-0.0040	-0.0067	0.0017	-0.0456***	-0.0249	-0.0364***
	(0.0079)	(0.0166)	(0.0135)	(0.0059)	(0.0147)	(0.0172)	(0.0141)
OLS (Complier Reweighted)							
Removed	-0.0034	0.0088*	0.0039	-0.0011	-0.0327**	-0.0027	-0.0308**
	(0.0083)	(0.0052)	(0.0175)	(0.0147)	(0.0144)	(0.0178)	(0.0139)
RF (Only Court-by-Year FEs)							
Judge removal tendency	0.0822**	0.1853**	0.1233*	0.0202	-0.0412	0.0920	0.0144
	(0.0367)	(0.0814)	(0.0640)	(0.0306)	(0.0729)	(0.0874)	(0.0643)
IV (Only Court-by-Year FEs)							
Removed	0.2086**	0.5276**	0.3509*	0.0514	-0.1173	0.2619	0.0410
	(0.0980)	(0.2488)	(0.1919)	(0.0777)	(0.2082)	(0.2536)	(0.1831)
IV (With Full Set of Controls)							
Removed	0.1769*	0.5584**	0.3831**	0.0350	-0.1041	0.3028	0.0509
	(0.0961)	(0.2513)	(0.1938)	(0.0782)	(0.2001)	(0.2475)	(0.1856)
Sample	≥11 y.o.	≥15 y.o.	≥15 y.o.	≥11 y.o.	≥15 y.o.	≥15 y.o.	≥15 y.o.
AR *p*-value	0.0553	0.0151	0.0344	0.6541	0.6039	0.2040	0.7833
AR confidence set (95%)	[-.002,.386]	[.111,1.165]	[.038,.836]	[-.117,.193]	[-.524,.3]	[-.158,.861]	[-.324,.44]
Dependent mean	0.0630	0.1967	0.1136	0.0382	0.1389	0.2286	0.1029
Complier mean if not removed	0.0353	0.1803	0.0522	0.0556	0.1853	0.2600	0.1481
N	11139	7025	7025	11139	7025	7025	7025

Note: The '≥11 y.o. Sample' is used in columns 1 and 4 (see Section 3.3). In columns 2-3 and 5-7, I further limit the sample to children who had reached the age of criminal responsibility (15) at the time of the judgment. 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. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D4. *Effect of Removal on Parent Outcomes I*

	Death By Month 24			Hospitalization, Months 1-12		In Year t+1	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All-Cause	Suicide	Overdose	Mental Health	Substance Use	Married	No Labor Income
OLS (No Controls)							
Removed	-0.0017	0.0005	-0.0012	0.0086	0.0038	0.0171	-0.0325**
	(0.0043)	(0.0012)	(0.0018)	(0.0075)	(0.0071)	(0.0149)	(0.0140)
OLS (With Full Set of Controls)							
Removed	-0.0011	0.0003	-0.0011	0.0197**	0.0119*	-0.0144	0.0262**
	(0.0041)	(0.0013)	(0.0018)	(0.0078)	(0.0068)	(0.0119)	(0.0126)
OLS (Complier Reweighted)							
Removed	-0.0034	-0.0004	-0.0011	0.0194**	0.0116	-0.0149	0.0416***
	(0.0044)	(0.0014)	(0.0018)	(0.0085)	(0.0075)	(0.0127)	(0.0135)
RF (Only Court-by-Year FEs)							
Judge removal tendency	-0.0393	0.0044	0.0001	-0.0396	-0.0021	-0.0642	-0.0024
	(0.0260)	(0.0075)	(0.0057)	(0.0396)	(0.0346)	(0.0802)	(0.0767)
IV (Only Court-by-Year FEs)							
Removed	-0.0890	0.0101	0.0003	-0.0903	-0.0047	-0.1493	-0.0055
	(0.0602)	(0.0169)	(0.0128)	(0.0913)	(0.0789)	(0.1886)	(0.1751)
IV (With Full Set of Controls)							
Removed	-0.0877	0.0115	-0.0008	-0.0775	-0.0145	-0.1313	0.0243
	(0.0598)	(0.0170)	(0.0129)	(0.0892)	(0.0742)	(0.1437)	(0.1440)
Sample							
AR p-value	0.1355	0.4981	0.9481	0.3807	0.8451	0.3568	0.8658
AR confidence set (95%)	[-.213,.024]	[-.022,.046]	[-.027,.024]	[-.258,.096]	[-.164,.129]	[-.421,.147]	[-.255,.315]
Dependent mean	0.0169	0.0027	0.0026	0.0693	0.0613	0.4003	0.6171
Complier mean if not removed	0.0514	0.0005	0.0034	0.0387	0.0772	0.3590	0.7089
N	18557	18557	18557	18429	18429	18098	18387

