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Court-Ordered Care

Ronja Helénsdotter

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To Helén Andersson and Pertti Karjalainen

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Introduction

With my research, I aim to inform the design of policies that target disadvantaged subpopulations, such as the mentally ill, substance abusers, and children who interact with child welfare and juvenile justice systems. There is ample correlational evidence across disciplines documenting, for example, the overrepresentation of mental and physical illness among child welfare clients, social clustering in self-harming behavior, and high rates of relapse following substance abuse treatment (Richardson et al., 2012; Deutsch and Fortin, 2015; Fontanella et al., 2015; de Andrade et al., 2019). However, there is insufficient causal evidence on research questions in this domain.

In my dissertation, I study one of the most vulnerable groups in society: children and youths who interact with the child protection system. This population fares disproportionately worse in adulthood and is at a high risk of engaging in destructive behavior, such as self-harm, substance abuse, and crime (Sariaslan et al., 2022). At the same time, it is a surprisingly large group: more than 1 in 3 U.S. children are investigated for maltreatment at some point before their 18$^{\text{th}}$ birthday (Kim et al., 2017).

In the first chapter of my dissertation, titled "Surviving Childhood: Health and Crime Effects of Removing a Child From Home", I study the effects of removing children from their homes via court order on all-cause mortality, suicide, and accidental overdose. I also examine the effects on hospitalization related to mental illness and substance abuse, criminal behavior, and a range of parent outcomes. I construct a novel data set based on court documents spanning 2001-2019 that I transcribe and link with detailed register data. The final data set contains over 26,000 child-by-case observations. To disentangle causality from correlation, I employ an instrumental variable (IV) design that capitalizes on quasi-random variation in judge assignment together with across-judge variation in the tendency to favor removal. Intuitively, such judge designs take advantage of systematic differences in judge decision-making that courtroom participants are subject to due to the 'luck' of the draw.

I find that court-ordered out-of-home placement has large adverse effects on the mortality of the marginal child. Removal increases the risk of death by the year the child turns 19 by 7 percentage points (relative to a control mean of 1%). This increase is primarily driven by suicides that occur while the removed children are still in out-of-home care. In addition, removal causes large increases in the risk of being hospitalized for mental illness and the risk of committing 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 several potential explanations for the detrimental effects on child health. Peer victimization, adverse care home conditions, and peer-to-peer spillovers appear to be important channels.

My second dissertation chapter, titled "Treated Together: Spillovers Among Youths Admitted to Residential Treatment", is focused on youths struggling with substance abuse and self-harm. Such youths are often treated in group-based programs. However, concerns have been raised about the risk of adverse outcomes through peer-to-peer spillovers (Richardson et al., 2012). Hence, I analyze the effects of peers placed in residential treatment facilities on each other's outcomes using novel data on the universe of youths (over 16,000) admitted to state-owned treatment facilities in Sweden between 2000 and 2020. To overcome the issue of nonrandom assignment of youths to facilities, I use the natural flow of youths to and from facilities within a given year by including facility-by-year fixed effects. Intuitively, this design takes advantage of randomness in who is in the facility when the youth enters. I empirically show that the central assumption – i.e., the variation in peer composition is as good as random after netting out facility-by-year fixed effects – is plausible.

I find strong evidence of reinforcing peer effects in substance abuse and self-harm. Exposing youths with a history of substance abuse to a 1-standard deviation higher share of youths with the same problem history increases the risk of experiencing adverse events (death, hospitalization, readmission, or narcotic crime) related to substance abuse during the 1-12 months after discharge by 5.6%). This effect is primarily driven by an increase in the risk of dying or being hospitalized from substance abuse.

Likewise, placing a youth with a history of self-harm in a facility with a 1-standard deviation higher share of peers with a history of self-harm increases the risk of being hospitalized or dying from self-harm during the 1-12 months after discharge by 27.3%.

As seen in the other chapters of my dissertation, the decision to remove a child from home has potentially severe consequences. Given the high stakes, it is important that decisions are fair, consistent, and based on the case's merits. However, prior studies, including my first dissertation chapter, document substantial variation in decision-making for otherwise similar cases. To enhance consistency and fairness, a common practice in government institutions is to assign high-stakes or complex cases to more experienced agents. This potential remedy relies on the assumption that experienced agents are more skilled, which is not evident, especially in settings with limited feedback.

In the third chapter of my dissertation, titled "Making Better Choices: The

Role of Learning in the Judicial System" (with Jason Baron and Joseph Doyle), we investigate the role of judges' experience in the decision to remove children from their homes and the accuracy of these decisions. To further deepen our knowledge about the causes of variation in decision-making, we also examine how judges respond to decisions made by appellate courts. The analysis is based on over 20,000 Swedish child protection court cases from 2001 to 2019, which are linked with rich register data and novel data on appellate court decisions. To isolate the role of judge experience from case selection and systematic differences between judges, we exploit the quasi-random assignment of cases to judges together with temporal variation within each judge by including court-by-year and judge fixed effects.

Our results offer several important insights. First, we find strong and robust evidence that judges become more stringent with experience, conditional on court-by-year and judge fixed effects. One more year of experience as a judge increases the probability of removal by about 1.8 percentage points (relative to a dependent mean of 88.4%). This increase in stringency with experience is entirely driven by male judges: one more year of experience increases the probability of a male judge ordering removal by approximately 3.3 percentage points. For female judges, the point estimate is close to zero and lacks statistical significance. The difference in effect size is statistically significant at the 5% level.

The behavior change is not consistent with skill improvements as children who are randomly assigned to more experienced judges are more likely to die by the year they turn 19. The lack of learning is likely rooted in the limited access to information about the consequences of the court's decision. A potential driver of the positive relationship between stringency and experience can be signals from appellate courts. Indeed, we find that judges respond to appellate courts' decisions to *reverse* the judges' previous judgment to *not* remove a child from home by increasing their stringency. However, this effect is short-term and there is no detectable effect after one month. A more likely explanation is a change in judge preferences with experience.

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Chapter 1

Surviving Childhood: Health and Crime Effects of Removing a Child From Home

Ronja Helénsdotter[1]

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]University of Gothenburg, Department of Economics, Vasagatan 1, SE 405 30, Gothenburg. E-mail: ronja.helensdotter@economics.gu.se. I am grateful for the invaluable advice, feedback, and support of my supervisors Randi Hjalmarsson and Andreea Mitrut. I thank David Autor; Jason Baron; Mitchell Downey; Joseph J. Doyle, Jr.; Andreas Dzemski; Amy Finkelstein; Marie-Pascale Grimon; Emily Leslie; Mikael Lindahl; Matthew Lindquist; and the participants and discussants at Copenhagen Business School, Duke University, EALE, MIT, Stockholm University, Texas Economics of Crime Workshop, The Ragnar Frisch Centre for Economic Research, The Stockholm Health Day, and University of Gothenburg for many helpful comments and suggestions. I gratefully acknowledge funding support from Vetenskapsrådet, Herbert & Karin Jacobssons Stiftelse, Kungl. och Hvitfeldtska stiftelsen, and Stiftelsen Lars Hiertas Minne. The material and data provided by the administrative courts of Sweden, The Swedish National Archives, Stockholm City Archive, The National Courts Administration, Statistics Sweden, The National Board of Health and Welfare, The National Board of Institutional Care, and The National Council for Crime Prevention made this paper possible. This research has been approved by the Swedish Ethical Review Authority.

1.1 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).[2,3] 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.[4] 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 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.[5]

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).[6] 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 place-

[2] 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.

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

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

[5] 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.

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

ment 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).

Using IV analysis, I find that out-of-home placement has significant adverse effects on the mortality of the marginal child.[7] 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 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.[8] 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).[9] In Appendix 1.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

[7] 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.

[8] 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.

[9] 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.

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), 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.[10]

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.[11] 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.[12] 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.[13] In Appendix 1.F, I elaborate on how the European setting differs from the settings considered in prior studies.

[10]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 1.2.2 and Appendix 1.F.

[11]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).

The paper proceeds as follows. Section 1.2 presents the institutional background and a cross-country comparison. Section 1.3 describes the data. Section 1.4 outlines the IV model and discusses the validity of the assumptions. Effects on child mortality are presented in Section 1.5 while effects on other short-term outcomes are presented in Section 1.6. Section 1.7 probes possible mechanisms. Section 1.8 concludes.

1.2 Institutional Background

1.2.1 Child Protection System in Sweden

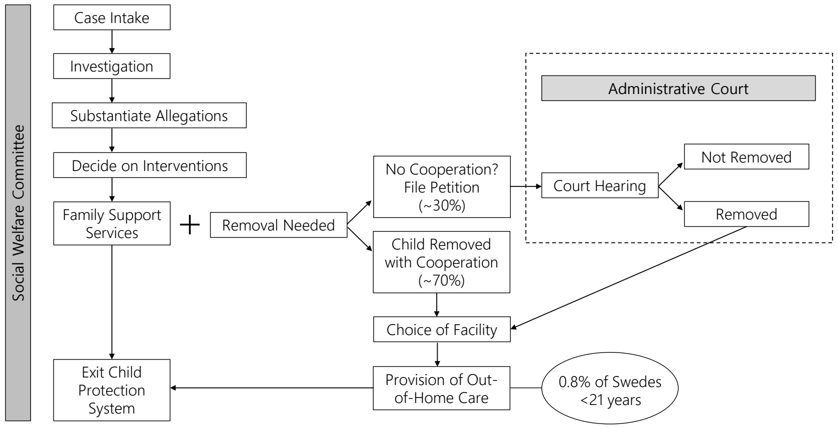
Figure 1.1 provides a representation of the child protection process in Sweden. The local SWC (*socialnämnden*) is responsible for child protection.[14] This responsibility is broad and encompasses, e.g., preventive work, maltreatment investigations, evaluation of service need, and service provision. However, the SWC does not have the authority to take children into care without the consent of the

[12]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.

[13]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 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.

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

Figure 1.1. *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.

caregivers and the child.[15] When no consent can be attained, the SWC files a petition with one of Sweden's 12 administrative courts.[16]

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.[17] The case is then automatically assigned to a department within the court according to a rotating system.[18] 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.[19] Fortunately, junior judges only make up 3% of my analysis samples and the results are robust to excluding these judges and cases that are typically not assigned to junior judges.[20]

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).[21]

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

[15] 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.

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

[17] 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.

[18] 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.

[19] 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.

[20] 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.

[21] 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.

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).[22]

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.[23,24] Hence, there is only one judiciary outcome.

If the court does not rule in favor of out-of-home placement, the child cannot be removed from home. The SWC must then continue to offer support services (e.g., a support family that can care for the child part-time) but the family can decline such services.[25]

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

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

[23]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.

[24]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).

[25]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.

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.[26] 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.[27] 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).[28]

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

[26]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.

[27]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).

[28]These calculations are based on a register known to be subject to underreporting (see Section 1.3.1).

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.[29]

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 1.F for more comparative statistics and institutional details.

1.3 Data

1.3.1 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 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 1.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.[30] 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.[31]

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 law) from the National Council for Crime Prevention (1997-2021).[32] See Appendix 1.G for variable definitions.

1.3.2 Judge Removal Tendency

As described in Section 1.4, 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

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

[30] 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.

[31] 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.

[32] 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.

in the focal case:

$$Z_{j(c)} = \frac{1}{n_j - n_{j(c)}} \left(\sum^{n_j} R_{j(i)} - \sum^{n_{j(c)}} R_{j(i)} \right), \tag{1.1}$$

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.[33] The results are robust to changes in instrument construction, including the use of a higher cutoff for the number of cases per judge.

1.3.3 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 all-cause mortality in the months following the court's judgment and refer to it as the 'All Ages Sample'.[34]

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'.

[33]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).

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

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.[35] This sample (N=11,205) is referred to as the '$\geq$11 y.o. Sample'.

Table 1.1 displays descriptive statistics at the child and birth parent level (Panel A) and judge level (Panel B) for each analysis sample.[36] 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 1.1, which is the first piece of evidence supporting random assignment.[37,38]

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

Figure 1.2 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.[39] Probabilities for removed and non-removed

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

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

[37]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).

[38]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).

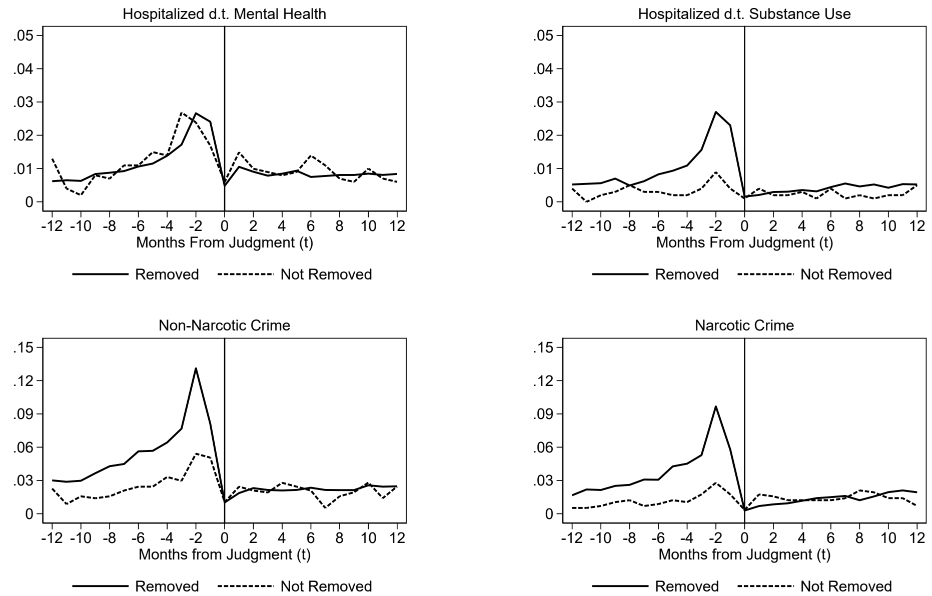
[39]The date of the crime, rather than the date of conviction or reporting, is used for crime out-

Table 1.1. *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 1.3.3. Statistics are shown for observations with non-missing information.

Figure 1.2. *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.

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 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 1.2 illustrates that it is difficult to use event studies to estimate the causal effects of removal in this context.

comes. 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.

1.4 Empirical Methodology

1.4.1 Instrumental Variable Model

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

$$Y_{i,c,t} = \beta R_{i,c,t} + X'_{i,c,t}\theta + \eta_{i,c,t}, \tag{1.2}$$

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 1.3.2, judge removal tendency is measured as the leave-out mean removal rate. The first-stage equation in the 2SLS model is:

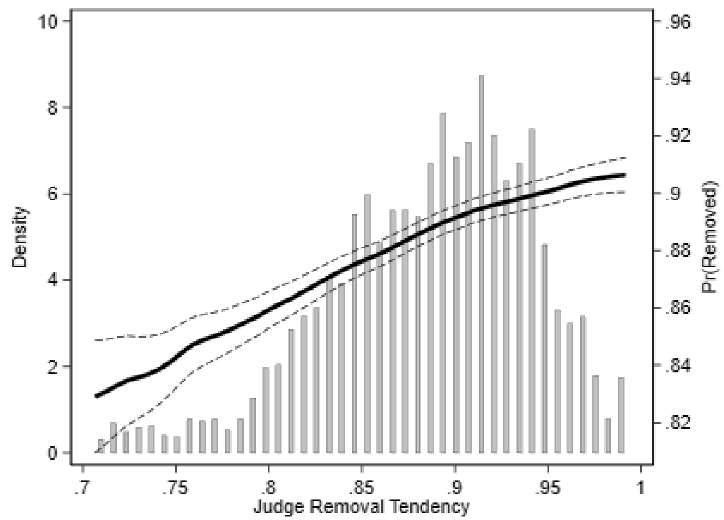
$$R_{i,c,t} = \pi Z_{j(c)} + \alpha_{h,t} + \epsilon_{i,c,t}, \tag{1.3}$$

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 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.

Figure 1.3. *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 1.3.3). 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.

1.4.2 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 1.3 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 1.3, 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 1.2. 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 1.1, Panel A). Irrespective of the analysis sample and whether extra controls are added, the estimated coefficient is large, positive, and highly significant with an effective F-statistic around 50-70.[40] 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.[41]

Table 1.2. *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 1.1 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.

1.4.3 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 1.2, judges are expected to be assigned to cases quasi-randomly (conditional on observable controls) given the features of the institutional setting. Table 1.3 provides strong empirical evidence that judges are ran-

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

[41] 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 1.3. *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. * $p < .1$. ** $p < .05$. *** $p < .01$.

domly assigned, conditional on court-by-year FEs. The first column regresses removal on 25 background variables. Important predictors of removal 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.

1.4.4 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 1.2, 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 1.4.5) is provided by Frandsen et al. (2023).[42] 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.[43]

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

[42] 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.

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

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).

1.4.5 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 1.4.4, 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.

1.5 Results for Child Mortality

1.5.1 Baseline Results

Table 1.4 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 1.1, the sample sizes are slightly smaller because of sample attrition stemming from emigration.[44]

As shown in the last column of Table 1.4, 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. 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,[45] which suggests that the difference in estimates is not driven by effect heterogeneity.

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.

[44]See Appendix 1.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). 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).

[45]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 1.1. 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.

Table 1.4. *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	$\geq$11 y.o.	$\geq$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 '$\geq$11 y.o. Sample', respectively. Each sample is described in Section 1.3.3. 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 1.1. Reported AR *p*-values and confidence sets are for *IV (Only Court-by-Year FEs)*. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

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 over-representation 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 1.4, removal increases the risk of the marginal child dying by the year they turn 19 by over 7 percentage points (significant at the 5% 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.[46] 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.[47]

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.[48] 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.[49] 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 1.3) and the first-stage coefficient is around 0.4.[50] However, the IV estimate extrapolates the induced 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 1.4 also provides reduced-form (RF) estimates.[51] 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 conven-

[46] 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).

[47] In Appendix 1.F, I discuss reasons for why my findings contrast with recent findings reported in studies conducted in the US.

[48] 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 $(0.0156*0.8+0.0875*0.2)*0.1356*10168\approx41$ deaths among compliers and 31 deaths among non-compliers. According to my point estimates, only $0.0156*0.1356*10168\approx22$ 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.