Note: The 'All Ages Sample' is used (see Section 3.3). I also condition on having data on any birth parent. 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. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D5. *Effect of Removal on Parent Outcomes II*

	Crime, Months 1-12			Severity of Crime	
	(1)	(2)	(3)	(4)	(5)
	Non-Narcotic	Against Persons	Narcotic	Non-Minor	Minor
OLS (No Controls)					
Removed	-0.0166*	-0.0058	0.0023	-0.0021	-0.0012
	(0.0092)	(0.0062)	(0.0064)	(0.0099)	(0.0069)
OLS (With Full Set of Controls)					
Removed	-0.0011	0.0029	0.0072	0.0144	0.0067
	(0.0091)	(0.0064)	(0.0064)	(0.0099)	(0.0073)
OLS (Complier Reweighted)					
Removed	0.0027	0.0060	0.0074	0.0170	0.0089
	(0.0101)	(0.0068)	(0.0071)	(0.0110)	(0.0082)
RF (Only Court-by-Year FEs)					
Judge removal tendency	0.0750*	0.0585**	-0.0273	0.0458	0.0060
	(0.0433)	(0.0289)	(0.0350)	(0.0508)	(0.0366)
IV (Only Court-by-Year FEs)					
Removed	0.1708*	0.1333*	-0.0622	0.1043	0.0136
	(0.1013)	(0.0681)	(0.0802)	(0.1166)	(0.0834)
IV (With Full Set of Controls)					
Removed	0.1636*	0.1271*	-0.0700	0.0998	0.0107
	(0.0952)	(0.0659)	(0.0758)	(0.1084)	(0.0809)
Sample					
AR p-value	0.0792	0.0478	0.3519	0.3548	0.8943
AR confidence set (95%)	[-.013,.363]	[.004,.265]	[-.223,.077]	[-.111,.319]	[-.153,.168]
Dependent mean	0.0966	0.0427	0.0604	0.1354	0.0620
Complier mean if not removed	0.0439	0.0231	0.0863	0.1236	0.0655
N	18429	18429	18429	18429	18429

Note: The 'All Ages Sample' is used (see Section 3.3). I also condition on having data on any birth parent. 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. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table D6. *Predictors of Death Among Removed Children*

	Death by Year Child Turns 19
Girl	-0.0033*
	(0.0017)
	[9157]
Age at judgment	0.0001
	(0.0003)
	[9157]
Sibling case	-0.0042**
	(0.0018)
	[9157]
Foreign background	-0.0046***
	(0.0017)
	[9157]
Behavior petition	0.0038**
	(0.0018)
	[9157]
Environment petition	-0.0034**
	(0.0017)
	[9157]
Child consents to removal	0.0011
	(0.0023)
	[5691]
At least 1 parent consents to removal	0.0042*
	(0.0022)
	[5691]
Hosp. (yrs t-1 to t-3), mental health	0.0064
	(0.0045)
	[8172]
Hosp. (yrs t-1 to t-3), substance use	0.0073
	(0.0055)
	[8172]
Ever institutional care by month 6	0.0032*
	(0.0017)
	[9138]
Ever congregate care by month 6	0.0022
	(0.0017)
	[8427]
Any across-municipality move by month 6	-0.0030*
	(0.0016)
	[9138]
More than 1 placement change by month 6	0.0009
	(0.0017)
	[8427]

Note: This table reports OLS estimates of separately regressing death by the year the child turns 19 on each of the listed variables. The base sample used is all removed children in the 'Year 19 Sample'. The sample size (displayed in brackets) varies by regression since I exclude observations with missing information on the regressor of interest. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

E Heterogeneity (including MTEs)