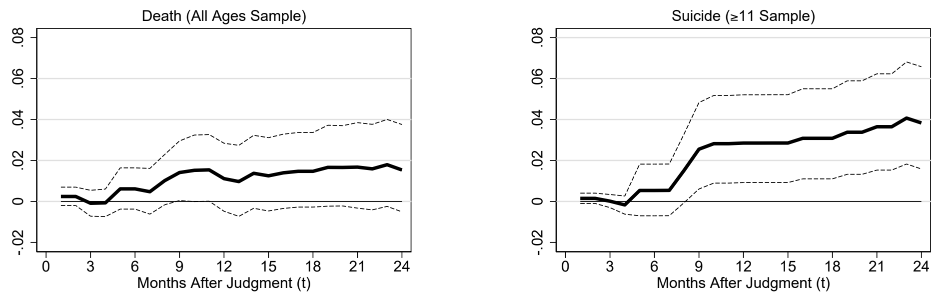
[49] The legal mandate to place children with mental health problems in out-of-home care has been discussed, changed, and clarified over the last two decades in several official reports, government bills, and rulings (e.g., Swedish Government, 2002;SOU, 1998:31, 2000:77). According to the Supreme Administrative 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.

[50] The range of variation in the instrument in Aizer and Doyle (2015) is 12 percentage points, while it is around 25 percentage points in Bhuller et al. (2020).

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

tional levels (Table 1.4, 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.[52]

Figure 1.4. *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 1.4 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 '$\geq$11 y.o. Sample', the estimates become significant (5% level) already by month 9 and remain steady for the subsequent months.

1.5.2 Heterogeneity

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

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

together, I can split the sample by whether siblings are part of the same court case.

Results by subgroups are presented in Table 1.5. 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.

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 1.5. *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 1.3.3). 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. * $p < .1$. ** $p < .05$. *** $p < .01$.

1.5.3 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.

1.6 Effects on Other Outcomes

1.6.1 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.

Table 1.6. *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 1.3.3). 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 1.1. 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. * $p < .1$. ** $p < .05$. *** $p < .01$.

As shown in the first column of Table 1.6, removal increases the risk of the marginal child being hospitalized for mental illness within the first year by around 20 percentage points (significant at the 5-10% level).[53] 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).[54] 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 1.5.1), especially in light of the large confidence sets.[55]

Turning to substance use-related outcomes (columns 4-5 of Table 1.6), the IV estimates are not statistically significant, which is in line with the non-significant effect found on overdose by month 24.

The outcomes used in Table 1.6 condition on the child surviving and never emigrating during the first year. Figure 1.5 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.[56] The next kink at month 8 coincides with the steep rise in suicides (which results in these children

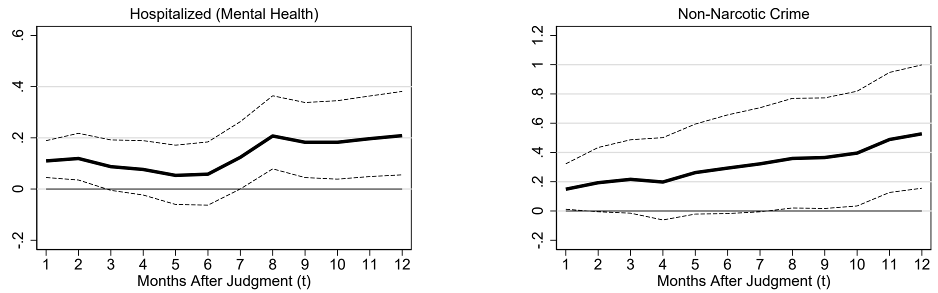
[53]OLS and reduced-form estimates are provided in Table D3. I also present estimated effects on the likelihood of committing a minor versus non-minor crime. I define minor crimes as those that do not result in a criminal trial. Note that all non-minor crimes must be processed in a trial even if the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

[54]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.

[55]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 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≈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.

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

Figure 1.5. *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.

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 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).

1.6.2 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.

1.7 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.

1.7.1 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.[57]

[57] 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

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).[58,59] 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.

1.7.2 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).[60] 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

et al., 2019).

[58]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).

[59]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).

[60]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).

within the first 6 months following the court's judgment.[61] 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 1.1, 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 placement instability (Table E1).[62,63]

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 1.3.1). 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.

1.7.3 Peers

As I show in Section 1.6, 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.

[61]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.

[62]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.

[63]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.

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.

1.7.4 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 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).[64] 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.

[64]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.

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.[65] 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, 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).

1.7.5 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%

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

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$^{\text{th}}$ 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$^{\text{th}}$ 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.[66] 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 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 18$^{\text{th}}$ birthday.[67] 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.

[66]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.

[67]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.

1.8 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 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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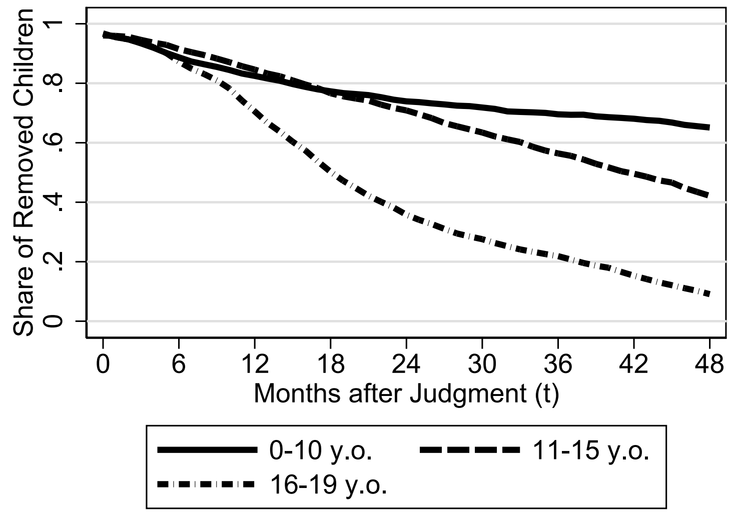
Appendix Tables and Figures

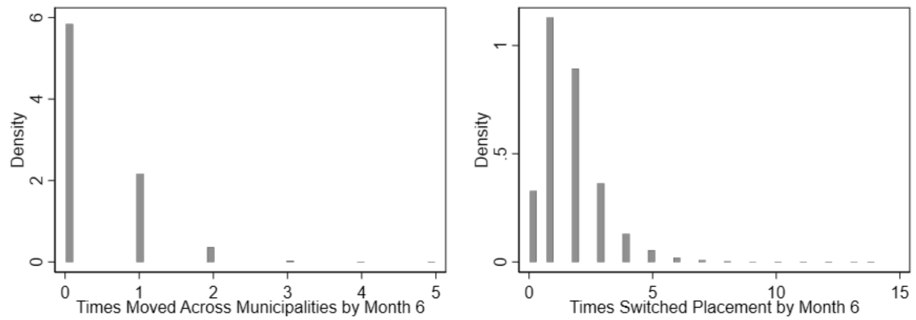
1.A Descriptive Statistics

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



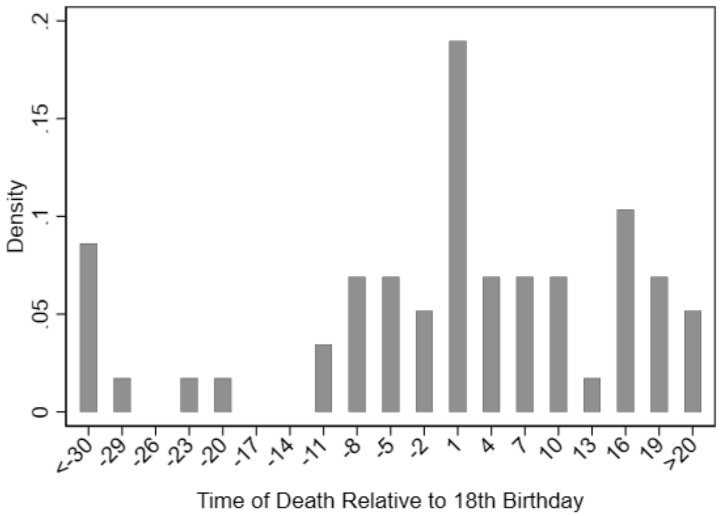
Note: This figure shows the share of children (by age at judgment) still in out-of-home care for any number of days during month t post-judgment in the 'All Ages Sample' conditional on the child (i) being removed from home and (ii) existing in the placement data on any day in the judgment month ± 1 month.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

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 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.

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.

1.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.[68] 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.

[68]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 B1. *Test of Selective Attrition*

		Missing Information on...				
	(1)	(2)	(3)	(4)	(5)	(6)
	No Personal Identity Number	Death by Year 19	Death by Month 24	Death by Month 24	Hospital-ization Months 1-12	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	$\geq$11 y.o.	$\geq$11 y.o.	$\geq$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. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table B2. *Descriptive Statistics Excluding Attriters*

	Year 19	Month 24		Month 12	
	Mortality	Mortality	Mortality	Hospital-ization	Crime
<u>*A: Child & Parent Characteristics*</u>					
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
≥1 parent consents to removal	0.52	0.36	0.48	0.49	0.57
Case 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
<u>*B: Judge Characteristics*</u>					
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	≥11 y.o.	≥11 y.o.	≥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 1.3.3) 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 '≥11 y.o.' and '≥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
<u>*OLS (No Controls)*</u>						
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)
<u>*OLS (With Full Set of Controls)*</u>						
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)
<u>*OLS (Complier Reweighted)*</u>						
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)
<u>*RF (Only Court-by-Year FEs)*</u>						
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)
<u>*IV (Only Court-by-Year FEs)*</u>						
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)
<u>*IV (With Full Set of Controls)*</u>						
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 1.4 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. * $p < .1$. ** $p < .05$. *** $p < .01$.

1.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 1.1 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 1.4 (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 1.1 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 1.1) 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 1.4 (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 '≥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 1.3.3) 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*

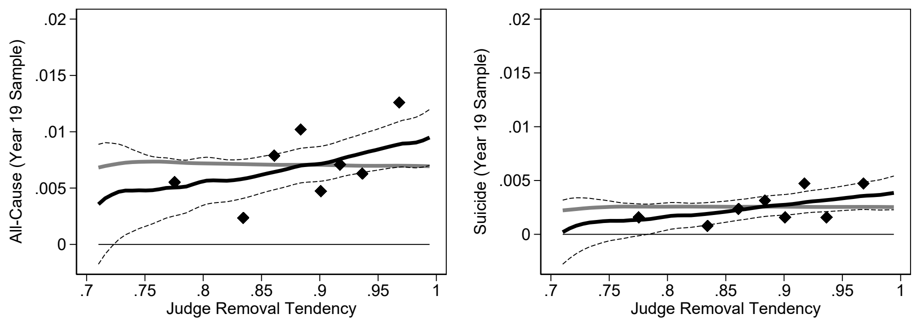
	(1) Behavior	(2) Environment	(3) 0-10 years	(4) 11-15 years	(5) 16-20 years	(6) Native Background	(7) Foreign Background	(8) Girl	(9) Boy
A: Main Measure of Judge Removal Tendency									
Judge removal tendency	0.2039***	0.5172***	0.4728***	0.4238***	0.3386***	0.4568***	0.3871***	0.4733***	0.3954***
	(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. * $p < .1$. ** $p < .05$. *** $p < .01$.

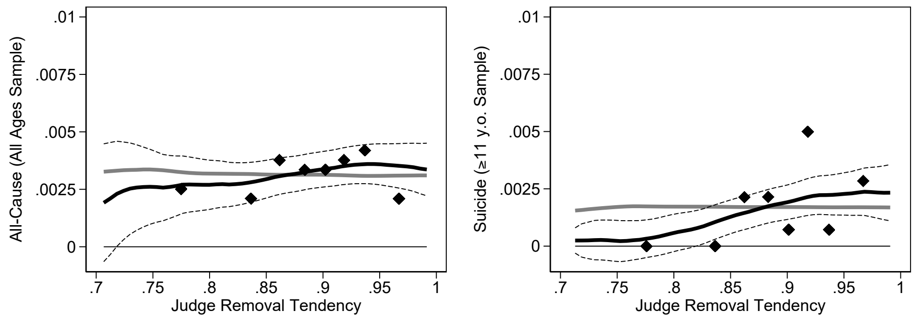
1.D Results

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 1.1) on judge removal tendency. The sample used is indicated on the y-axis title (see Section 1.3.3 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 ≥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 ≥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 ≥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 ≥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 1.3.3). 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 1.1. 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) All-Cause	(2) Suicide	(3) Overdose	(4) Mental Health	(5) Substance Use	(6) Married	(7) 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 1.3.3). 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 1.1. 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) Non-Narcotic	(2) Against Persons	(3) Narcotic	(4) Non-Minor	(5) 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 1.3.3). 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 1.1. 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. * $p < .1$. ** $p < .05$. *** $p < .01$.

1.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

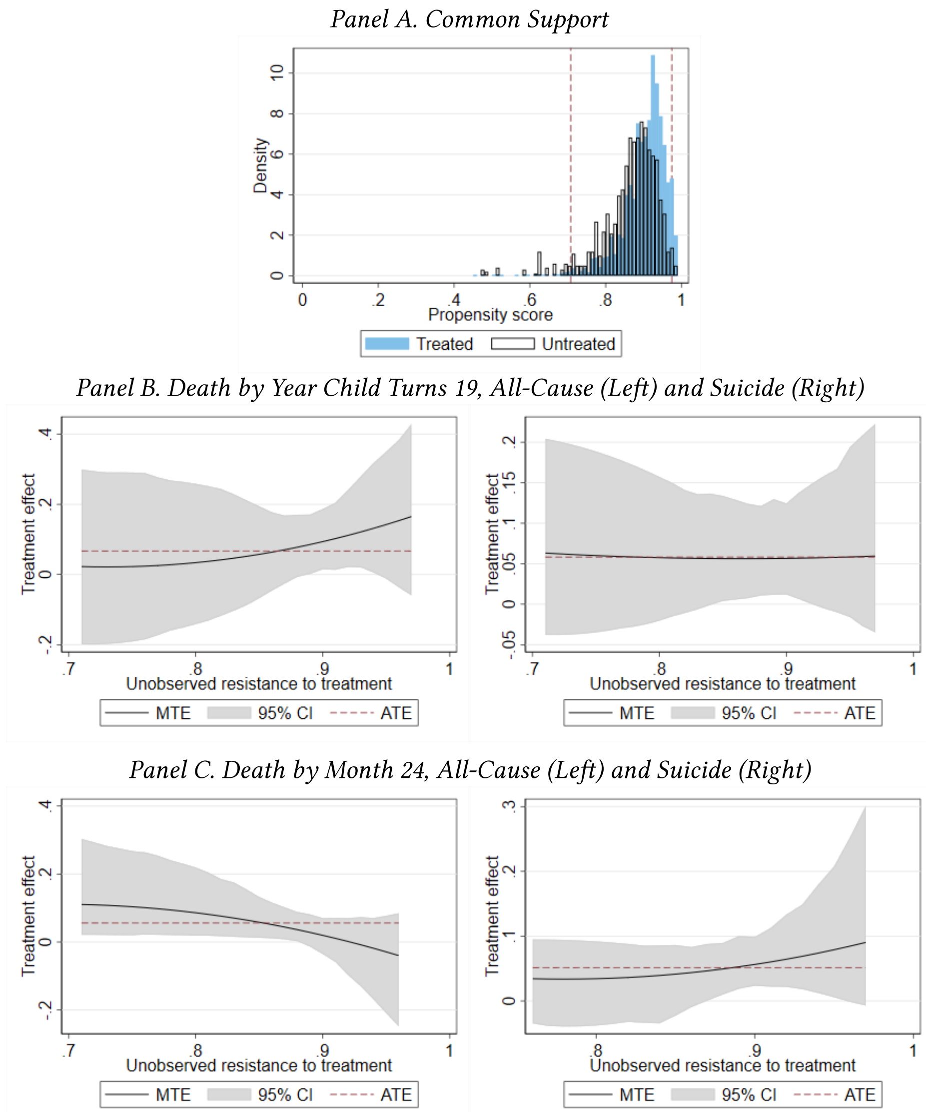
Note: This table presents IV estimates of removal on child mortality. The 'Year 19 Sample' is used in Panels A-C, the 'All Ages Sample' is used in Panel D, and the '≥11 y.o. Sample' is used in Panel E (see Section 1.3.3). I limit the samples to the subgroup specified at the top of each column. High (low) 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 1.1. 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 1.3.3). 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*



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 '$\geq$ 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 '$\geq$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. * $p < .1$. ** $p < .05$. *** $p < .01$.

1.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 1.2.2, 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.

1.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 '≥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	≥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.

Chapter 2

Treated Together: Spillovers Among Youths Admitted to Residential Treatment

Ronja Helénsdotter[1]

Individuals struggling with substance abuse and self-harm are often treated in group-based programs. However, concerns have been raised about the risk of adverse outcomes through peer-to-peer spillovers. This paper analyses the effects of peers placed in residential treatment facilities on each other's outcomes. I use novel data on the universe of youths (over 16,000) admitted to state-owned treatment facilities in Sweden between 2000 and 2020. To overcome the issue of nonrandom assignment of youths, I make use of the natural flow of youths to and from facilities within a given year by including facility-by-year fixed effects. I find strong evidence of reinforcing peer effects in substance abuse and self-harm: exposing youths with a history of substance abuse (self-harm) to peers with a similar background increases the risk of experiencing adverse events (for example, hospitalization) related to substance abuse (self-harm) post-discharge.

[1]University of Gothenburg, Department of Economics, Vasagatan 1, SE 405 30, Gothenburg. E-mail: ronja.helensdotter@economics.gu.se. I am thankful to my supervisors, Randi Hjalmarsson and Andreea Mitrut, for their valuable guidance. I thank Jason Baron, Joseph Doyle, Andreas Dzemski, Matthew Lindquist, Kevin Schnepel, and the seminar participants at the University of Gothenburg for many helpful comments and suggestions. Funding from Herbert & Karin Jacobssons Stiftelse, Kungl. och Hvitfeldtska stiftelsen, and Stiftelsen Lars Hiertas Minne is gratefully acknowledged. The material and data provided by Statistics Sweden, The National Board of Health and Welfare, The National Board of Institutional Care, and The National Council for Crime Prevention made this paper possible. This research is approved by the Swedish Ethical Review Authority.

2.1 Introduction

Youth suicide rates and, especially, drug overdose rates have risen dramatically during recent decades in many Western countries. In the US, the rate of drug overdose among youths (ages 15-25) has more than quintupled since 2001 while the rate of suicide has increased by 46%, resulting in 8,000 unintentional overdoses and 7,000 suicides in 2020 (CDC, 2023). Major predictors of overdose and suicide are nonlethal substance abuse and self-harm (Hawton et al., 2003; Degenhardt and Hall, 2012).[2] People struggling with substance abuse and self-harm are often treated in group-based programs. However, the cost-effectiveness, efficacy, and safety of group programs have been questioned (Beetham et al., 2020; Wakeman et al., 2020). In particular, high and long exposure to individuals who engage in the same type of behavior may have detrimental effects on the recovery process (Taiminen et al., 1998; Richardson et al., 2012). But, due to the challenges with estimating peer effects, little is known about the presence and character of such effects.

In this paper, I exploit novel Swedish data on youths (under age 21) placed together in residential treatment facilities to estimate the effects of exposure to peers with a history of substance abuse and self-harm on the risk of experiencing adverse outcomes related to substance abuse and self-harm post-placement (as measured by death, hospitalization, re-institutionalization, and narcotic crime). To identify youths with a history of substance abuse and self-harm, I use register data on hospitalizations and narcotic crimes that occur strictly before placement and information on the reason for institutionalization (as stated on the Swedish child protection authority's application for placement).[3] Hence, I only capture behaviors that are serious enough to warrant hospitalization, prosecution, or institutionalization.[4] I also consider other peer characteristics: crime, mental disorders, gender, foreign background, and age.

Estimating peer effects is empirically difficult (Manski, 1993). In most nat-

[2]There is a lack of consensus on the definitions of substance use, misuse, abuse, and dependence (Mahmoud et al., 2017). In Sweden, youths can be legally institutionalized if their use of addictive substances (alcohol, drugs, or other addictive substances) poses a significant risk to their health or development. I use the term substance abuse to denote such use. Substance use disorders are included in the term substance abuse but substance abuse is not limited to substance use disorders. Rare use of a substance (e.g., alcohol) is typically not considered youth substance abuse in the context of this paper unless the substance is highly dangerous (e.g., amphetamine). I use the term self-harm to denote self-inflicted poisonings and injuries.

[3]The reason stated on the placement application cannot be changed once the application has been approved. The application must be approved before the youth arrives at a facility.

[4]Physicians only hospitalize patients with severe injuries or illnesses that cannot wait or be treated within the Swedish primary care system. Even if a youth accidentally harms themselves while being under the influence, my measure of substance abuse would not capture the incident since I only include hospitalizations in which substance use or self-harm is registered as the main cause of illness or harm (primarily overdoses, substance use disorder, and severe acts of self-harm).

ural settings, individuals select into peer groups. If similar individuals tend to interact with one another, allowing for self-selection inflates estimated peer effects. In addition, the environments in which peer groups operate might vary across groups, thereby creating within-group correlation in unobservables. It is especially challenging to identify the endogenous social effect of peer behavior on the individual due to the simultaneity of individual and peer behavior.[5]

With these challenges in mind, the setting considered in this paper lends itself well to the study of peer effects. Specifically, I use data on the universe of individuals admitted to residential youth facilities managed by the Swedish National Board of Institutional Care (NBIC). These youths are placed in the care of NBIC because they engage in substance abuse, criminal behavior, or some other destructive behavior that threatens their health or development. Assignment of youths to facilities is done by a central placement unit based on the availability of beds and observable characteristics.[6] These facilities are miles apart, meaning that across-facility contact is limited. Within facilities, youths spend large amounts of time together (in the common rooms, fenced-in yard, on-site school, therapy sessions, etc.), partly because they are not free to leave the premises.[7]

The facilities youths are assigned to are different from each other in terms of, e.g., target group, treatment portfolio, and staff education. To avoid within-group correlation in unobservables, I apply the identification strategy used originally in Bayer et al. (2009) and replicated by Stevenson (2017) and Damm and Gorinas (2020), which relies on the natural variation in the peer group generated by the continuous entry and exit of individuals from the group. Specifically, I adopt a fixed effects (FEs) approach to exploit variation in peer composition within facility-by-year cells.

In my main specification, I define each peer measure as the share of *other* youths with a pre-placement history of h (e.g., substance abuse) on the first day of the youth's first placement spell.[8] Guided by previous research showing reinforcing peer effects in crime and substance use (Kremer and Levy, 2008; Bayer et al., 2009), I estimate separate effects for youths with versus without a history of the behavior of interest.[9] The central assumption is that the variation in first-day peer characteristics is as good as random after netting out facility-by-year FEs. Through a series of tests, I show that this assumption is empirically valid.

There is ample correlational evidence that exposure to substance abuse and self-harm (via peers, family members, and the media) are important predictors of own substance abuse and self-harm (Hawton et al., 2012; Costello et al., 2021). In particular, studies in psychology and sociology document that segregation of youths who engage in problematic behaviors in special education, community, and treatment programs appears to exacerbate problematic behavior (Dishion and Tipsord, 2011). Potential mechanisms that have been suggested include social learning, social identification, and conformity with a perceived social norm (Jarvi et al., 2013; Neighbors et al., 2013; Martínez et al., 2023).[10]

I find strong evidence of peer effects in substance abuse and self-harm among youths with a prior history of the *same* behavior. Specifically, placing a youth with a pre-placement history of substance abuse in a facility with a 1-standard deviation higher share of youths with the same problem history increases (5% significance level) the risk of experiencing adverse events (death, hospitalization, readmission, or narcotic crime) related to substance abuse during the 1-12 months after discharge by 2.7 percentage points (relative to a mean of (48.3%). This effect is primarily driven by an increase in the risk of dying or being hospitalized from substance abuse.

Similarly, placing a youth with a history of self-harm in a facility with a 1-standard deviation higher share of peers with a history of self-harm increases (1-5% level) the risk of being hospitalized or dying from self-harm during the 1-12 months after discharge by 4.1 percentage points (mean: 15.0%). This increase is primarily driven by a rise in hospitalizations from self-harm. The results are robust to alternative specifications and samples.

Among youths with no history of substance abuse or self-harm, there is little

[5]I only consider own and peer characteristics measured before social interaction commences (i.e. exogenous peer effects). For identification of endogenous peer effects, see Lee (2007), Bramoullé et al. (2009), and Blume et al. (2015).

[6]Assignment should be done immediately upon receiving a placement application from the Swedish social services. The placement unit takes into consideration (in falling order of importance): gender, availability, special needs, security class, age, group composition, and home region (see Section 2.2.1).

[7]Naturally, there is self-selection into how much and with whom the youth chooses to interact in the facility. I abstract from this within-facility behavior and estimate the impact of potential peer composition.

[8]A placement spell is defined as an uninterrupted placement at NBIC but can include periods of isolation and facility transfers. I use the peer composition on the first day because total peer exposure is a function of placement length and future peer composition, which can depend on unobservable characteristics of the focal youth and their peers (see Section 2.3).

[9]There are several reasons to expect that peer effects for youths with versus without a history of the behavior of interest differ in magnitude. For example, there is variation in youth's predisposition to different types of behaviors and susceptibility to peer influence in that behavior. Specific genes, underlying disorders, and personality traits can be important risk factors for developing one particular behavior but not another (Brown, 2002; Laukkanen et al., 2009; Allen et al., 2012; Hilt and Hamm, 2014). At the same time, youths placed at NBIC are on average 16 years old at the time of first placement and have typically already been heavily exposed to peers who engage in a host of problematic behaviors via pre-placement networks. It is possible that youths who have not yet started to engage in behavior h before institutionalization are not predisposed to behavior h and not susceptible to peer influence in that regard. Another reason to expect reinforcing, but not introductory, peer effects is within-facility clustering of youths by behavior history. If youths who engage in the same behavior tend to interact more with each other, the exposure to other youths placed at the same facility might be low.

[10]Examples of social learning are learning about more potent combinations of drugs and more effective ways to self-harm with available material by observing peers. An example of social identification is identifying with someone who abuses substances and imitation of that behavior.

evidence of adverse peer effects. However, the outcomes I consider are extreme (death, hospitalization, re-institutionalization, and crime). If there are introductory peer effects, it might take several years before the behavior has progressed to the point where I can observe an effect on these extreme outcomes.

I analyze heterogeneity by youth characteristics (gender, foreign background, and age) and placement characteristics (peer group size, crowdedness, and placement in the youth's home county). The analysis reveals reinforcing (but at times imprecisely estimated) peer effects in substance abuse and self-harm in all subsamples and few statistically significant differences in effect size.

What are the drivers of these peer effects in substance abuse and self-harm? Youths with a history of these behaviors can influence their peers' outcomes through, for example, learning, imitation, hampering treatment provision, and spreading mental illness. I find evidence suggesting that direct exposure to substance abuse and self-harm incidents are important for the main results. In particular, there is evidence of adverse peer effects already during placement and considerable clustering in incidents. Among youths who are ever hospitalized for self-harm or substance abuse during placement at NBIC, almost 1 in 4 are hospitalized in the same month as their peer is hospitalized for the *same* reason. I also find some evidence that an increased share of peers with a history of substance abuse leads to disruptions of treatment provision through increased time spent on the run from NBIC.

While I cannot rule out any alternative mechanisms, I find no evidence that the results are driven by a higher share of resource-demanding peers, depressed peers, or peers who have other mental disorders. Neither do I find much support for an expanded network of dealers, increased drug availability, or social learning being major drivers of the main findings.

The bulk of credible studies on peer substance use consider self-reported drinking, smoking, and occasional marijuana use among students (e.g., Duncan et al., 2005; Kremer and Levy, 2008; Card and Giuliano, 2013).[11] Only three papers include heavy drugs (Gaviria and Raphael, 2001; Lundborg, 2006; Eisenberg et al., 2014), again using surveys in which students are asked whether they have ever used different substances during specified time periods. Most studies tend to find evidence of positive peer effects, but sometimes only in specific subsamples or substances.

To the best of my knowledge, there is no paper that identifies peer effects in substance *abuse*, i.e. use of addictive substances that significantly harms the individual's health or development (e.g., alcoholism). Even if there are peer effects in, for example, occasional binge drinking with friends, it is not evident that there are meaningful peer effects in alcoholism as the underlying mechanisms and importance of biological, environmental, and psychological risk factors are likely different (Tarter and Vanyukov, 2001). In addition, even if a peer effect in alcoholism exists among friends, it might not exist in treatment programs where staff can interrupt harmful peer interactions. At the same time, knowledge about peer-to-peer spillovers in substance abuse in treatment programs is particularly policy-relevant as it is important for treatment design. By using administrative data on death, institutionalization, hospitalization, and narcotic crimes, I shed light on how exposure to peers with a history of substance abuse affects own outcomes and add to the growing economic literature on the determinants of harmful substance use (e.g., Powell et al., 2018; Ruhm, 2019; Alpert et al., 2022).

Eisenberg et al. (2014) is the only other study on the causal effects of exposure to self-harming peers that I am aware of. Using survey data from college students, they do not find any evidence of peer effects in suicidal ideation or non-suicidal self-injury.[12] A related literature is the small line of work on causal peer effects in mental well-being. In line with the literature on peer effects in substance use, these studies are typically conducted in school environments using survey data (Aizer, 2009; Golberstein et al., 2016; Zhang, 2019; Giulietti et al., 2022; Kiessling and Norris, 2023).[13] The studies that focus on student-to-student spread of mental well-being tend to find no or modest increasing effects, while those that examine the effects of other peer features (such as ability ranking) find larger effects.

Peer effects in residential facilities — which is the setting of my study — may be very different from those in school environments. Prior correlational evidence suggests that social effects in mental health predominately exist in close relationships and relationships outside the school environment (Zalk et al., 2010; Fruehwirth et al., 2019). Moreover, limited contact opportunities with family, prior history of problematic behaviors, and experience of adverse childhood events might make youths in residential facilities more susceptible to peer influences (Dishion and Tipsord, 2011). By studying peer effects in self-harm and mental disorders in residential care using register data on hospitalizations and death, I not only add to the peer effects literature but also to the economic literature on the determinants of self-harm and mental illness (e.g., Persson and Rossin-Slater, 2018; Adhvaryu et al., 2019; Baranov et al., 2020).

Peer effects in youth facilities are surprisingly understudied. Even though at least 6 million children are placed in residential facilities without parental care each year in the name of child welfare and justice (United Nations, 2020),

[11] For a review of the literature on peer effects, see Sacerdote (2011).

[12] An important difference between Eisenberg et al. (2014) and this paper is the severity of the self-harm studied. I only consider self-harm which at least results in hospitalization. In contrast, Eisenberg et al. (2014) classify, for example, having punched oneself as self-harm. The visibility of peer self-harm and the role of peer influence can be very different depending on the severity of the self-inflicted harm.

[13] Two studies that use non-survey data are Getik and Meier (2022) and Bütikofer et al. (2020), where the former uses Swedish register data to examine how gender composition in school cohorts affects mental health and the latter uses Norwegian register data to estimate the mental health effects of school selectivity.

only three papers can identify peer effects using credible designs in such settings (Bayer et al., 2009; Stevenson, 2017; Font and Mills, 2022).[14] The main focus of these three papers is crime-related outcomes. Bayer et al. (2009) use data from juvenile correctional facilities in Florida and find predominately reinforcing peer effects, meaning that exposing juveniles to peers with a criminal history within the same crime category increases recidivism. Stevenson (2017) uses similar data and finds evidence of social contagion of crime-oriented noncognitive factors.[15] Font and Mills (2022) use data on youths placed in foster care in Wisconsin. They do not find that exposure to peers with a higher risk of future imprisonment increases own risk of criminal behavior, running away, or early parenthood, but they find an increase in high school dropout rate. By considering novel groups of variables (substance abuse, self-harm, and mental disorders), I extend our knowledge about the peer effects in residential facilities.[16,17]

The paper proceeds as follows. Section 2.2 presents the institutional background and describes the data. Section 2.3 outlines the empirical methodology and discusses the validity of the identifying assumption. Section 2.4 presents the estimated peer effects while Section 2.5 probes possible mechanisms. Section 2.6 concludes.

[14]This population fares disproportionately worse than children living in family-type homes (Li et al., 2019; Gutterswijk et al., 2020) and are at a high risk of future involvement in socially destructive behaviors, such as crime and substance abuse (Shook et al., 2011; Aizer and Doyle, 2015; Eren and Mocan, 2021).

[15]These findings are in line with a sizable literature in economics that documents the importance of social interactions in explaining criminal behavior (e.g., Case and Katz, 1991; Glaeser et al., 1996; Ludwig et al., 2001; Jacob and Lefgren, 2003; Kling et al., 2005; Deming, 2011; Billings et al., 2014; Damm and Dustmann, 2014; Corno, 2017; Billings et al., 2019; Billings and Schnepel, 2022). Especially relevant for this paper is the work on peer effects in prisons (Harris et al., 2018; Damm and Gorinas, 2020). Literatures in criminology, sociology, and psychology also provide evidence of peer effects in crime (see Pratt et al., 2010).

[16]A related literature is that on the effects of placing juveniles in residential facilities (e.g., Hjalmarsson, 2009; Aizer and Doyle, 2015; Eren and Mocan, 2021). See Doyle and Aizer (2018) for a review of studies on the effects of child protection interventions. Of particular relevance is Helénsdotter (2023), who includes data on youths placed at NBIC in her analysis of the causal effects of involuntary placement in out-of-home care. Helénsdotter (2023) shows that such placement greatly increases the risk of death and hospitalization due to self-harm. However, she finds little evidence of an increase in the risk of experiencing adverse events related to substance abuse.

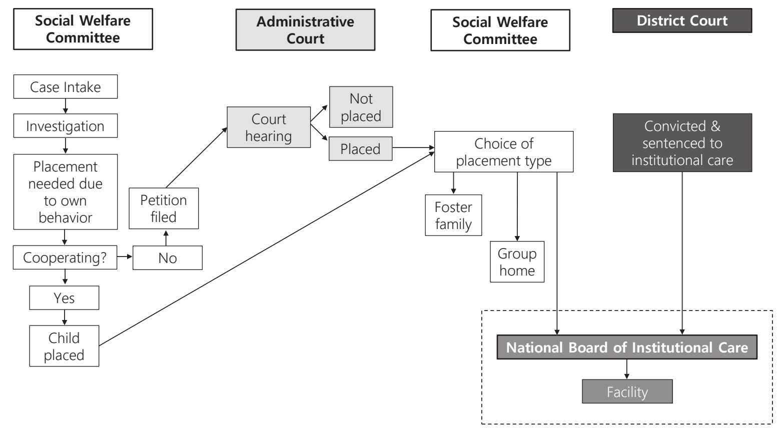
[17]This paper is also related to the literature on the effects of exposure to peers who come from deficient or criminal homes, which finds adverse effects on a range of outcomes (e.g., Carrell and Hoekstra, 2010; Carrell et al., 2018; Billings and Hoekstra, 2019; Santavirta and Sarzosa, 2019). There are also papers on neighborhood effects that include measures of mental health, drinking, and smoking (see Chyn and Katz, 2021, for a review). In addition, the literature on social influences on crime at the extensive margin is relevant. For example, Hjalmarsson and Lindquist (2012) provide indirect evidence that role modeling might explain why children of criminally active fathers are more likely to have a record.

2.2 Setting & Data

2.2.1 Institutional Care in Sweden

Each year, the National Board of Institutional Care (NBIC) houses youths aged below 21 in about 20 residential treatment facilities spread across Sweden.[18] Almost all youths (90%) are involuntarily placed at NBIC. The remaining 10% are either voluntarily placed or sentenced to serve time at NBIC for having committed a crime punishable by prison.[19] In the two former cases, the local Social Welfare Committee (SWC; *socialnämnden*) decides whether the youth should be placed at NBIC, while it is the District Court in the latter case. Figure 2.1 provides a representation of the possible routes to institutionalization.

Figure 2.1. *Pathways to Institutionalization in Sweden*



Note: This figure provides a representation of the different routes to placement at NBIC. Almost all youths at NBIC are placed by the SWC, either involuntarily via the court (90%) or voluntarily (5%). All of these youths engage in substance abuse, criminal behavior, or another destructive behavior that poses a significant risk to their health or development. The SWC decides whether the child should be placed at NBIC, but the central placement unit at NBIC allocates youths to facilities. Youths can also be sentenced to serve time at NBIC by a district court for committing a crime punishable by prison (5%).