Table E1. *Results by Placement Characteristics*

	Pr(Institution)		Pr(Instability)		Pr(New Municipality)	
	Low	High	Low	High	Low	High
A: Death by Year Child Turns 19						
Removed	0.0407**	0.0850	0.0758***	0.0286	0.0787	0.0442*
	(0.0202)	(0.0592)	(0.0293)	(0.0365)	(0.0529)	(0.0236)
Dependent mean	0.0049	0.0092	0.0071	0.0071	0.0083	0.0059
N	5081	5087	5083	5085	5087	5081
B: Death by Year Child Turns 19 (Suicide)						
Removed	0.0166	0.0668*	0.0270	0.0397*	0.0637**	0.0180
	(0.0120)	(0.0365)	(0.0172)	(0.0223)	(0.0314)	(0.0148)
Dependent mean	0.0018	0.0033	0.0026	0.0026	0.0028	0.0024
N	5081	5087	5083	5085	5087	5081
C: Death by Year Child Turns 19 (Overdose)						
Removed	0.0055	0.0284	0.0280*	-0.0053	0.0053	0.0170*
	(0.0056)	(0.0339)	(0.0143)	(0.0183)	(0.0287)	(0.0096)
Dependent mean	0.0004	0.0031	0.0018	0.0018	0.0026	0.0010
N	5081	5087	5083	5085	5087	5081
D: Death by Month 24 Post-Judgment						
Removed	0.0051	0.0186	0.0125	0.0099	0.0134	0.0045
	(0.0088)	(0.0219)	(0.0119)	(0.0184)	(0.0241)	(0.0087)
Dependent mean	0.0014	0.0049	0.0023	0.0040	0.0050	0.0013
N	9535	9554	9545	9544	9547	9542
E: Death by Month 24 Post-Judgment (Suicide)						
Removed	0.0107	0.0808**	0.0398**	0.0279	0.0652**	0.0183
	(0.0073)	(0.0364)	(0.0162)	(0.0194)	(0.0297)	(0.0121)
Dependent mean	0.0005	0.0029	0.0016	0.0018	0.0021	0.0013
N	5605	5584	5594	5595	5595	5594

Note: This table presents IV estimates of removal on child mortality. The 'Year 19 Sample' is used in Panels A-C, the 'All Ages Sample' is used in Panel D, and the '$\geq$11 y.o. Sample' is used in Panel E (see Section 3.3). I limit the samples to the subgroup specified at the top of each column. High (low) probability of institutional placement is defined as an above (below) median risk of being placed in an institutional facility in the first six months following removal. High (low) probability of placement instability is defined as an above (below) median risk of having more than one placement switch in the first six months following removal. High (low) probability of moving to a new municipality is defined as an above (below) median risk of moving to a new municipality at least one time in the first six months following removal. Predictions are made using LASSO and full sets of court-by-year FEs, SWC FEs, and child and parent characteristics listed in Table 1. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

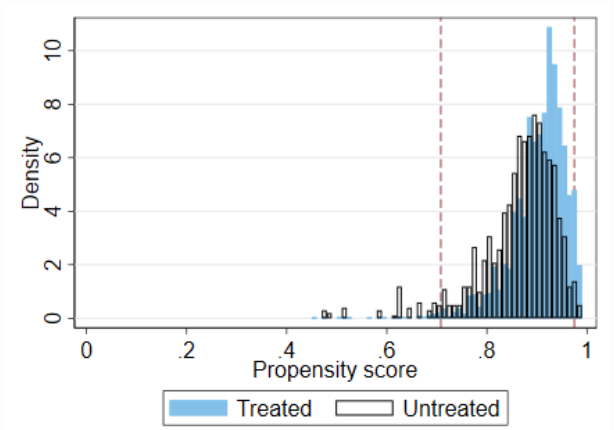
Table E2. *Heterogeneity of Effects on Child Hospitalization & Crime*