Regardless of the placement route, the SWC must submit an application to the central placement unit at NBIC. The placement unit assigns the youth to a facility while considering (in falling order): gender, availability of beds, special needs, security class, age, group composition, and home region. The placement

[18]During the sample period, some facilities have closed and new ones have opened. Hence, the total number of unique facilities during the sample period is higher.

[19]According to the Swedish Criminal Code, a person under the age of 18 who commits a crime that would be sanctioned with imprisonment if the perpetrator had been 18 or older should be ordered to serve time at NBIC instead of prison unless there are special circumstances.

unit is open around the clock and the assignment should be done immediately upon receiving the application. No applications can be denied. Almost all youths are placed in their assigned facility the same day or the day after their placement application is submitted. Around one-third of youths are transferred to a new facility at some point after entering their first facility. However, in my main specification, I only exploit variation in the peer composition on the first day of arrival to estimate peer effects.

At NBIC, youths live together while being supervised by staff. All youths have separate bedrooms. The youths are institutionalized because they engage in substance abuse, criminal behavior, or some other destructive behavior (e.g., prostitution) that significantly threatens their health or development. While many of the youths at NBIC suffer from mental disorders and engage in self-harming behavior (e.g., skin cutting), having a mental disorder or engaging in self-harm is not legal grounds for placement at NBIC in itself.

The NBIC facilities are miles apart. Therefore, across-facility contact is very limited. Within facilities, youths spend large amounts of time together: in the common rooms, fenced-in yard, on-site school, therapy sessions, basketball practice, etc. In fact, youths are not free to leave the premises. Hence, the doors are locked and the facility is usually surrounded by a high fence.

Facilities vary in terms of target group, treatment portfolio, security class, and staff education. All facilities accept youths struggling with substance abuse or other destructive behaviors, and almost all accept youths with criminal backgrounds. Some facilities exclusively accept youths with a certain sex or age, while some facilities are more equipped than others to handle youths with special needs. Therefore, it is essential for identification to only exploit variation in peer composition within facilities.

Unless the youth is sentenced to serve time at NBIC, the SWC must reassess whether the need for care persists every six months. If the SWC determines that the youth would benefit from being placed in a foster family or other form of placement, the SWC can transfer the youth from NBIC at any time. If the youth no longer exposes their health or development to a significant risk of harm through abuse of addictive substances, criminal activity, or some other destructive behavior, the SWC terminates care. At the latest, care is terminated when the youth turns 21.

Youths placed at NBIC for committing an offense punishable by prison serve a fixed sentence. The length of the sentence is determined by the court in advance and must be at least 14 days and at most 4 years. There is no possibility of early release.

2.2.2 Cross-Country Comparison

Providing global estimates of the number of children and youths in residential facilities without parental care is difficult since there is a plethora of placement types and, in some countries, large informal sectors.[20] Nevertheless, the United Nations (2020) estimates that at least 6 million children and youths are placed in such facilities each year in the name of child welfare and justice. The estimated rate of placement in Sweden (2-3 children per 1,000) is lower than the rates in most Western countries. For example, in Canada, Germany, and the United Kingdom, the rate of placement is estimated to be 5-10 children per 1,000.

The institutional features and legal framework governing the provision of care to youths with behavioral problems are almost identical in Sweden and other Scandinavian countries. While the system employed in the US is very different, the systems used in other parts of Europe are similar to the Scandinavian system. In particular, favoring treatment in residential facilities akin to NBIC over prison when a youth abuses drugs, engages in prostitution, or commits crimes is common throughout Europe. In the US, on the other hand, the rate of imprisonment is over 10 times as high as in Western Europe (United Nations, 2020). For overviews of the systems employed in several Western countries, see Whittaker et al. (2022).

2.2.3 Data Description

The primary data source is the administrative records kept by NBIC on the universe of youths admitted to residential care during 2000-2020 in Sweden. The full sample contains 27,683 youth-by-spell observations and 16,461 unique youths. The records include placement reasons (as specified on the placement application submitted by the SWC) as well as information on facility assignments and dates related to admission, discharge, moves within NBIC, and episodes of isolation and absconding.

Statistics Sweden links the administrative records on youths placed at NBIC with register data using the youths' personal identity numbers. These personal identity numbers are unique and given to all Swedish residents, including foreign-born. I have accurate personal identity numbers for 93% of the youths. The reason for missing an accurate personal identity number is almost always recent immigration to Sweden. Statistics Sweden matches the youths to their parents and adds data on, e.g., gender, birth date, immigration/emigration dates, foreign background, labor income, and marital status of both youths and parents.

The main explanatory variables are measured *before* placement. History of substance abuse takes the value 1 if the youth (i) is placed at an NBIC facility because they expose their health or development to a significant risk of harm

[20] In Sweden, there is in practice no informal sector as all residential child and youth facilities must be registered with The Health and Social Care Inspectorate.

through substance abuse, (ii) is hospitalized from substance abuse, or (iii) commits a narcotic crime at any point during the 24 months prior to placement.[21] History of self-harm takes the value 1 if the youth is hospitalized from self-harm at least one time during the 24 months before placement. The main outcomes are measured similarly but during the 1-12 months *after* the placement ends. The outcome variables also take the value 1 if the youth dies from substance abuse or self-harm post-placement. See Appendix 2.C for detailed variable definitions.

Information on all hospitalizations at Swedish hospitals (private and public) related to substance abuse, self-harm, and mental health comes from the National In-Patient Register (1997-2020) kept by the National Board of Health and Welfare. Via the National Board of Health and Welfare, I also obtain data on all deaths (date and cause) from the National Cause of Death Register (1997-2021), court petitions for involuntary adult addiction treatment from the Register of Compulsory Care Under the Act on Care of Substance Abusers (2000-2020), and out-of-home placements from the Register on Service Provision to Children and Young Persons (2000-2020). Last, I obtain data on all legal proceedings (date of crime, date of decision, section of the law, and sanction) from the National Council for Crime Prevention (1997-2021).

To measure peer composition, I start with the full sample of youths (N=27,683). I define the peer group at the facility level: peers on day d are the other youths living in the same facility on day d as the youth i. However, I exclude youths who are in isolation or on the run on day d.[22] The main peer measure ($Peer\ history_{i,f,h}$) is the share of peers in facility f with pre-placement history h (e.g., substance abuse) on the *first day* that the youth is admitted to NBIC:

$$Peer\ history_{i,f,h} = \frac{1}{N_f - 1} \sum_{j \neq i} History_{j,f,h} \tag{2.1}$$

where $History_{j,f,h}$ is an indicator taking the value 1 if peer j in facility f has a pre-placement history of h and N_f is the number of available peers in facility f on the day of youth i's arrival. Since a youth can have a history of multiple behaviors, the shares do not sum to one.

Another potential peer measure is total exposure to peers with a certain history. Such a measure can be constructed as the sum of daily exposure shares over the total number of days that youth i spent in institutional care. An issue with using total exposure rather than first-day exposure is that the former might be heavily affected by unobservable characteristics of the focal youth even after accounting for facility-by-year FEs. In particular, unobservable factors may affect

[21] I use the date of the crime, not the date of conviction or reporting.

[22] One-third of youths run away or get put in isolation at least once during their first placement spell. Among those who run away or get put in isolation, the median number of days on the run or in isolation is 11.

the number of days the youth spends at NBIC and in individual facilities. For 95% of the sample, placement at NBIC is terminated when the youth no longer engages in destructive behavior or turns 21. Hence, a youth with severe prior substance abuse problems likely spends more time at NBIC than a youth with minor prior substance abuse problems, even after controlling for any history of substance abuse. If the youth with severe problems is more likely to abuse substances post-placement than the youth with minor problems (regardless of peer exposure), I may wrongly attribute the difference in post-placement behavior to greater peer exposure.

Table A4 presents the relationship between the main measure of peer exposure (the share of peers with a certain history on the first day of youth i's arrival) and total peer exposure over all days youth i spent at NBIC during their first placement spell. Irrespective of the behavior of interest, the main peer measure is a strong predictor of total peer exposure (F-statistic 27-105).

2.2.4 Sample Creation and Descriptive Statistics

I limit the sample (N=27,683) to the first placement spell per youth (N=16,461). Since I only observe youths who were admitted from January 1, 2000, I cannot observe the full peer group at the start of 2000. While the median placement length is only 3 months, over 10% of the sample are placed for longer than 1 year (see Figure A4). As only 2.5% are placed longer than 2 years, I drop the first two years and restrict the analysis sample to youths admitted during 2002-2020 (N=14,648).[23] I then drop youths without any peers on their first day (5 observations) and youths in facility-by-year cells with less than 2 youths (22 observations). The final sample consists of 14,621 unique youths assigned to one of 42 facilities (referred to as the '2002-2020 Sample').

When studying outcomes based on register data obtained from another source than NBIC, I further restrict the sample to youths who are observable by Statistics Sweden, i.e. youths with valid personal identity numbers (1,140 observations dropped). I also restrict the sample to youths whose NBIC placement was terminated at least 12 months before the end of 2020, i.e. the end of my hospital data (1,047 observations dropped). I then drop youths who emigrate from Sweden at any point during the 1-12 months after placement termination and are therefore not observable in the hospital records during the main window of interest (61 observations). Finally, I drop one youth who, after the sample restrictions, is in a facility-by-year cell with less than 2 youths. This sample (N=12,372) is referred to as the 'Main Analysis Sample'.[24]

[23] Results are robust to dropping the first three years.

[24] To test for selective sample attrition, I regress dummies for missing in Statistics Sweden's register data, placement ended less than 12 months before the end of my hospital data (year 2020), and missing in the 'Main Analysis Sample', respectively, on my main peer measures. The results presented in Table A1 suggest that selective attrition is negligible.

Table 2.1 displays descriptive statistics at the child and birth parent level (Panel A), placement spell level (Panel B), and peer level (Panel C) for all first-time placements, the '2002-2020 Sample', and 'Main Analysis Sample'. The descriptive statistics reported in the first and second columns are very similar. In the last column, the share of youths with missing personal identity numbers is (naturally) zero, and the share of youths with missing background information on any birth parent is smaller. However, the share is still large (23%). The reason for the high share of youths with missing information on at least one birth parent during the year before placement is that almost a quarter of the youths are foreign-born and, in this subgroup, it is common that at least one birth parent is unobservable at some point during the previous year.

Table 2.1. *Descriptive Statistics*

	All First-Time Placements	2002-2020 Sample	Main Analysis Sample
A: Child & Parent Characteristics			
Female	0.34	0.35	0.35
Foreign	0.29	0.30	0.23
Age	15.78	15.77	15.76
<15 y.o.	0.22	0.22	0.23
≥18 y.o.	0.13	0.13	0.14
Placement type:			
Involuntary	0.90	0.91	0.91
Sentenced	0.05	0.05	0.05
Voluntary	0.05	0.04	0.05
Required to attend school	0.43	0.43	0.44
Completed 9th grade	0.41	0.41	0.44
Missing personal identity number	0.07	0.08	0.00
History of:			
Substance abuse	0.57	0.57	0.57
Self-harm	0.05	0.05	0.05
Crime	0.75	0.76	0.75
Neurodevelopmental disorder	0.03	0.03	0.03
Depression	0.02	0.02	0.02
Other mental illness	0.06	0.06	0.06
Any birth parent:			
Dead	0.06	0.06	0.06
<18 y.o. at birth of child	0.02	0.02	0.02
Married, yr t-1	0.47	0.49	0.49
No labor income, yr t-1	0.49	0.50	0.50
Hosp. d.t. mental health, yr t-1	0.03	0.04	0.04
Hosp. d.t. substance use, yr t-1	0.03	0.03	0.03
Any crime, yr t-1	0.09	0.10	0.10
Missing any X, yr t-1	0.29	0.29	0.23

Table 2.1. *Continued*

B: Placement Characteristics			
Days in institutional care during spell	151.62	148.38	149.65
Total days in institutional care	254.45	248.91	260.81
Number of spells	2.30	2.35	2.27
First facility is in new county	0.71	0.72	0.69
C: Peer Characteristics on First Day			
Peer substance abuse	0.63	0.63	0.61
Peer self-harm	0.06	0.06	0.06
Peer crime	0.82	0.82	0.81
Peer NDD	0.04	0.04	0.04
Peer depression	0.02	0.02	0.02
Peer other mental illness	0.07	0.07	0.07
Peer female	0.32	0.32	0.33
Peer foreign	0.27	0.27	0.26
Peer <15 y.o.	0.21	0.21	0.22
Peer missing personal id. no.	0.05	0.05	0.04
Peer size	19.44	19.46	19.00
Above median peer size	0.38	0.38	0.38
Unique facilities	47	42	42
Unique birth parents	26017	23017	21192
Observations	16461	14621	12372

Note: This table presents descriptive statistics on child, parent, placement, and first-day peer characteristics for all first-time placements at NBIC and each analysis sample as described in Section 2.2.4. Child and parent characteristics are measured at or by the start of the youth's first NBIC placement. Statistics are shown for observations with non-missing information.

2.3 Empirical Methodology

2.3.1 Empirical Specification

To investigate how exposure to peers with a history of substance abuse and self-harm affects individual outcomes, and motivated by the specification in Bayer et al. (2009), I estimate (for each history category):

$$\begin{aligned} Y_i = & \beta_0(Own\ history_i * Peer\ history_i) \\ & + \beta_1(No\ own\ history_i * Peer\ history_i) \\ & + \alpha(Own\ history_i) + \delta_{f,t} + \varepsilon_{i,f,t} \end{aligned} \tag{2.2}$$

where Y_i is an outcome measured for youth i who was first admitted in year t to facility f, $Own\ history_i$ is an indicator taking the value 1 if the youth has a history of the category of interest, $No\ own\ history_i$ indicates the opposite, $Peer\ history_i$ is the share of peers in facility f on the day of youth i's arrival who have a history of the category of interest, $\delta_{f,t}$ are facility-by-year FEs, and

$\varepsilon_{i,f,t}$ is an error term.

Facility-by-year FEs are included to account for systematic differences in own and peer characteristics between facilities and over time. By including such FEs, I only exploit variation in peer composition within facility-by-year cells. I demonstrate robustness to using (i) facility-by-quarter FEs, (ii) facility and year FEs, and (iii) facility-by-history and year FEs. Figure A1 depicts the variation in peer characteristics after accounting for facility-by-year FEs. For residualized peer substance abuse, the mean is 0.628 (std. dev. 0.089) while it is 0.060 for self-harm (std. dev. 0.053). Figure A2 presents plots for peer substance abuse and self-harm by the youth's own history. Beyond the distribution of the residualized and mean-standardized first-day measures, the plots in Figure A2 also include flexible regressions of total peer exposure on first-day peer exposure by own history. In each plot, total peer exposure appears to monotonically increase in first-day exposure.

Since the treatment (peer composition) is assigned to all youths who enter the same facility on the same day, I cluster the standard errors at the facility-by-day level (see Abadie et al., 2023). I show robustness to alternative levels of clustering.

As shown in Table 2.2, own history of a behavior is a strong predictor of future behavior in the same problem category in the 1-12 months after the end of the placement spell. This is also the case when controlling for all child and parent controls listed in Table 2.1. Hence, the baseline probability of engaging in a given risky behavior in the future is vastly different for those with and without a pre-placement history of the behavior. In line with previous studies on peer effects in criminal behavior (e.g., Bayer et al., 2009, Damm and Gorinas, 2020), I chose Specification 2.2 (before gaining access to the data) to allow the effect of peer history to depend on the individual's own history. Thereby, I can shed light on the presence and character of introductory versus reinforcing peer effects. Such knowledge is essential for understanding the mechanisms at play and for the challenging task of optimally assigning youths with certain backgrounds to peer groups.

2.3.2 Test of Identifying Assumption

At which facility a youth is placed is not random. To circumvent the issue of non-random assignment, I rely on natural variation in peer composition within facility-by-year cells as youths are discharged and new ones are admitted. This approach has been employed by Bayer et al. (2009), followed by Stevenson (2017) and Damm and Gorinas (2020). It relies on the central assumption that the variation in peer characteristics is as good as random after netting out facility-by-year FEs.

To examine the validity of this assumption, I present results from employing randomization checks in Table 2.3 and Table A2 (see Bayer et al., 2009 and Guryan et al., 2009). In Table 2.3, I regress the share of peers with a history of substance abuse (self-harm) on own history of the corresponding behavior with and without facility-by-year FEs. In all regressions, I control for the leave-out mean of the relevant history category within the population at risk of being the youth's peers (i.e. the leave-out mean within the facility-by-year cell). The leave-out mean within the randomization cell is included to account for the mechanical negative correlation between individual and peer characteristics.

In line with facility specialization, there is a strong correlation between own and peer pre-placement history when no facility-by-year FEs are included (columns 1 and 3 in Table 2.3). Hence, naive estimations of peer effects that do not take into account such specialization would likely yield biased estimates. Fortunately, when facility-by-year FEs are included, the correlations between own and peer history are small and lack statistical significance. These results provide strong empirical evidence that youths are as-if-randomly assigned to peer groups within facility-by-year cells. In Table A2, I present further evidence in support of the central assumption using the diagnostic testing procedure proposed in Bayer et al. (2009).[25]

Table 2.2. *Own History as Predictor of Outcomes*

	Substance Abuse, Month 1-12		Self-Harm, Month 1-12	
	(1)	(2)	(3)	(4)
Own substance abuse	0.3289***	0.3149***		0.0074**
	(0.0083)	(0.0089)		(0.0037)
Own self-harm		0.0044	0.1100***	0.0788***
		(0.0197)	(0.0137)	(0.0133)
Constant	0.1410***	-0.0924	0.0285***	0.0612
	(0.0051)	(0.1079)	(0.0015)	(0.0471)
Child & parent controls	No	Yes	No	Yes
Adjusted R-squared	0.1566	0.1625	0.0343	0.0511
Dependent mean	0.3296	0.3296	0.0344	0.0344
Observations	12372	12372	12372	12372

Note: The dependent variable is any adverse event (readmission, hospitalization, death, or crime) related to substance abuse or self-harm (respectively) during the 1-12 months after exit from the first placement spell at NBIC. Each dependent variable is regressed on an indicator taking the value 1 if the youth has a pre-placement history of the corresponding behavior. The 'Main Analysis Sample' is used. All specifications are estimated using OLS and include facility-by-year FEs. The full set of child and parent characteristics listed in Table 2.1 are included as controls in columns 2 and 4. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

[25] First, I predict each outcome (substance abuse and self-harm post-placement) based on the full set of child and parent characteristics listed in Table 2.1 and full sets of home county and

Table 2.3. *Test of Exogenous Peer Variation*

	2002-2020 Sample		Main Analysis Sample	
	(1)	(2)	(3)	(4)
A: Peer Substance Abuse				
Own substance abuse	0.0264***	-0.0053	0.0275***	-0.0062
	(0.0034)	(0.0051)	(0.0039)	(0.0058)
Dependent mean	0.6286	0.6286	0.6102	0.6102
B: Peer Self-Harm				
Own self-harm	0.0244***	-0.0079	0.0246***	-0.0099
	(0.0040)	(0.0089)	(0.0042)	(0.0091)
Facility*year FEs	No	Yes	No	Yes
Dependent mean	0.0604	0.0604	0.0619	0.0619
Observations	14621	14621	12371	12371

Note: The dependent variable is the share of peers with a history of substance abuse (Panel A) or self-harm (Panel B) on the first day of the youth's placement. All models are estimated with OLS and control for the facility-by-year leave-out mean. Columns 2 and 4 also include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

While the outlined strategy allows for a causal analysis of peer effects, the estimated effect might reflect the influence of other peer characteristics than the peer characteristic of interest. For example, if own depression increases the risk of engaging in self-harm, and peer depression affects own depression, the estimates of the effect of peer self-harm on own self-harm are likely subject to omitted variable bias. To alleviate such concerns, I follow Altonji et al. (2005) and examine the stability of my estimates to the inclusion of additional peer characteristics. In the peer effects literature, concerns about omitted variable bias have been addressed in a similar fashion by Golsteyn et al. (2020).

2.4 Results

2.4.1 Peer Effects in Substance Abuse and Self-Harm

Table 2.4 presents the estimated effects of peer substance abuse and self-harm, respectively, on the risk of youth i experiencing an adverse event (hospitalization, re-institutionalization, death, or crime) related to each behavior during the 1-12 months after placement exit. Separate effects are estimated for youths with versus without a history of each behavior. Columns 1 and 4 include no other controls than facility-by-year FEs. Columns 2 and 5 include controls for the peer history measures listed in panel C of Table 2.1, while columns 3 and 6 add controls for the full set of child and parent characteristics.[26]

facility-by-year FEs. I then regress predicted outcomes on the peer variables and corresponding own history variables with and without facility-by-year FEs. In line with the validity of the central assumption, the estimated coefficients are close to zero, lack individual significance, and are not jointly significant when facility-by-year FEs are included.

As shown in column 1 of Table 2.4, being first placed in a facility on a day with a higher share of peers with a history of substance abuse significantly increases (at the 5% level) the risk of experiencing an adverse event related to substance abuse in the 1-12 months after placement, but only if the youth has a history of substance abuse. Relative to the average risk of a future substance abuse-related event among youths with a history of substance abuse (48.3%), the point estimate of 12.5 percentage points in column 1 implies that a 1-standard deviation increase in peer exposure (0.22) increases the risk of future substance abuse problems by 2.7 percentage points (or 5.6%) to 51.0% for youths at the mean.

For youths without a history of substance abuse, the estimated peer effect is negative and not statistically significant. Tests of equality reveal a significant difference in coefficients at the 1% level.

The same pattern is revealed for self-harm (column 4, Table 2.4): a large reinforcing peer effect is found (significant at the 5% level), while the estimated introductory peer effect is small and lacks statistical significance. The difference in effects is significant at the 5% level. The point estimate of 40.3 percentage points for youths with a history of self-harm is striking. However, the variation in peer self-harm is smaller compared to substance abuse. Hence, a 1-standard deviation increase in exposure to peer self-harm (0.10) would increase the risk of future self-harm by 4.1 percentage points. Among youths with a history of self-harm, such an increase would raise the average risk of being hospitalized or dying from self-harm post-exit from 15.0% to 19.1% for youths at the mean.

As shown in columns 2-3 and 5-6 of Table 2.4, the estimated reinforcing peer effects in substance abuse and self-harm remain similar when controlling for the other peer measures and child and parent characteristics. This suggests that omitted variable bias is potentially of limited concern for my estimates (Altonji et al., 2005) and supports that it is the peer characteristic of interest, rather than some other peer characteristic such as gender composition, that is driving the estimated effects.

[26]Estimated effects of all peer measures interacted with own history of the corresponding peer measure on the main outcomes (substance abuse and self-harm post-placement) are available in Table A5. In Table A8, I provide estimated peer effects in crime, depression, and any mental disorder (excluding substance abuse and self-harm). Peer effects in crime by crime category are offered in Table A9.

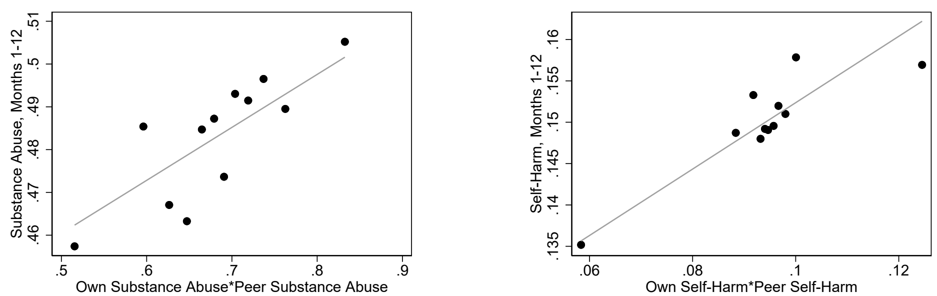
Table 2.4. *Effect of Peers on Post-Exit Outcomes*

	Substance Abuse, Month 1-12			Self-Harm, Month 1-12		
	(1)	(2)	(3)	(4)	(5)	(6)
Own history*Peer history	0.1247**	0.1124**	0.0985**	0.4032**	0.4379***	0.4249***
	(0.0490)	(0.0503)	(0.0501)	(0.1609)	(0.1616)	(0.1591)
No own history*Peer history	-0.0123	-0.0277	-0.0226	-0.0040	0.0259	0.0110
	(0.0421)	(0.0438)	(0.0438)	(0.0310)	(0.0330)	(0.0328)
Dependent mean if own hist.=1	0.4829	0.4829	0.4829	0.1500	0.1500	0.1500
Dependent mean if own hist.=0	0.1236	0.1236	0.1236	0.0278	0.0278	0.0278
Test of equality (p-value)	0.0008	0.0006	0.0032	0.0106	0.0099	0.0085
Peer controls	No	Yes	Yes	No	Yes	Yes
Child & parent controls	No	No	Yes	No	No	Yes
Observations	12372	12372	12372	12372	12372	12372

Note: The dependent variable is an indicator equal to one if the individual experiences an adverse event (readmission, hospitalization, death, or crime) related to substance abuse or self-harm (respectively) during the 1-12 months after exit from the first placement spell at NBIC. Each dependent variable is regressed on first-day peer exposure interacted with the youth's own history of the behavior indicated in the top row. All specifications are estimated using OLS and include facility-by-year FEs. Whether controls for other first-day peer characteristics (substance abuse, self-harm, crime, neurodevelopmental diagnosis, depression, other mental disorder, female, foreign-born, under 15 years old, and missing personal identity number) and the full set of child and parent characteristics (as listed in Table 2.1) are included is indicated at the bottom of the table. The 'Main Analysis Sample' is used. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Figure 2.2 depicts the relationship between peer history and future outcomes among youths with a history of the same behavior. The relationship is approximately linear for both substance abuse and self-harm. Figure A6 provides the corresponding figure for youths with no such history. Figure A6 also illustrates the relationship between peer characteristics and future outcomes when the number of peers (rather than the share) with a certain history is used.

Figure 2.2. *Relationship Between Peer History and Post-Exit Outcomes*



Note: Black dots represent the risk of experiencing an adverse event related to substance abuse and self-harm post-placement over the share of first-day peers with a history of the corresponding behavior interacted with own history. All values are residualized and mean-standardized using facility-by-year FEs, no own history interacted with peer exposure, and own history.

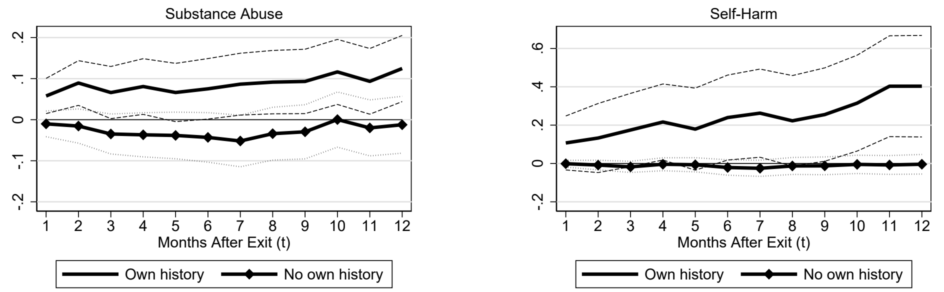
Figure 2.3 traces out the effect of peer exposure on the cumulative risk of experiencing adverse events related to each behavioral category by month t after the end of the NBIC placement (with 90% confidence intervals). Separate lines are shown for youths with and without a history of the behavior. Among youths with a history of substance abuse, the estimated effect on post-placement substance abuse is positive for the full 12-month window and appears to be slightly increasing in size over time. The point estimates are statistically significant (at conventional levels) at all time points except month 5. For youths with no history of substance abuse, the point estimates are negative and lack statistical significance.

For self-harm, the effect is positive and increases over the months following placement exit among youths with a prior history of self-harm. The effect is statistically significant in months 6-7 and from month 9 onward. Among youths with no such prior history, the point estimates are close to zero during the year post-placement.

2.4.2 Heterogeneity

Prior studies document important gender and age differences in the prevalence of substance abuse and self-harm as well as susceptibility to peer influence (Kloos et al., 2009; Bresin and Schoenleber, 2015; Laursen and Veenstra, 2021). There is

Figure 2.3. *Effect of Peers on Post-Exit Outcomes*



Note: Black lines show OLS estimates of the effect of peers on the cumulative probability of experiencing an adverse event related to substance abuse and self-harm (respectively) by month t post-exit. The relevant outcome is stated in the subfigure heading. Dashed lines show 90% confidence bands. All specifications condition on being in Sweden during month t.

also some evidence of heterogeneity in these behaviors by foreign background (Jablonska et al., 2009; Cristini et al., 2015). Hence, I first split the sample by gender, age, and foreign background. Results are presented in Table 2.5. Columns 3, 6, and 9 of Table 2.5 give the difference between the point estimates in the preceding two columns. Interestingly, there is evidence of an introductory peer effect in substance abuse among youths younger than 15. Compared to youths aged 15 or older, the introductory peer effect estimate is significantly larger (p-value=0.054). The point estimate for youths younger than 15 (12.4) implies that a 1-standard deviation (0.19) increase in peer substance abuse raises the risk of experiencing adverse outcomes related to substance abuse from 11.9% to 14.3% at the mean (i.e. by 19.6%).

Table 2.5. *Heterogeneity of Peer Effects in Substance Abuse and Self-Harm I*

	Gender			Background			Age at Placement		
	(1) Girl	(2) Boy	(3) Δ	(4) Foreign	(5) Native	(6) Δ	(7) <15 yrs	(8) ≥15 yrs	(9) Δ
A: Substance Abuse, Months 1-12									
Substance abuse*Peer substance abuse	0.1852***	0.0737	0.1115	0.0835	0.1346**	-0.0510	0.1674	0.0667	0.1007
	(0.0714)	(0.0677)	(0.0982)	(0.1184)	(0.0553)	(0.1262)	(0.1045)	(0.0579)	(0.1177)
Not substance abuse*Peer substance abuse	-0.0013	-0.0502	0.0488	-0.0129	0.0197	-0.0326	0.1236*	-0.0499	0.1736*
	(0.0600)	(0.0602)	(0.0848)	(0.0968)	(0.0491)	(0.1051)	(0.0740)	(0.0534)	(0.0900)
Dependent mean if own hist.=1	0.3906	0.5304		0.4996	0.4790		0.4044	0.4950	
Dependent mean if own hist.=0	0.1194	0.1259		0.1224	0.1240		0.1195	0.1258	
Observations	4309	8037	12346	2785	9508	12293	2728	9561	12289
B: Self-Harm, Months 1-12									
Self-harm*Peer self-harm	0.3266*	0.3352	-0.0085	0.1852	0.4389***	-0.2537	0.8885**	0.3178*	0.5707
	(0.1932)	(0.3707)	(0.4185)	(0.5474)	(0.1687)	(0.5514)	(0.4154)	(0.1703)	(0.4416)
Not self-harm*Peer self-harm	0.0165	0.0167	-0.0002	0.1252	-0.0270	0.1523	-0.0407	-0.0081	-0.0326
	(0.0436)	(0.0393)	(0.0586)	(0.0893)	(0.0346)	(0.0926)	(0.0757)	(0.0337)	(0.0817)
Dependent mean if own hist.=1	0.1911	0.0619		0.1414	0.1515		0.1207	0.1563	
Dependent mean if own hist.=0	0.0491	0.0173		0.0225	0.0295		0.0347	0.0258	
Observations	4309	8037	12346	2785	9508	12293	2728	9561	12289

Note: The dependent variable (indicated in the panel heading) is any adverse event related to substance abuse or self-harm (respectively) during the 1-12 months after exit from the first placement spell at NBIC. Each dependent variable is regressed on first-day peer exposure interacted with the youth's history of the behavior. The 'Main Analysis Sample' is restricted to the subsample indicated in the top row. All specifications are estimated using OLS and include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Prior studies document that peer effects tend to be stronger in groups with similar characteristics (e.g., Billings and Schnepel, 2022). To inspect whether peer effects increase with connectivity in the current context, I calculate two measures of peer exposure: (i) the share of peers with history h and the same sex as the focal youth and (ii) the share of peers with history h and the opposite sex as the focal youth. In Table A10, I regress the main outcomes on these gender-specific peer measures, interacted with own history. The results suggest that the reinforcing peer effects in substance abuse and self-harm are entirely driven by exposure to peers of the same sex. In fact, the point estimated effects of opposite-sex peer history are negative and lack statistical significance. The differences in point estimates are significant at the 5-10% level. Turning to foreign background, there is less evidence of larger peer effects with connectivity (Table A11).

Heterogeneity can also be explored using placement characteristics (Table 2.6). First, I split the sample by peer group size on the day of the focal youth's arrival. The effects might vary by peer size for several reasons. For example, a larger group might facilitate homophily (i.e. clustering of similar youths) as there are more peers to choose from when deciding who to interact with. Hence, the effect of an increase in the share of peers with a certain history might be amplified among youths with the same history in large groups. In addition, the effect of increasing the share of peers with a history of behavior h can vary by the size of the peer group since the same percent increase translates into different changes in the total number of peers with a history of h.

As shown in column 3 of Table 2.6, there is a statistically significant difference in effect size for substance abuse. Specifically, the effect of peer substance abuse is larger (significant at the 5% level) when there are more than 20 peers available on the first day of the youth's arrival. For self-harm, there is only minor evidence of a larger impact of peer self-harm among peer groups with more than 20 youths, but only if the youth does not have a prior history of self-harm.

How crowded the facility is on the day of arrival may also be important for the transmission of harmful behaviors. If there are unusually many youths, it might be more difficult for staff to detect contraband such as drugs, peer-to-peer victimization, and inappropriate interactions. In columns 4-5 of Table 2.6, I split the sample by whether there are more peers on the first day than the median within that facility-by-year cell. However, I find no statistically significant differences.

Moreover, being further away from home might facilitate peer influence by limiting external contact (Dobson and Dozois, 2008). Hence, I explore heterogeneity by whether the facility is in the youth's home county in columns 7-9 of Table 2.6 but again find no statistically significant differences.

Table 2.6. *Heterogeneity of Peer Effects in Substance Abuse and Self-Harm II*

	Peer Count			High Share Occupied Beds			Across-County Move		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	≤20 peers	>20 peers	Δ	Yes	No	Δ	Yes	No	Δ
A: Substance Abuse, Months 1-12									
Substance abuse*Peer substance abuse	0.0621	0.3254***	-0.2634**	0.1615*	0.1880***	-0.0265	0.1471**	0.1264	0.0208
	(0.0568)	(0.1078)	(0.1223)	(0.0959)	(0.0604)	(0.1126)	(0.0611)	(0.0873)	(0.1055)
Not substance abuse*Peer substance abuse	-0.0202	0.0746	-0.0948	0.0543	0.0162	0.0381	-0.0251	0.0438	-0.0689
	(0.0467)	(0.1052)	(0.1156)	(0.0886)	(0.0517)	(0.1018)	(0.0543)	(0.0727)	(0.0899)
Dependent mean if own hist.=1	0.4752	0.4929		0.4881	0.4796		0.4888	0.4681	
Dependent mean if own hist.=0	0.1203	0.1277		0.1332	0.1179		0.1259	0.1191	
Observations	6929	5420	12349	4616	7698	12314	8501	3778	12279
B: Self-Harm, Months 1-12									
Self-harm*Peer self-harm	0.4098**	0.2016	0.2082	0.3540	0.4208**	-0.0668	0.2587	0.6360**	-0.3773
	(0.1809)	(0.3573)	(0.4020)	(0.2665)	(0.1971)	(0.3297)	(0.1914)	(0.3029)	(0.3575)
Not self-harm*Peer self-harm	-0.0189	0.1364*	-0.1552*	0.0660	-0.0190	0.0850	-0.0061	0.0317	-0.0378
	(0.0341)	(0.0772)	(0.0847)	(0.0683)	(0.0357)	(0.0764)	(0.0377)	(0.0590)	(0.0694)
Dependent mean if own hist.=1	0.1686	0.1145		0.1429	0.1538		0.1381	0.1754	
Dependent mean if own hist.=0	0.0316	0.0231		0.0271	0.0283		0.0289	0.0255	
Observations	6929	5420	12349	4616	7698	12314	8501	3778	12279

Note: The dependent variable (indicated in the panel heading) is any adverse event related to substance abuse or self-harm (respectively) during the 1-12 months after exit from the first placement spell at NBIC. Each dependent variable is regressed on first-day peer exposure interacted with the youth's history of the behavior. The 'Main Analysis Sample' is restricted to the subsample indicated in the top row. All specifications are estimated using OLS and include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

While the effects are imprecisely estimated in some subsamples, the point estimated effects of peer substance abuse and self-harm among youths with a history of the behavior are positive in all subsamples presented in Tables 2.5-2.6.