	Gender		Petition grounds		Background		Sibling Case		Age at Judgment	
	Girl	Boy	Behavior	Environ.	Foreign	Native	Yes	No	16-20 yrs	11-15 yrs
A: Hosp. d.t. Mental Health, Months 1-12										
Removed	0.2557	0.1214	0.4384	0.0577	0.2644	0.2158*	-0.1756	0.2823**	0.1867	0.1870*
	(0.1638)	(0.1070)	(0.3348)	(0.0869)	(0.1774)	(0.1219)	(0.1586)	(0.1258)	(0.1972)	(0.1006)
Dependent mean	0.0950	0.0356	0.0776	0.0374	0.0505	0.0723	0.0160	0.0714	0.0789	0.0503
N	5148	5987	5271	3932	4715	6417	1690	9439	4957	6179
B: Non-Narcotic Crime, Months 1-12										
Removed	0.2729	0.7701**	1.1983	0.0694	0.7694	0.3800	0.2312	0.5738*	0.8641***	-0.4420
	(0.3131)	(0.3724)	(0.8074)	(0.1665)	(0.5243)	(0.2899)	(0.7566)	(0.3109)	(0.3330)	(0.4301)
Dependent mean	0.1262	0.2487	0.2355	0.0702	0.1886	0.2031	0.0821	0.2041	0.2009	0.1871
N	2979	4037	4459	1368	2954	4062	402	6603	4957	2058
C: Crime Against Person, Months 1-12										
Removed	0.0793	0.5910*	0.6392	0.2143*	0.2458	0.3105	0.5522	0.3838	0.5857**	-0.3133
	(0.2111)	(0.3039)	(0.5966)	(0.1130)	(0.3671)	(0.2242)	(0.8604)	(0.2400)	(0.2476)	(0.3520)
Dependent mean	0.0628	0.1511	0.1397	0.0270	0.1124	0.1147	0.0323	0.1187	0.1118	0.1181
N	2979	4037	4459	1368	2954	4062	402	6603	4957	2058
D: Hosp. d.t. Substance Use, Months 1-12										
Removed	0.0990	0.0174	0.1443	-0.0007	-0.0282	0.0974	0.0497	0.0674	0.1816	-0.0361
	(0.1119)	(0.1074)	(0.2890)	(0.0369)	(0.1220)	(0.1032)	(0.0866)	(0.1008)	(0.1815)	(0.0624)
Dependent mean	0.0408	0.0361	0.0632	0.0084	0.0216	0.0505	0.0077	0.0438	0.0629	0.0184
N	5148	5987	5271	3932	4715	6417	1690	9439	4957	6179
E: Narcotic Crime, Months 1-12										
Removed	0.2190	-0.2428	-0.0997	0.0192	0.0424	-0.1653	-0.0896	-0.1644	0.0642	-0.4028
	(0.2190)	(0.3168)	(0.6168)	(0.0884)	(0.3834)	(0.2626)	(0.4592)	(0.2585)	(0.2632)	(0.3455)
Dependent mean	0.0611	0.1964	0.1859	0.0205	0.1117	0.1590	0.0199	0.1464	0.1580	0.0933
N	2979	4037	4459	1368	2954	4062	402	6603	4957	2058

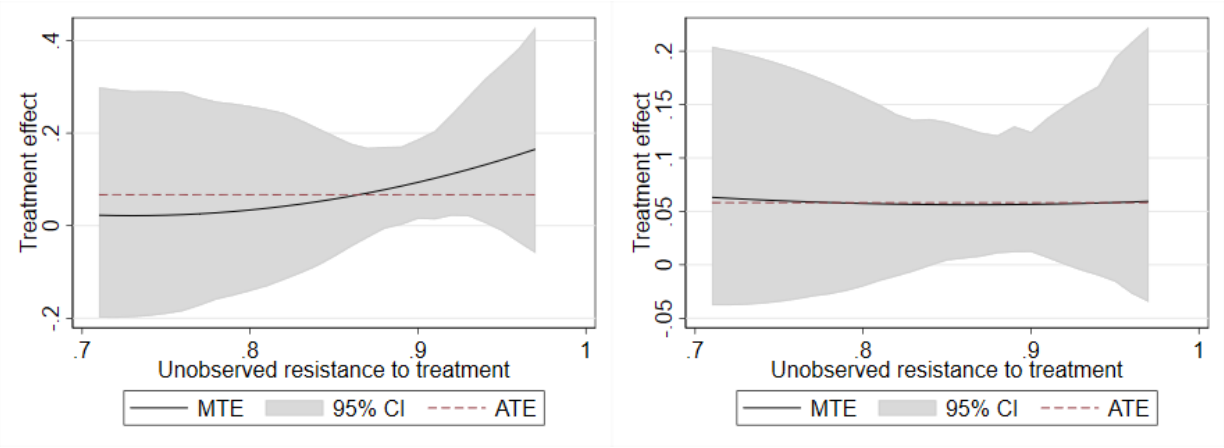
Note: This table presents IV estimates of removal on child hospitalization and crime. The '$\geq$11 y.o. Sample' is used in Panels A and D (see Section 3.3). In Panels B-C and E, I further limit the sample to children who had reached the age of criminal responsibility (15) at the time of the judgment. I also limit each sample to the subgroup specified at the top of each column. All estimations control for court-by-year FEs. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Figure E1. *Common Support and MTEs*

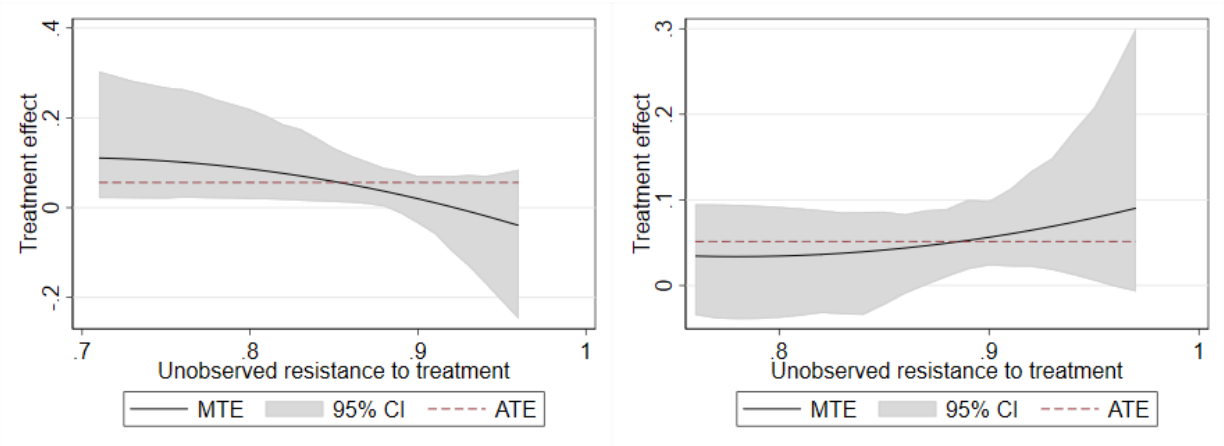
Panel A. Common Support



Panel B. Death by Year Child Turns 19, All-Cause (Left) and Suicide (Right)



Panel C. Death by Month 24, All-Cause (Left) and Suicide (Right)



Note: Panel A presents the propensity score distribution for removed and not removed children when using the 'Year 19 Sample' (distributions are very similar in the 'All Ages' and '≥ 11 y.o.' samples). Dashed vertical lines show, after trimming 1% of the sample with common support, the top and bottom scores at which there is overlap in the distribution. Panels B-C present the MTEs (black line) attained by fitting a polynomial model of degree 2 using the local IV approach and the 'Year 19 Sample' (Panel B), the 'All Ages Sample' (Panel C, left), or the '≥11 y.o. Sample' (Panel C, right). The shaded area shows 95% confidence intervals. Standard errors are generated from 300 bootstrap replications and clustered at the court-by-year level. The dashed line indicates the ATE, which is constructed as a weighted average of the MTEs.

Table E3. *Average Treatment Effects on Child Mortality (Based on MTEs)*

	(1) Linear Specification	(2) Global Quadratic	(3) Global Cubic	(4) Global Quartic
A: Death by Year Child Turns 19				
ATE	0.0730	0.0669	0.0680	0.0801
	(0.0491)	(0.0536)	(0.0591)	(0.0528)
ATT	0.0635	0.0508	0.0473	0.0496
	(0.0684)	(0.0763)	(0.0930)	(0.0722)
ATUT	0.0803	0.0899	0.1070	0.1548
	(0.0722)	(0.0658)	(0.0976)	(0.1220)
B: Death by Year Child Turns 19 (Suicide)				
ATE	0.0589**	0.0581*	0.0584*	0.0633**
	(0.0298)	(0.0317)	(0.0318)	(0.0318)
ATT	0.0628*	0.0614	0.0604	0.0613*
	(0.0350)	(0.0387)	(0.0408)	(0.0353)
ATUT	0.0301	0.0312	0.0358	0.0550
	(0.0264)	(0.0304)	(0.0386)	(0.0670)
C: Death by Month 24				
ATE	0.0530*	0.0560*	0.0553*	0.0519*
	(0.0293)	(0.0292)	(0.0289)	(0.0299)
ATT	0.0709	0.0782*	0.0809*	0.0817
	(0.0436)	(0.0453)	(0.0453)	(0.0505)
ATUT	0.0046	0.0010	-0.0104	-0.0284
	(0.0403)	(0.0387)	(0.0530)	(0.0950)
D: Death by Month 24 (Suicide)				
ATE	0.0557**	0.0514***	0.0524***	0.0477**
	(0.0232)	(0.0180)	(0.0200)	(0.0218)
ATT	0.0537**	0.0449*	0.0405	0.0368
	(0.0235)	(0.0229)	(0.0300)	(0.0272)
ATUT	0.0508*	0.0575*	0.0828	0.0521
	(0.0298)	(0.0345)	(0.1096)	(0.0828)

Note: This table presents approximations of the ATE, ATT, and ATUT of being removed from home on child all-cause mortality and suicide. The estimates are constructed as weighted averages of the MTEs. The MTEs are estimated using the 'Year 19 Sample' (Panels A-B), the 'All Ages Sample' (Panel C), and the '$\geq$11 y.o. Sample' (Panel D). As I do not have full support, the treatment effect parameter weights are rescaled to sum to 1 over the region of common support. In columns 1-4, I adopt parametric specifications with 1-4 degrees. Trimming: 1%. Standard errors are based on 300 bootstrap replications and clustered at the court-by-year level. * $p < .1$. ** $p < .05$. *** $p < .01$.