2.4.3 Placebo & Robustness Checks

Table A3 offers a placebo test in which the main peer measures are replaced with the share of peers with history h in facility f 180 days *before* the youth enters the facility.

Robustness checks related to specification decisions are presented in Table C1. Baseline results are provided in Panel A for comparison. The results are robust to controlling for the full set of child, parent, and peer characteristics (see Table 2.1) and the crime rate in the youth's home municipality in the month before intake (Panel B). The results are also robust to controlling for the leave-out mean history of the behavior of interest within the randomization cell (Panel C). Moreover, I demonstrate robustness to clustering the standard errors at the facility-by-year level (Panel D) and replacing facility-by-year FEs with facility-by-quarter FEs (Panel E), facility and year FEs (Panel F), and facility-by-history and year FEs (Panel G).

In Table C2, I present evidence that the results are robust to sample selection by restricting the sample to placements longer than 14 days (Panel A), placements without imputed exit dates (Panel B), placements starting in 2003 or later (Panel C), placements starting before 2019 (Panel D), and youths who were not placed at the same facility and at the same time as their full or half sibling (Panel E). Last, I demonstrate robustness to how peer exposure is measured in Panels F-G by replacing the main peer measures with the share of peers with history h on the day after entry (Panel F), the average share of peers with history h during the first 0-2 days after entry (Panel G), and the number of peers with history h on the first day of entry (Panel H).

The results are also robust to dropping each facility (Tables C3-C6).

2.5 Mechanisms

My analysis suggests that being exposed to peers who have a background of substance abuse and self-harm has large adverse effects on the post-placement outcomes of youths with a history of such behaviors. In this section, I explore mechanisms through which peer exposure can impact post-placement outcomes.

2.5.1 Decomposed Effects on Post-Placement Outcomes

First, I unpack the estimated peer effects by estimating the effects of peer substance abuse on the individual variables included in the construction of my main measure of post-placement substance abuse.

Post-placement substance abuse takes the value 1 if the youth is hospitalized, dies, or is readmitted to an NBIC youth facility due to substance abuse or commits a narcotic crime during the 1-12 months after placement termination. In Table 2.7, I regress the composite outcome and components of the composite outcome on the main measure of peer substance abuse interacted with own history. Among youths with a history of substance abuse, the point estimated effect of exposure to peers with a history of substance abuse is positive in all columns but lacks statistical significance when the outcome is narcotic crime and cannabis-related hospitalizations.

In Table 2.8, I decompose the effect of exposure to peers with a history of self-harm. The first column of Table 2.8 provides the baseline results. The composite measure of self-harm includes hospitalizations and deaths due to confirmed and potential self-harm. In columns 2-5, I regress each variable on the composite measure of peer self-harm. The reinforcing peer effect in self-harm appears to be driven by an increase in the risk of being hospitalized due to both confirmed and potential self-harm.

The lack of a statistically significant increase in narcotic crime following exposure to peers with a history of substance abuse might be explained by the youths' living situation after placement. First, if youths commit crimes before placement exit for which they are sentenced to serve time in prison, they might not be able to commit narcotic crimes in the 1-12 months after placement exit. Second, prosecutors are encouraged to drop cases against institutionalized youths (The Prosecutor-General of Sweden, 2006). Hence, if peer exposure increases the risk of being readmitted I might not be able to observe a rise in narcotic crimes even if there is a meaningful effect. Table A6 presents evidence that exposure to peers with a history of substance abuse increases the likelihood of being readmitted (for any reason) to NBIC and lowers the likelihood of being placed in a foster family among youths with a prior history of substance abuse.[27] The point estimate for being placed in prison is positive but imprecise for youths with a history of substance abuse.[28]

[27] Figure A3 shows the share of youths in out-of-home care in the months after exiting their first placement at NBIC.

[28] The estimates for prison and adult addiction treatment should be interpreted with caution given the small sample. Since prison and adult addiction treatment are rare outcomes for youths under age 18, I limit the sample to youths who are at least 18 at the time of exit, which leaves fewer than 3,000 youths.

Table 2.7. *Decomposed Effect of Peers on Substance Abuse, Months 1-12*

	Composite	Due to Substance Use...			Committed	Hospitalized due to...				
	(1) Substance Abuse	(2) Death, Hosp. or Readmission	(3) Death	(4) Readmission	(5) Narcotic Crime	(6) Any Substance Abuse	(7) Alcohol	(8) Narcotics	(9) Cannabis	(10) Other Substance Abuse
History of Substance Abuse										
Own history*Peer hist.	0.1247**	0.1213***	0.0041**	0.0822*	0.0178	0.0952***	0.0245*	0.0165**	0.0111	0.0780***
	(0.0490)	(0.0458)	(0.0019)	(0.0434)	(0.0482)	(0.0274)	(0.0126)	(0.0072)	(0.0124)	(0.0207)
No own history*Peer hist.	-0.0123	-0.0054	-0.0010	0.0160	-0.0012	-0.0119	0.0017	0.0023	0.0075	-0.0123
	(0.0421)	(0.0376)	(0.0018)	(0.0350)	(0.0433)	(0.0223)	(0.0121)	(0.0055)	(0.0100)	(0.0162)
Dep. mean if own hist.=1	0.4829	0.3481	0.0024	0.2947	0.2775	0.0979	0.0196	0.0069	0.0164	0.0622
Dep. mean if own hist.=0	0.1236	0.0758	0.0000	0.0581	0.0772	0.0235	0.0102	0.0008	0.0042	0.0087
Test of equality (p-value)	0.0008	0.0005	0.0103	0.0550	0.6379	0.0000	0.0324	0.0052	0.6835	0.0000
Observations	12372	12372	12372	12327	9518	12327	12327	12327	12327	12327

Note: Each dependent variable at the top of the table is regressed on peer history of substance abuse interacted with own history of substance abuse. The 'Main Analysis Sample' is used. In columns 4-10, youths who died during the first 12 months are excluded (N=45). In column 5, youths aged below 15 are excluded because such young children are not observable in the National Council for Crime Prevention's crime register. All estimations include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table 2.8. *Decomposed Effect of Peers on Self-Harm, Months 1-12*

	Composite	Known Intent		Unknown Intent	
	(1) Self-Harm	(2) Hospitalization	(3) Death	(4) Hospitalization	(5) Death
History of Self-Harm					
Own history*Peer history	0.4032**	0.3163**	0.0000	0.4078**	-0.0055
	(0.1609)	(0.1576)	(0.0179)	(0.1606)	(0.0195)
No own history*Peer history	-0.0040	-0.0038	-0.0005	-0.0070	0.0035
	(0.0310)	(0.0296)	(0.0041)	(0.0306)	(0.0056)
Dependent mean if own hist.=1	0.1500	0.1400	0.0030	0.1476	0.0030
Dependent mean if own hist.=0	0.0278	0.0241	0.0009	0.0268	0.0011
Test of equality (p-value)	0.0106	0.0396	0.9790	0.0090	0.6697
Observations	12372	12327	12372	12327	12372

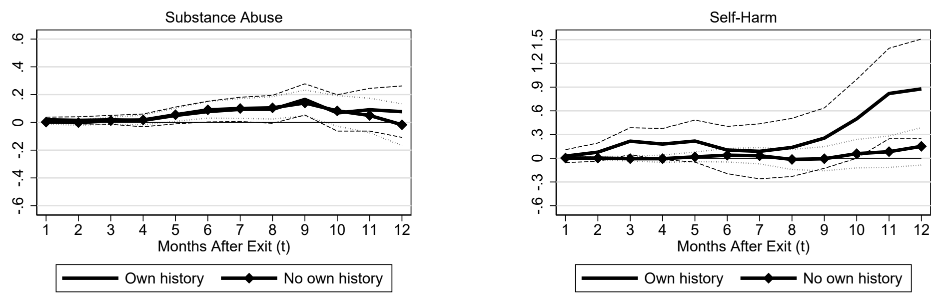
Note: Each dependent variable at the top of the table is regressed on peer history of self-harm interacted with own history of self-harm. The 'Main Analysis Sample' is used. In columns 2 and 4, youths who died during the first 12 months are excluded (N=45). All estimations include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

2.5.2 Socialization and Co-Harming

If peers with a history of substance abuse and self-harm are more likely to engage in these activities at NBIC than other peers, such an increase in exposure to incidents involving substance abuse and self-harm during placement can impact own risk of substance abuse and self-harm through, for example, triggering of craving or emotional distress (Walton et al., 1995). In addition, youths who identify with peers who engage in these behaviors may imitate the behavior and join their peers, which can be a form of socialization and create a sense of closeness and community (Taiminen et al., 1998). The intensity and frequency of own behavior may increase to match peer behavior. Due to the addictive nature of both behaviors (Davis and Lewis, 2019), such an increase in frequency and intensity can impact the risk of experiencing adverse outcomes even after placement.

To shed some light on this mechanism, I first examine whether first-day peer composition affects the likelihood of death, hospitalization, and narcotic crimes during placement. Figure 2.4 traces out the effects of first-day peer composition on the risk of experiencing adverse events related to substance abuse and self-harm during the months after admission but before placement termination. Since placements last for 5 months on average, the confidence intervals are wide toward the end of the displayed 12-month window.

Figure 2.4. *Effect of Peers on Behavioral Outcomes During Placement*



Note: Black lines show OLS estimates of the effect of peers on the cumulative probability of an adverse event related to substance abuse and self-harm (respectively) occurring by month t post-intake. The relevant outcome is stated in the subfigure heading. Dashed (dotted) lines show 90% confidence bands for youths with (without) a history of the behavior. All specifications condition on the NBIC placement ending after month t.

In line with the main results, the point estimates for peer substance abuse and self-harm are positive among youths with a prior history of the corresponding behavior for the full 12-month window. However, the estimates are imprecise. For substance abuse, the reinforcing peer effect is significant at the 5-10% level in months 6-7 and 9, while for self-harm, the estimate is significant at the 5-10% level in months 3 and 10-12.[29]

Moreover, there appears to be considerable clustering of severe substance abuse and self-harm incidents: almost 1 in 4 youths who are hospitalized for self-harm while placed at NBIC are hospitalized (at least once) in the same month as their peer is hospitalized for the same reason. An equivalent share is observed among youths who are hospitalized for substance abuse while placed at NBIC.

If peer behavior affects own behavior through direct exposure to substance abuse and self-harm incidents, we should expect an effect of exposure to peers with a history of substance abuse (but not self-harm) on the length of time the youth spends at NBIC because the placement length depends on whether the youth threatens their health or development through substance abuse, criminal activity, or other destructive behavior (excluding self-harm). In Table A7, I regress the effective number of days spent at NBIC (excluding any time on the run or in isolation) on each peer measure interacted by own history. As expected, exposure to peers with a history of substance abuse significantly increases (1% level) the time youths with a history of substance abuse spend at NBIC. Exposure to peers with a history of self-harm, on the other hand, does not significantly increase placement duration.

All in all, these results speak in favor of peers influencing youth substance abuse and self-harm through direct exposure to peer behavior.

2.5.3 Interrupted Treatment

Peer composition may affect post-placement outcomes by hampering treatment provision. In particular, substance-abusing youths might coordinate to help each other escape and use substances outside the treatment facilities. In support of this channel, I find evidence that exposure to peers with a history of substance abuse increases the number of days on the run if the youth has a history of substance abuse (Table A7).[30,31] However, the reinforcing peer effect in substance abuse is

[29]My measures of substance abuse and self-harm are largely based on hospital data. However, there are staff members at each NBIC facility around the clock. Hence, drug use and self-harming acts committed at NBIC might be caught *before* the youth can overdose or inflict serious injury to themselves, thereby avoiding hospitalization. In addition, there is medically trained staff at all NBIC facilities. These staff members can potentially handle some injuries that would otherwise warrant hospitalization.

[30]Even though the security of NBIC facilities is supposed to be high, it is surprisingly common to escape: almost 1 in 5 manage to escape at least one time during their first placement spell.

[31]Treatment can also be affected by being placed in isolation, which implies that the youth is kept separate from the other youths and only has contact with staff members. However, it is not evident that exposure to substance abusing or self-harming peers would influence time spent in isolation. Isolation is supposed to be used if staff members believe that the youth will benefit from receiving care alone. Isolation is sometimes (inaccurately) used as a punishment. As shown in Table A7, no statistically significant effects are found on the likelihood of being placed in isolation or the number of days spent in isolation.

still large and statistically significant in the subsample of youths who never run away during their first placement spell (Table A12).

If a peer group is functioning poorly (e.g., frequent overdoses and self-harming incidents), some youths may need to be moved. Such a move naturally creates a discontinuity in treatment provision as the youth who moves must establish new relationships with treatment providers. Such discontinuities in treatment can have adverse effects on the youth's recovery process. However, as shown in columns 2-3 of Table A7, I find no statistically significant effects of peer composition on the likelihood of switching facilities or the number of facility switches during the placement spell.

2.5.4 Quality of Care

Exposure to peers with a history of substance abuse and self-harm might adversely affect own outcomes because such peers are, possibly, especially resource-demanding and, thereby, negatively affect the quality of care available to the focal youth. For example, a youth who self-harms frequently might require close supervision around the clock, which could affect how much time staff can allocate to other youths. Hence, my findings might not be driven by a social interaction effect. To shed some light on this potential channel, I consider other groups of youths who likely require high-intensity care: youths with neurodevelopmental disorders, youths who have committed violent crimes, and youths who have committed such serious crimes that they are sentenced to serve time at NBIC (e.g., murder).

In Panel A of Table A13, I regress my main outcomes (substance abuse and self-harm, months 1-12) on the share of peers with neurodevelopmental disorders interacted with own history of substance abuse and self-harm, respectively. The point estimates are negative and not statistically significant, suggesting that exposure to neurodivergent peers does not increase the risk of experiencing adverse events related to substance abuse or self-harm after placement exit.

Being exposed to violent and hardened criminals can impact the probability of engaging in substance abuse and self-harm through multiple channels. Violent and hardened criminals can be resource-demanding in the sense that staff members may need to spend more time breaking off fights, writing incident reports, and caring for youths put in isolation. However, exposure to such youths can also impact future outcomes through an increased risk of victimization. Nevertheless, as shown in Panels B-C of Table A13, there is little evidence that exposure to peers with such backgrounds increases the risk of experiencing adverse events related to substance abuse and self-harm.

While I cannot offer conclusive evidence, these results suggest that the main results (reinforcing peer effects in substance abuse and self-harm) are not driven by exposure to resource-demanding peers.

2.5.5 Spread of Mental Illness

It has been suggested that mental disorders such as depression are "contagious", i.e. being exposed to depressed peers increases the risk of developing depression (e.g., Giulietti et al., 2022). In turn, depression and other mental disorders are predictors of self-harm and substance abuse (Beautrais, 2000; Bostwick et al., 2016). However, as shown in Table A5, there is no statistically significant effect of peer depression on the likelihood of experiencing adverse events related to substance abuse or self-harm in the year following placement. Likewise, regressing an indicator for hospitalization due to depression on peer depression interacted with own history of depression does not yield significant estimates (Table A8). In fact, the point estimated effect on future depression is negative. Hence, my results do not support the existence of depression contagion. Neither can I find any evidence that exposure to peers with a history of any other mental disorder (excluding substance abuse and self-harm) adversely affects own mental health. Nevertheless, I cannot rule out the existence of peer effects in depression or other mental disorders. For example, I only measure outcomes during the year after placement exit, but it may take several years before an effect on hospitalizations from depression can be detected.

2.5.6 Networks, Availability, and Learning

Placement with a higher share of youths with a history of substance abuse may expand the youth's network of dealers and increase the availability of addictive substances. This channel might be especially relevant if the youth moves to a new county because their capacity to smuggle addictive substances into NBIC is likely lower if the facility is far from their network of dealers. However, as shown in Table 2.6, there is no statistically significant difference in the reinforcing peer effect in substance abuse by whether the youth must move to a new county.

Being exposed to a higher share of peers with a history of substance abuse and self-harm may also facilitate future substance abuse and self-harm through social learning. Youths might teach each other about heavier substances, potent drug combinations, and administration methods to maximize the desired effect, thereby increasing the risk of hospitalization and death. Likewise, there may occur youth-to-youth spread of knowledge about how to effectively self-harm with available material. If learning is an important driver, we should expect larger effects for youths with relatively low knowledge or experience of the behavior.

The lack of introductory peer effects (i.e. peer effects for youths with no history of the behavior) in the full sample speaks against social learning being a major driver of peer influence.

If knowledge and experience are correlated with age, we should expect larger peer effects in young subsamples. Indeed, there is evidence of an introductory peer effect in substance abuse among youths aged below 15 (Table 2.5). However, no further evidence suggests that the youngest youths are most adversely affected.

2.6 Conclusion

This paper examines how exposure to youths with a history of substance abuse and self-harm affects other youths' outcomes. To identify causal peer effects, I exploit novel data on youths placed in residential treatment facilities and plausibly exogenous variation in peer composition within facility-by-year cells stemming from the steady flow of new admissions and discharges.

The results yield strong evidence of reinforcing peer effects in substance abuse and self-harm. Exposing individuals with a pre-placement history of substance abuse (self-harm) to a higher share of peers with a history of the *same* behavior increases the risk of experiencing adverse events related to substance abuse (self-harm) in the year after discharge. The strong effects persist even after controlling for a rich set of alternative peer characteristics (crime, neurodevelopmental disorder, depression, other mental disorder, gender, age, and foreign background). In contrast, I find little evidence of introductory peer effects.

Heterogeneity analysis by child characteristics (gender, foreign background, and age) and placement characteristics (peer group size, crowdedness, and across-county move) yield reinforcing peer effects in all subsamples. However, the estimates are often imprecise due to the smaller sample size. Few significant differences are detected.

The reinforcing peer effect in substance abuse is primarily driven by an increase in the risk of death and hospitalization due to abuse of other addictive substances than cannabis. For self-harm, the reinforcing peer effect is driven by an increased risk of being hospitalized from self-harm.