F Comparison

The adverse effects that I find are in line with those reported in 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). As discussed in Bald, Chyn, et al. (2022) and Gross and Baron (2022), there can be several reasons for the mixed findings. In this appendix, I add to these discussions.

All of the aforementioned studies are conducted in North America but not in the same state or time period. My study is conducted in a European county (Sweden) after 2000. Hence, my findings should be interpreted in light of the high health outcomes for children in Europe relative to the US (UNICEF Innocenti, 2020). In particular, children rarely die from abuse, overdoses, self-harm, or any other form of injury in Northern and Western Europe. Depending on the age group, the rates of general and injury-related deaths among children in the US are often twice as large as the rates in Northern and Western Europe (World Health Organization Mortality Database, 2022).

Europe and especially Scandinavia offer generous public services that promote care in the home environment. 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, psychiatric) health care (Robila, 2014). Residents that fall ill, have a disability, or struggle financially receive economic benefits via Sweden's strong social security system. Families in need are offered even more extensive services, such as a support family that can care for the child part-time, help with housekeeping, parent training, and a variety of 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.

Being placed in out-of-home care does not change the child's access to any social services, nor does Sweden give children in out-of-home care priority access to health care. Indeed, few European countries grant children in out-of-home care priority access to health care (Vinnerljung and Hjern, 2018).

In the US, on the other hand, out-of-home placement makes the child eligible for a host of possible services. The package of additional resources varies by state and over time. During the last decades, there have been a number of 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, for example, Head Start (an early childhood program), free school meals, Medicaid (a program providing health care coverage), and compensation for tuition, education, and training expenses.

It is plausible that the estimates reported in the US studies capture — to a varying extent — the positive effects of access to services like Head Start. Since eligibility to support services stays constant in my setting, my estimates do not pick up such effects.

Another important difference between Sweden and the US is the placement composition. While a third of children in the US stay with a relative (Children's Bureau, 2020), only 5% of the children in the 'All Ages Sample' are placed in the home of a relative at some point in the first 6 months. In addition, the use of congregate care is common throughout Europe (Whittaker et al., 2022). In particular, congregate care is about three times as common in Sweden as in the US. A number of studies report that adverse outcomes are concentrated among children placed in non-

kinship care and especially congregate care. For example, according to Anderson (2011), children in group homes are more than 7 times as likely to express suicidal thoughts as children in kinship care.

These differences in placement composition are related to differences in placement grounds. Almost half of the children in the 'Year 19 Sample' are taken into care because of their own behavior, which is rare in the US. On the other hand, I still find significant adverse effects on mortality among children removed solely because of deficiencies in the home environment.

Other reasons to expect variation in results between study settings is the rate of placement (Baron and Gross, 2022). However, during this paper's time frame (early 2000s to late 2010s), Sweden's rate of out-of-home care (voluntary and involuntary) is actually lower than the rates observed in several other Western countries (Gilbert, 2012). As noted in Section 2.2, Sweden's rate of involuntary placement is about half as large as the rate in the US. Hence, it is not evident that the difference in results between recent studies in the US and my study is driven by a Sweden-specific practice to take an excessive number of children into care.

G Data Dictionary, Sample Restrictions, and Literature Overview

Judge Variables

Judge removal tendency: I calculate judge removal tendency as the mean removal rate in all other cases handled by the same judge, leaving out the focal case.

Junior judge: =1 if the judge is junior at the time of judgment.

Female judge: =1 if the judge is female.

Judge age: Judge age in years at the time of the judgment. Measured using judge year of birth.

Outcome Variables

Death by year child turns 19: =1 if child dies before or during the year they turn 19.

Death by month t: =1 if dies before or during month t post-judgment.

Death (suicide): =1 if dies and the cause is intentional self-harm (ICD10-codes X60-X84).

Death (overdose): =1 if dies and the cause is unintentional drug or alcohol poisoning (ICD10-codes X40-X45).

Hospitalization due to mental health: =1 if hospitalized with intentional self-harm (ICD10-codes X60-X84) or a mental and behavioral disorder (ICD10-codes F2-F9) as the main cause of harm/diagnosis.

Hospitalization due to substance use: =1 if hospitalized with unintentional drug or alcohol poisoning (ICD10-codes X40-X45), mental and behavioral disorders due to psychoactive substance use (ICD10-codes F1), or alcoholic liver disease (K70) as the main cause of harm/diagnosis.

Non-narcotic crime: =1 if committed an offense under The Swedish Criminal Code. Start date of crime is used through out the paper.

Crime against person: =1 if committed an offense under Chapter 3-7, Section 5-6 of Chapter 8, or Section 1 of Chapter 17 of The Swedish Criminal Code.

Narcotic crime: =1 if committed an offense under The Swedish Penal Law on Narcotics.

Non-minor crime: =1 if committed an offense that resulted in a criminal trial. All non-minor crimes must be processed in a trial even if the perpetrator admits guilt.

Minor crime: =1 if committed a minor offense (e.g., driving under the influence) that did not result in a criminal trial.

Control Variables

Girl: =1 if the child is female.

Age at judgment: Child age in years at the time of the judgment based on child date of birth.

Sibling case: =1 if two or more children are part of the same court case.

Foreign background: =1 if the child is born in another country than Sweden or has two parents born in another country than Sweden.

Behavior case: =1 if the SWC filed the petition for child removal on the grounds that the child's own behavior poses a palpable risk to her health or development, i.e. under Section 3 of the Care of Young Persons Act.

Environment case: =1 if the SWC filed the petition for child removal on the grounds that the home environment is deficient, i.e. under Section 2 of the Care of Young Persons Act.

Double grounds: =1 if the SWC filed the petition for child removal on both grounds, i.e. under Section 2 and Section 3 of the Care of Young Persons Act.

Child consents to removal: =1 if the lawyer assigned to represent the child or the child themselves consents to child removal.

At least 1 parent consents to removal: =1 if at least one of the parents listed in the case file consents to child removal.

Case largely based on child mental health: =1 if child psychological problems (including developmental disorders) is a case topic, but not crime, addiction, prostitution, vagabonding, honor culture, or tendency to runaway.

Non-junior case type: =1 if the case falls into any of the following categories: (i) suspected physical or sexual abuse of a young child, (ii) environmental case in which the parent(s) have an intellectual or similar developmental disorder, or (iii) behavior cases in which the need for care to a large extent is based on ADHD or autism.

Committed (yrs t-1 to t-3): Crime against person: =1 if the child committed an offense under Chapter 3-7, Section 5-6 of Chapter 8, or Section 1 of Chapter 17 of The Swedish Criminal Code in any of the three calendar years prior to the judgment. Start date of crime is used.

Committed (yrs t-1 to t-3): Narcotics: =1 if the child committed an offense under The Swedish Penal Law on Narcotics in any of the three calendar years prior to the judgment.

Committed (yrs t-1 to t-3): Other crime: =1 if the child committed any offense other than crimes against person or narcotic crimes under The Swedish Criminal Code in any of the three calendar years prior to the judgment.

Hospitalized (yrs t-1 to t-3) due to: Mental health: =1 if the child was hospitalized in any of the three calendar years prior to the judgment with intentional self-harm (ICD10-codes X60-X84) or a mental and behavioral disorder (ICD10-codes F2-F9) as the main cause of harm/diagnosis.

Hospitalized (yrs t-1 to t-3) due to: Substance use: =1 if the child was hospitalized in any of the three calendar years prior to the judgment with unintentional drug or alcohol poisoning (ICD10-codes X40-X45), mental and behavioral disorders due to psychoactive substance use (ICD10-codes F1), or alcoholic liver disease (K70) as the main cause of harm/diagnosis.

Missing, yrs t-1 to t-3: =1 if data is missing for the child during any of the three calendar years prior to the judgment.

Any birth parent: Dead: =1 if any birth parent died before the judgment.

Any birth parent: <18 y.o. at birth of child: =1 if any birth parent was under the age of 18 at the time of the child's birth.

Any birth parent: Married, yr t-1: =1 if any birth parent was married at the end of the calendar year prior to the judgment.

Any birth parent: No labor income, yr t-1: =1 if any birth parent had no labor income during the full calendar year prior to the judgment.