I investigate possible mechanisms and find some evidence of adverse peer effects already during placement. In addition, there is considerable clustering in incidents. Among youths who are hospitalized for self-harm or substance abuse during placement, almost 1 in 4 are hospitalized at least once in the same month as their peer is hospitalized for the same reason. These results speak in favor of direct exposure to peer behavior being an important driver. I also find some evidence that exposure to peers with a history of substance abuse leads to disruptions of treatment provision through increased time spent on the run from NBIC.

While I cannot rule out any alternative mechanisms, I find no evidence that the results are driven by a higher share of resource-demanding peers, depressed peers, or peers who have other mental disorders (excluding substance abuse and self-harm). Neither do I find much evidence in support of an expanded network of dealers, increased drug availability, or learning being major drivers of the adverse peer effects in substance abuse and self-harm.

All in all, the adverse effects of exposure to peers with a history of substance abuse and self-harm on post-discharge outcomes are likely generated by peers reinforcing each others' addictive and self-harming behaviors through social influence.

This paper is limited to youths placed in residential facilities. The social spillovers of substance abuse, self-harm, crime, and mental disorders might be very different for other populations (e.g., adults), in other settings (e.g., schools), and for other types of relationships (e.g., parent-child interactions). Moreover, 95% of the youths in the studied sample are placed involuntarily. A study conducted using youths who are voluntarily admitted for treatment in residential facilities might yield different results. Future studies on spillovers in substance abuse and self-harm using other samples are needed.

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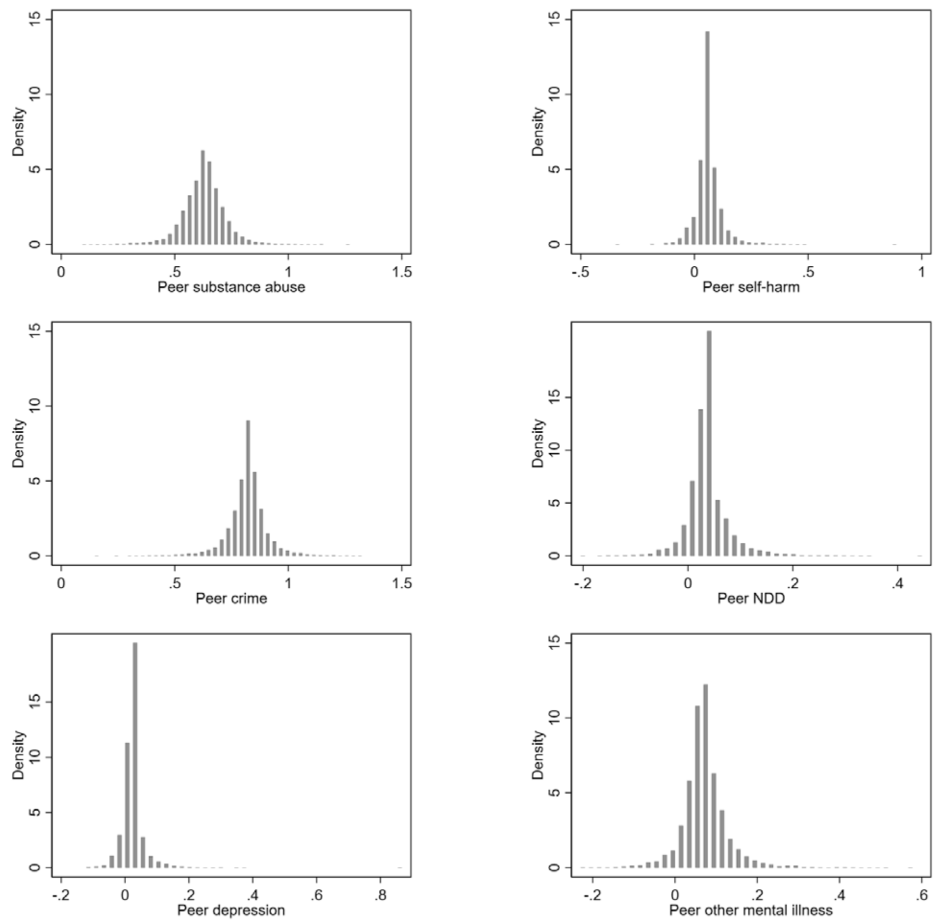
Appendix Tables and Figures

2.A Additional Descriptive Statistics, Tests, and Results

Figure A1. *Variation in Residualized Peer History*

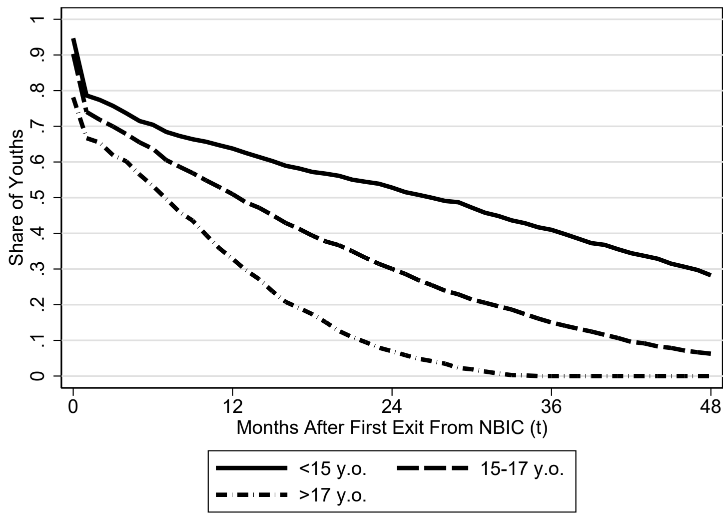


Note: Variation in peer pre-placement history of substance abuse, self-harm, crime, neurodevelopmental disorder, depression, and other mental illness, respectively, after accounting for facility-by-year FEs. The relevant peer variable is indicated on the x-axis. The '2002-2020 Sample' is used (see Section 2.2.4).

Figure A2. *Relationship Between First-Day Peer Exposure and Total Peer Exposure*

Note: This figure depicts the relationship between total and first-day peer exposure when each peer exposure variable is interacted with own history of substance abuse (plot A), no own history of substance abuse (plot B), own history of self-harm (plot C), and no own history of self-harm (plot D). The histogram in each plot depicts the distribution of own-history-specific first-day peer exposure (leaving out the top and bottom 2%). The solid lines show Kernel-weighted local polynomial regressions of total peer exposure*own history on first-day peer exposure*own history (or interacted with an indicator for no own history in plots B and D). The dashed lines show 90% confidence bands. The 'Main Analysis Sample' is used (see Section 2.2.4). The plotted values are mean-standardized residuals from regressions on facility-by-year FEs, an indicator for own history, and first-day peer exposure*no own history (or first-day peer exposure*own history in plots B and D).

Figure A3. *Share of Youths in Out-of-Home Care After First Exit*



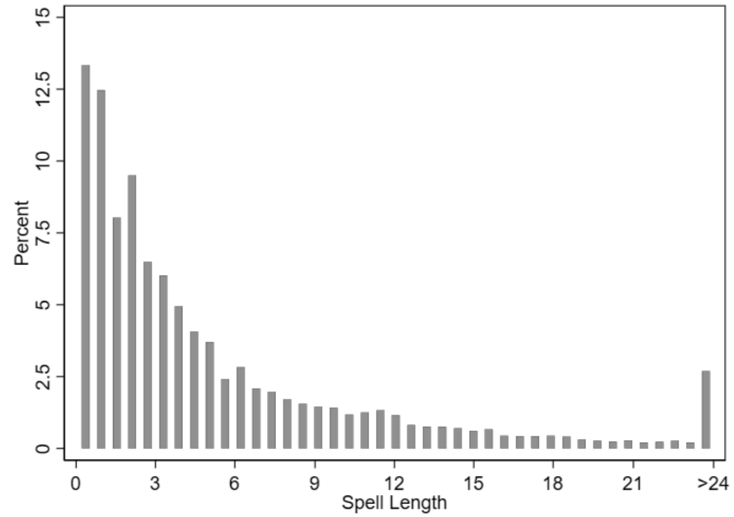
Note: This figure shows the share of youths (by age at admission) who are in any form of out-of-home care (including foster family, group home, and institution) for at least one day during month t after exiting NBIC at month 0. The 'Main Analysis Sample' is restricted to youths who exited care before 2013 since the non-NBIC placement data is known to suffer from underreporting from 2014 onward.

Table A1. *Test of Selective Attrition*

	(1) Missing in Statistics Sweden's Register Data	(2) Pacement Ended <12 Months Before end of Hospital Data	(3) Missing in Main Analysis Sample
Peer substance abuse	-0.0009	-0.0051	-0.0047
	(0.0210)	(0.0091)	(0.0229)
Peer self-harm	0.0052	0.0091	0.0055
	(0.0285)	(0.0191)	(0.0354)
Dependent mean	0.0780	0.0751	0.1538
Observations	14621	14621	14621

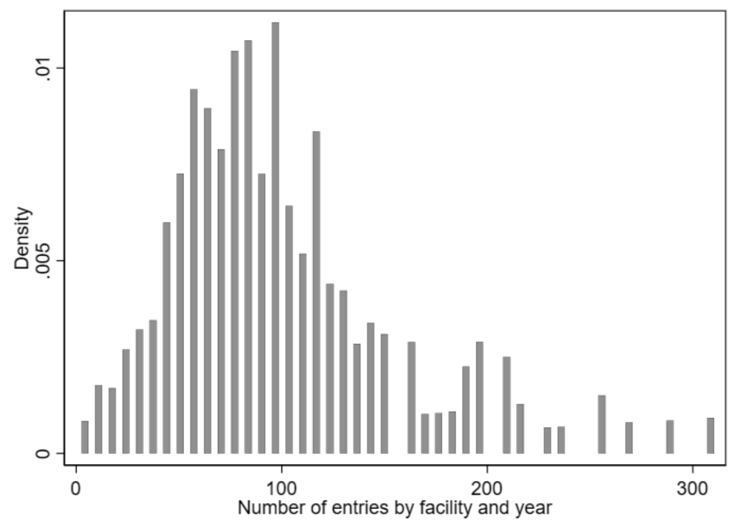
Note: I use the '2002-2020 Sample' and regress indicators for (i) youth missing in Statistics Sweden's register data, (ii) placement ended less than 12 months before the end of the hospital data (year 2020), and (ii) youth missing in the 'Main Analysis Sample' on my main peer measures. All regressions include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Figure A4. *Length of Placement Spell*



Note: This figure presents the distribution of spell lengths. I use the sample of first-time placements but restricted to placements that start before 2020 since I only have information about exit dates until the end of 2021 (N=15,562).

Figure A5. *Number of Admissions by Facility and Year*



Note: This histogram shows the number of admissions by facility and year, including admissions of youths who have been placed at NBIC before. The number of youth by facility admissions is 51,079.

Table A2. *Robustness Test of Exogenous Peer Variation*

	Predicted Substance Abuse Post-Exit		Predicted Self-Harm Post-Exit	
	(1)	(2)	(3)	(4)
Peer substance abuse	0.1453***	0.0016	0.0045*	0.0001
	(0.0058)	(0.0036)	(0.0025)	(0.0012)
Peer self-harm	0.0326*	0.0006	0.0560***	-0.0014
	(0.0168)	(0.0067)	(0.0090)	(0.0021)
Peer crime	-0.0328***	0.0035	-0.0181***	-0.0005
	(0.0087)	(0.0041)	(0.0047)	(0.0013)
Peer NDD	0.1569***	0.0004	-0.0063	0.0023
	(0.0189)	(0.0089)	(0.0091)	(0.0029)
Peer depression	-0.0295	-0.0122	-0.0206*	0.0018
	(0.0246)	(0.0109)	(0.0120)	(0.0032)
Peer other mental illness	0.0487***	0.0056	0.0038	0.0002
	(0.0144)	(0.0063)	(0.0074)	(0.0020)
Peer female	-0.0103**	-0.0020	-0.0131***	-0.0014
	(0.0051)	(0.0073)	(0.0026)	(0.0020)
Peer foreign	-0.0135*	-0.0042	-0.0161***	-0.0007
	(0.0074)	(0.0040)	(0.0027)	(0.0013)
Peer <15 y.o.	0.0456***	-0.0024	0.0009	-0.0017
	(0.0050)	(0.0043)	(0.0020)	(0.0012)
Peer missing personal id. no.	0.2427***	0.0043	0.0194***	-0.0010
	(0.0158)	(0.0096)	(0.0053)	(0.0030)
Facility*year FEs	No	Yes	No	Yes
F-statistic	142.578	0.479	13.186	0.441
p-value	0.000	0.905	0.000	0.927
Observations	12372	12372	12372	12372

Note: Test of exogenous peer variation using the 'Main Analysis Sample'. Outcomes are predicted using the full set of child and parent characteristics as indicated in Table 2.1, home county FEs, and facility-by-year FEs. In each regression, the predicted outcome is regressed with OLS on the listed peer measures and the corresponding own characteristics with or without facility-by-year FEs (as indicated in the table). Reported *F*-statistic (*p*-value) of joint significance is for the displayed variables. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A3. *Placebo Test*

	Substance Abuse, Month 1-12	Self-Harm, Month 1-12
	(1)	(2)
Own history*Peer history	0.0463	0.2175
	(0.0487)	(0.1361)
No own history*Peer history	-0.0714*	-0.0078
	(0.0421)	(0.0341)
Dependent mean if own hist.=1	0.4829	0.1500
Dependent mean if own hist.=0	0.1236	0.0278
Observations	11925	11925

Note: In this placebo test, I reproduce columns 1 and 4 of Table 2.4 but substitute the main peer measures with the share of peers with history h in facility f 180 days *before* the youth enters said facility. Youths who were placed at NBIC during the first 180 days in 2002 are dropped from the 'Main Analysis Sample' because I cannot observe the full peer group before 2002. All specifications are estimated using OLS and include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A4. *Relationship Between First-Day Peer Exposure and Total Peer Exposure*

	Total Peer Exposure		Total Peer Exposure* Own History		Total Peer Exposure* No Own History	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Exposure to Peers With History of Substance Abuse						
First-day peer history	30.4495***	29.4412***				
	(5.5183)	(5.6625)				
Own history*First-day peer history			35.1261***	33.1206***		
			(4.9745)	(5.1022)		
No own history*First-day peer history					50.3452***	51.4150***
					(5.5417)	(5.6513)
Dependent mean if own hist.=1	60.04	60.04	60.04	60.04	0.00	0.00
Dependent mean if own hist.=0	52.89	52.89	0.00	0.00	52.89	52.89
Effective *F*-statistic	30.45	27.03				
Sanderson-Windmeijer *F*-statistic			42.51	38.90	39.65	37.09
B: Exposure to Peers With History of Self-Harm						
First-day peer history	23.7104***	24.1088***				
	(2.4278)	(2.5296)				
Own history*First-day peer history			66.1406***	66.7194***		
			(10.3585)	(10.3726)		
No own history*First-day peer history					27.5745***	27.4967***
					(2.2111)	(2.2794)
Dependent mean if own hist.=1	9.78	9.78	9.78	9.78	0.00	0.00
Dependent mean if own hist.=0	5.62	5.62	0.00	0.00	5.62	5.62
Effective *F*-statistic	95.38	90.83				
Sanderson-Windmeijer *F*-statistic			84.93	84.34	104.83	98.51
Peer controls	No	Yes	No	Yes	No	Yes
Observations	12372	12372	12372	12372	12372	12372

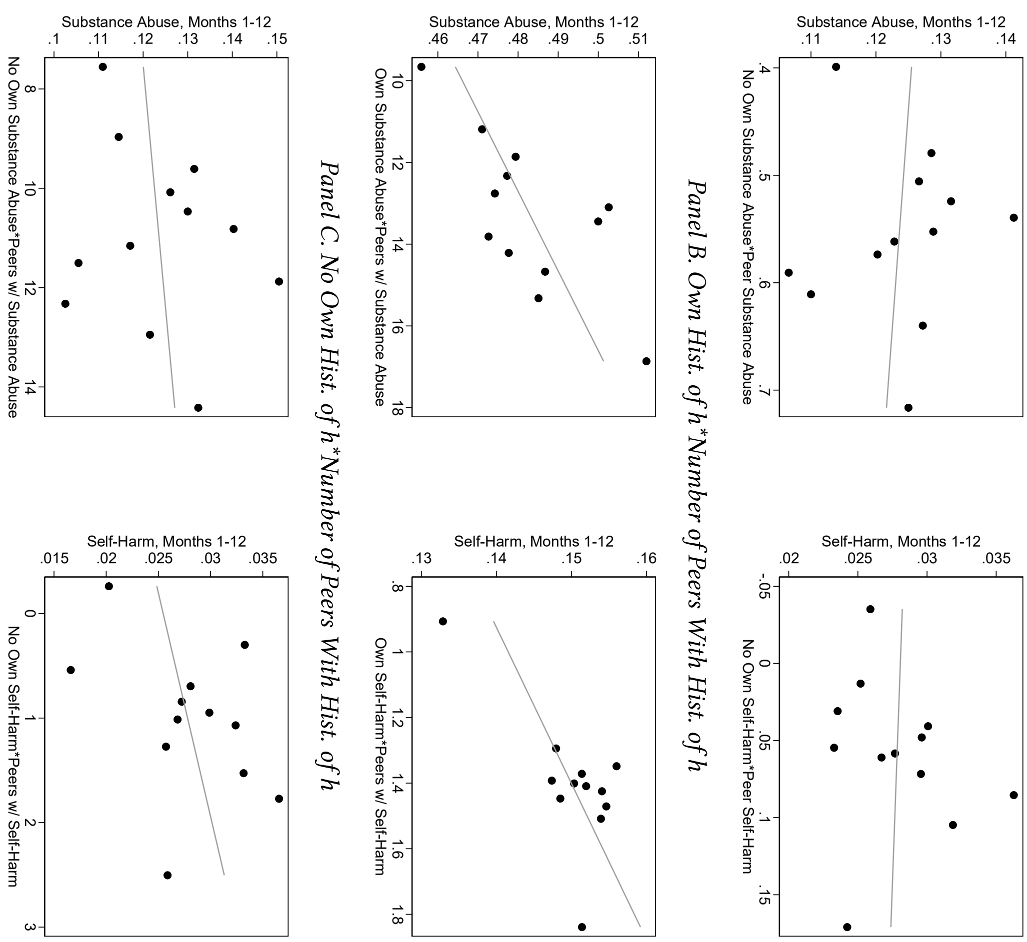
Note: The dependent variable in columns 1-2 is the youth's total exposure to peers with history h (as indicated in the panel heading) during their first placement spell. Total exposure is regressed on the share of peers with history h on the youth's first day. Olea and Pflueger (2013)'s effective F-statistic is reported. The dependent variable in columns 3-4 (5-6) is the interaction of total peer exposure with an indicator taking the value 1 if the youth has (does not have) a history of the same behavior. Each dependent variable in columns 3-6 is regressed on peer exposure on the youth's first day interacted with the youth's own history of the behavior. All estimations include facility-by-year FEs. Sanderson and Windmeijer (2016)'s F-statistics are reported. The "Main Analysis Sample" is used. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Figure A6. *Relationship Between Peer History and Post-Exit Outcomes*

*Panel A. No Own Hist. of h*Share of Peers With Hist. of h*

*Panel B. Own Hist. of h*Number of Peers With Hist. of h*

*Panel C. No Own Hist. of h*Number of Peers With Hist. of h*



Note: Black dots represent the risk of experiencing an adverse event related to substance abuse and self-harm post-placement over the share of first-day peers with a history of the corresponding behavior interacted with no own history (Panel A), number of first-day peers with a history of the corresponding behavior interacted with own history (Panel B), and number of first-day peers with a history of the corresponding behavior interacted with no own history (Panel C). All values are residualized and mean-standardized using facility-by-year FEs, (no) own history interacted with peer exposure, and (no) own history.