Any birth parent: Hosp. d.t. mental health, yr t-1: =1 if any birth parent was hospitalized in the calendar year prior to the judgment with intentional self-harm (ICD10-codes X60-X84) or a mental and behavioral disorder (ICD10-codes F2-F9) as the main cause of harm/diagnosis.

Any birth parent: Hosp. d.t. substance use, yr t-1: =1 if any birth parent was hospitalized in the calendar year prior to the judgment with accidental drug or alcohol poisoning (ICD10-codes X40-X45), mental and behavioral disorders due to psychoactive substance use (ICD10-codes F1), or alcoholic liver disease (K70) as the main cause of harm/diagnosis.

Any birth parent: Any crime, yr t-1: =1 if any birth parent committed an offense under The Swedish Criminal Code or The Swedish Penal Law on Narcotics in the calendar year prior to the judgment.

Any birth parent: Missing Xs, yr t-1: =1 if data is missing for any birth parents in the calendar year prior to the judgment.

Table G1. Sample Restrictions

Description	Observations	Sample Name
Constructing Sample Used For IV Calculation		
Base sample	26,481	
Drop cases with missing information on judge removal tendency	-6,008	
Final sample	20,473	IV Calc.
Constructing 'All in Registry' Sample		
Base sample	26,481	
Drop children that I cannot observe in Statistics Sweden's register data	-1,576	
Final sample	24,905	All in Registry
Constructing 'All Ages' Sample		
Base sample	24,905	All in Registry
Drop cases with missing information on judge removal tendency	-5,689	
Drop observations in court-by-year cells containing <2 judges	-80	
Final sample	19,136	All Ages
Constructing 'Year 19' Sample		
Base sample	19,136	All Ages
Drop children who turn 19 after the end of my data (year 2022)	-8,281	
Drop children whose cases are decided during or after the year they turn 19	-642	
Drop observations in court-by-year cells containing <2 judges	-13	
Final sample	10,200	Year 19
Constructing '$\geq$11 y.o.' Sample		
Base sample	19,136	All Ages
Drop children who are younger than 11 years old at the time of the judgment	-7,919	
Drop observations in court-by-year cells containing <2 judges	-12	
Final sample	11,205	$\geq$11 y.o.

Note: The initial sample consists of all child protection judgments handed down by any Swedish court during 2010-2019, eight courts during 2005-2010, and one court during 2001-2005.

Table G2. Overview of Literature on Effects of Child Protection Interventions

Study	Setting	Base Sample	Strategy	Child Outcome	Child Effect	Parent Outcome	Parent Effect
Doyle (2007)	Illinois, US	Medicaid recipients	CPS worker IV	Crime, teen mom, labor	Adverse	.	.
Doyle (2008)	Illinois, US	Medicaid recipients	CPS worker IV	Crime	Adverse	.	.
Berger et al. (2009)	US	National Survey of Child and Adolescent Well-Being	OLS, DID, FE	Behavior problems, cognitive skills	Null	.	.
Doyle (2013)	Illinois, US	Medicaid recipients	CPS worker IV	Crime, emergency healthcare episodes	Adverse	.	.
Lindquist and Santavirta (2014)	Stockholm, Sweden	Persons born 1953 residing in Stockholm 1963	OLS	Adult crime	Adverse only for teen boys	.	.
Warburton et al. (2014)	British Columbia, Canada	Boys	CPS worker IV, Policy change IV	Education, income ass., crime	Adverse or mixed	.	.
Roberts (2018)	South Carolina, US	Substantiated cases	CPS worker IV	Education	Favorable or null	.	.
Grimon (2020)	Allegheny county, US-PA	Neglect cases	Event-study + CPS worker IV	.	.	Health service use, benefit receipt, crime	Increases mothers' health service use
Bald, Chyn, et al. (2022)	Rhode Island, US	Substantiated cases	CPS worker IV	Education	Favorable only for young girls	Crime, future CPS case	Null
Baron and Gross (2022)	Michigan, US	Public School Pupils	CPS worker IV	Crime	Favorable	Crime, future CPS case	Favorable
Gross and Baron (2022)	Michigan, US	Public School Pupils	CPS worker IV	Crime, education, future CPS case	Favorable or null	.	.
Drange et al. (2022)	Norway	Universal	Event-study, CPS unit IV	Education, health, crime, welfare, labor	Increase in health service use	.	.
Gram Cavalca et al. (2022)	Denmark	Universal	Event-study, OLS	Education, health, crime	Increase in health service use, mixed effects on education	.	.

Note: This table lists the papers on the effects of child welfare interventions.