Table A5. *Effect of All First-Day Peer Characteristics on Post-Exit Outcomes*

	Substance Abuse, Month 1-12 (1)	Self-Harm, Month 1-12 (2)
Substance abuse*Peer substance abuse	0.1088**	0.0053
	(0.0502)	(0.0215)
Not substance abuse*Peer substance abuse	-0.0338	-0.0036
	(0.0438)	(0.0209)
Self-harm*Peer self-harm	-0.1790	0.4097**
	(0.2002)	(0.1595)
Not self-harm*Peer self-harm	0.0145	0.0118
	(0.0788)	(0.0328)
Crime*Peer crime	-0.0407	-0.0135
	(0.0526)	(0.0239)
Not crime*Peer crime	-0.0173	0.0038
	(0.0586)	(0.0313)
NDD*Peer NDD	0.3200	0.1775
	(0.3768)	(0.2964)
Not NDD*Peer NDD	-0.1195	0.0127
	(0.1085)	(0.0476)
Depression*Peer depression	-0.0570	-0.2587
	(0.5605)	(0.3820)
Not depression*Peer depression	0.0750	-0.0625
	(0.1278)	(0.0597)
Other mental illness*Peer other mental illness	0.2271	-0.0308
	(0.1722)	(0.1247)
Not other mental illness*Peer other mental illness	-0.0020	-0.0527
	(0.0752)	(0.0349)
Female*Peer female	-0.0232	-0.0305
	(0.0768)	(0.0343)
Not female*Peer female	0.1311*	-0.0690**
	(0.0731)	(0.0322)
Foreign*Peer foreign	-0.0447	0.0267
	(0.0531)	(0.0239)
Not foreign*Peer foreign	-0.0015	0.0296
	(0.0472)	(0.0219)
<15 y.o.*Peer <15 y.o.	-0.0758	0.0043
	(0.0510)	(0.0261)
Not <15 y.o.*Peer <15 y.o.	-0.0447	-0.0168
	(0.0496)	(0.0254)
Peer missing personal id. no.	0.0704	-0.0740
	(0.1139)	(0.0452)
Dependent mean if own hist.=1	0.4829	0.1500
Dependent mean if own hist.=0	0.1236	0.0278
Observations	12372	12372

Note: The dependent variables are the same as in Table 2.4. Each dependent variable is regressed on first-day peer exposure interacted with the youth's own history of the corresponding behavior. All specifications are estimated using OLS and include facility-by-year FEs. The dependent mean is conditional on the youth's own history of substance abuse (column 1) and self-harm (column 2). The 'Main Analysis Sample' is used. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A6. *Effect of First-Day Peer Composition on Living-Situation Post-Exit*

				Out-of-Home Care (Excl. NBIC)		
	(1) Readmission To NBIC	(2) Prison	(3) Adult Addic. Treatment	(4) Foster Care	(5) Group Home	(6) Other OOH Care Facility
A: History of Substance Abuse						
Own history*Peer history	0.1212**	0.0250	0.0307	-0.1178*	0.0225	0.0836
	(0.0528)	(0.0924)	(0.0348)	(0.0617)	(0.0695)	(0.0572)
No own history*Peer history	0.0365	0.1983	-0.0094	0.0037	-0.0560	0.0045
	(0.0521)	(0.1222)	(0.0298)	(0.0595)	(0.0679)	(0.0534)
Dependent mean if own hist.=1	0.3492	0.1141	0.0116	0.2361	0.5566	0.2558
Dependent mean if own hist.=0	0.2733	0.1419	0.0000	0.2567	0.4869	0.1980
B: History of Self-Harm						
Own history*Peer history	-0.1896	-0.0308	-0.0210	0.2749	-0.2154	-0.0360
	(0.1944)	(0.1631)	(0.0953)	(0.2342)	(0.2573)	(0.2337)
No own history*Peer history	-0.1235	-0.0123	0.0439	0.0802	0.1091	-0.2007**
	(0.0823)	(0.1011)	(0.0644)	(0.0928)	(0.1053)	(0.0852)
Dependent mean if own hist.=1	0.3166	0.0498	0.0263	0.2295	0.6208	0.2633
Dependent mean if own hist.=0	0.3168	0.1257	0.0076	0.2470	0.5173	0.2257
Observations	12327	2675	2496	7946	7946	7946

Note: All dependent variables are measured as indicators taking the value 1 if the youth ever enters a placement of the form indicated at the top of the table during the 12 months post-placement. Each dependent variable is first regressed on peer substance abuse interacted with own history of substance abuse (Panel A) and then peer self-harm interacted with own history of self-harm (Panel B). The 'Main Analysis Sample' is subject to restrictions depending on the outcome. Youths who die during the 12 months post-placement are excluded in all columns. Columns 2-3 exclude youths aged below 18 at the time of placement exit because being sentenced to prison and admission to adult addiction treatment are very rare outcomes for youths aged below 18. Columns 4-6 exclude youths who exited their first NBIC placement after 2012 because the register on non-NBIC placements suffers from underreporting from 2014 onward. All estimations include facility-by-year FEs and the full set of peer characteristics listed in Table 2.1. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A7. *Effect of First-Day Peer Composition on Placement Outcomes*

	Total	Facility Switch		Isolation		Runaway	
	(1) Days	(2) Ever	(3) Count	(4) Ever	(5) Days	(6) Ever	(7) Days
A: History of Substance Abuse							
Own history*Peer history	53.8995***	0.0495	0.0729	0.0379	-1.3806	0.0488	3.8723**
	(18.8101)	(0.0457)	(0.0962)	(0.0421)	(4.1363)	(0.0399)	(1.9560)
No own history*Peer history	-7.3880	0.0287	0.0641	-0.0339	-9.9280	-0.0281	-0.5963
	(19.4169)	(0.0461)	(0.0938)	(0.0410)	(7.0168)	(0.0378)	(1.8623)
Dependent mean if own hist.=1	145.4750	0.3186	1.5340	0.2378	5.7352	0.1974	4.5782
Dependent mean if own hist.=0	152.3112	0.2604	1.4199	0.1904	9.0007	0.1631	3.7076
B: History of Self-Harm							
Own history*Peer history	84.6300	-0.1220	-0.2253	-0.0210	-4.0581	0.0789	9.6968
	(82.5376)	(0.1757)	(0.3519)	(0.1755)	(15.8744)	(0.1583)	(6.1019)
No own history*Peer history	-18.3110	0.0500	0.0475	0.0769	-1.6821	-0.0477	-0.0841
	(26.3875)	(0.0742)	(0.1584)	(0.0676)	(5.2750)	(0.0615)	(2.6047)
Dependent mean if own hist.=1	153.8599	0.2830	1.4203	0.2730	10.3288	0.2143	3.2898
Dependent mean if own hist.=0	148.0955	0.2944	1.4889	0.2150	6.9320	0.1812	4.2561
Observations	14620	14621	14621	13076	13076	14621	14621

Note: Each dependent variable listed in the top row is first regressed on peer substance abuse interacted with own history of substance abuse (Panel A) and then peer self-harm interacted with own history of self-harm (Panel B). The '2002-2020 Sample' is used. The sample is smaller in columns 4-5 because registration of isolation episodes did not start until 2004. All estimations include facility-by-year FEs and the full set of peer characteristics listed in Table 2.1. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A8. *Effect of Peers on Other Outcomes, Months 1-12*

	(1) Crime	(2) Depression	(3) Any Mental Disorder Excl. Sub. Abuse & Self-Harm
Own history*Peer history	0.0243	-0.1317	0.0911
	(0.0649)	(0.1939)	(0.0763)
No own history*Peer history	-0.0644	-0.0181	-0.0324
	(0.0697)	(0.0240)	(0.0243)
Dependent mean if own hist.=1	0.4435	0.0536	0.1124
Dependent mean if own hist.=0	0.2025	0.0056	0.0150
Test of equality (p-value)	0.1821	0.5539	0.0896
Observations	9518	12327	12298

Note: The dependent variable in column 1 is an indicator equal to one if the individual commits a crime for which guilt is established or is readmitted due to their criminal behavior during the 1-12 months after exit from the first placement spell at NBIC. In column 1, the 'Main Analysis Sample' is restricted to youths who were at least 15 when they were first admitted to NBIC. The dependent variable in column 2 (3) is an indicator equal to one if the individual is hospitalized due to depression (any mental disorder excluding substance abuse and self-harm) during the 1-12 months after exit. Each dependent variable is regressed on first-day peer exposure interacted with the youth's history of the behavior indicated in the top row. All specifications are estimated using OLS and include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A9. *Effect of Peers on Crime by Category, Months 1-12*

	(1) Violent Sexual	(2) Other Against People	(3) Theft	(4) White Collar	(5) Vandalism	(6) Other
A: Committed Violent or Sexual Crime						
Own history*Peer history	0.0078	0.0270	0.0064	0.0343	0.0087	0.0427
	(0.0524)	(0.0318)	(0.0507)	(0.0311)	(0.0317)	(0.0287)
No own history*Peer history	0.0351	-0.0119	0.0820*	0.0035	0.0271	0.0151
	(0.0426)	(0.0244)	(0.0418)	(0.0242)	(0.0265)	(0.0245)
B: Committed Other Crime Against Persons						
Own history*Peer history	0.0919	0.0096	-0.0041	-0.0291	0.0474	-0.0295
	(0.1082)	(0.0721)	(0.0998)	(0.0628)	(0.0708)	(0.0616)
No own history*Peer history	0.0508	-0.0150	-0.1069*	-0.0161	0.0064	0.0113
	(0.0563)	(0.0327)	(0.0571)	(0.0332)	(0.0316)	(0.0276)
C: Committed Crime of Stealing						
Own history*Peer history	0.0047	0.0423	0.0212	0.0374	-0.0007	-0.0310
	(0.0512)	(0.0310)	(0.0538)	(0.0329)	(0.0305)	(0.0280)
No own history*Peer history	-0.0037	-0.0171	-0.0842**	-0.0266	-0.0127	0.0024
	(0.0400)	(0.0246)	(0.0411)	(0.0211)	(0.0234)	(0.0224)
D: Committed White-Collar Crime						
Own history*Peer history	0.0845	-0.1970**	0.1710	0.1589	0.0286	-0.0485
	(0.1522)	(0.0802)	(0.1650)	(0.1188)	(0.0712)	(0.0753)
No own history*Peer history	0.0066	-0.0244	-0.0082	-0.0128	0.0166	-0.0610**
	(0.0599)	(0.0412)	(0.0657)	(0.0362)	(0.0427)	(0.0297)
E: Committed Vandalism						
Own history*Peer history	-0.1340	-0.0043	-0.0825	0.0656	0.1452	-0.1055
	(0.1487)	(0.0917)	(0.1386)	(0.0931)	(0.1018)	(0.0701)
No own history*Peer history	-0.1295**	-0.0729**	-0.0488	-0.0795**	0.0309	-0.0380
	(0.0637)	(0.0366)	(0.0629)	(0.0390)	(0.0379)	(0.0337)
F: Committed Other Non-Narcotic Crime						
Own history*Peer history	0.0101	0.0046	0.0002	0.0160	-0.0220	-0.0021
	(0.0406)	(0.0245)	(0.0400)	(0.0233)	(0.0246)	(0.0218)
No own history*Peer history	0.0183	-0.0368	-0.0438	-0.0415**	-0.0205	0.0127
	(0.0370)	(0.0241)	(0.0386)	(0.0204)	(0.0221)	(0.0201)
Dependent mean if own hist.=1	0.2136	0.0791	0.2103	0.1032	0.0881	0.0729
Dependent mean if own hist.=0	0.1179	0.0430	0.1040	0.0370	0.0372	0.0356
Observations	9518	9518	9518	9518	9518	9518

Note: Each dependent variable at the top of the table is regressed on the peer history measure indicated in the panel heading interacted with own history. The dependent variables are indicators taking the value 1 if the youth commits such a crime 1-12 months after placement exit. The 'Main Analysis Sample' is used but limited to youths who are at least 15 at the time of intake. All estimations include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A10. *Effect of Gender-Specific Peer Exposure on Post-Exit Outcomes*

	(1) Substance Abuse, Month 1-12	(2) Self-Harm, Month 1-12
Own history*Peer history (same sex)	0.1320***	0.4210***
	(0.0495)	(0.1613)
No own history*Peer history (same sex)	-0.0028	-0.0014
	(0.0431)	(0.0340)
Own history*Peer history (opp. sex)	-0.0167	-0.4004
	(0.0727)	(0.4634)
No own history*Peer history (opp. sex)	0.0134	-0.0156
	(0.0573)	(0.0540)
Dependent mean if own hist.=1	0.4829	0.1500
Dependent mean if own hist.=0	0.1236	0.0278
$\beta_1 = \beta_2$ (p-value)	0.0010	0.0081
$\beta_1 = \beta_3$ (p-value)	0.0150	0.0721
$\beta_3 = \beta_4$ (p-value)	0.6459	0.4058
Observations	12372	12372

Note: The dependent variable (indicated in the panel heading) is any adverse event related to substance abuse or self-harm (respectively) during the 1-12 months after exit from the first placement spell at NBIC. *Peer history (same sex)* is defined as the share of peers with history h and the same sex as the focal youth on the focal youth's day of entry, while *Peer history (opp. sex)* is the share of peers with history h and the opposite sex on the focal youth's day of entry. Each dependent variable is regressed on these peer exposure measures interacted with the youth's history of the behavior. The 'Main Analysis Sample' is used. All specifications are estimated using OLS and control for own history, sex, and facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A11. *Effect of Foreign-Background-Specific Peer Exposure on Post-Exit Outcomes*

	(1) Substance Abuse, Month 1-12	(2) Self-Harm, Month 1-12
Own history*Peer history (same background)	0.1286**	0.3648**
	(0.0503)	(0.1686)
No own history*Peer history (same background)	0.0172	-0.0330
	(0.0439)	(0.0338)
Own history*Peer history (opp. background)	0.0984*	0.5661
	(0.0578)	(0.3467)
No own history*Peer history (opp. background)	-0.0694	0.0822
	(0.0504)	(0.0548)
Dependent mean if own hist.=1	0.4829	0.1500
Dependent mean if own hist.=0	0.1236	0.0278
$\beta_1 = \beta_2$ (p-value)	0.0111	0.0173
$\beta_1 = \beta_3$ (p-value)	0.4661	0.5746
$\beta_3 = \beta_4$ (p-value)	0.0006	0.1633
Observations	12372	12372

Note: The dependent variable (indicated in the panel heading) is any adverse event related to substance abuse or self-harm (respectively) during the 1-12 months after exit from the first placement spell at NBIC. *Peer history (same background)* is defined as the share of peers with history h and the same background (foreign or native) as the focal youth on the focal youth's day of entry, while *Peer history (opp. background)* is the share of peers with history h and the opposite background on the focal youth's day of entry. Each dependent variable is regressed on these peer exposure measures interacted with the youth's history of the behavior. The 'Main Analysis Sample' is used. All specifications are estimated using OLS and control for own history, foreign background, and facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A12. *Main Results by Whether the Youths Ever Escape or Placed in Isolation*

	Ever Isolation			Ever Runaway			Ever Isolation or Runaway		
	(1) Yes	(2) No	(3) Δ	(4) Yes	(5) No	(6) Δ	(7) Yes	(8) No	(9) Δ
A: Substance Abuse, Months 1-12									
Substance abuse*Peer substance abuse	0.1169	0.1396**	-0.0227	0.2129	0.1062**	0.1067	0.0745	0.1651***	-0.0906
	(0.1537)	(0.0574)	(0.1561)	(0.1438)	(0.0535)	(0.1446)	(0.1158)	(0.0610)	(0.1277)
Not substance abuse*Peer substance abuse	-0.0501	0.0064	-0.0564	0.1175	-0.0273	0.1448	-0.0923	0.0130	-0.1053
	(0.1595)	(0.0488)	(0.1585)	(0.1519)	(0.0441)	(0.1487)	(0.1217)	(0.0501)	(0.1282)
Dependent mean if own hist.=1	0.5775	0.4716		0.5531	0.4666		0.5570	0.4645	
Dependent mean if own hist.=0	0.2238	0.1111		0.2143	0.1064		0.2064	0.1035	
Observations	1964	8826	10790	2108	10184	12292	3066	7745	10811
B: Self-Harm, Months 1-12									
Self-harm*Peer self-harm	0.3000	0.3904**	-0.0904	0.5168	0.2561	0.2607	0.3726	0.2629	0.1097
	(0.3380)	(0.1865)	(0.3713)	(0.3854)	(0.1781)	(0.4082)	(0.2656)	(0.1998)	(0.3285)
Not self-harm*Peer self-harm	-0.0758	-0.0059	-0.0699	0.1075	-0.0304	0.1379	0.0526	-0.0470	0.0996
	(0.1071)	(0.0342)	(0.1070)	(0.1005)	(0.0329)	(0.0997)	(0.0817)	(0.0358)	(0.0871)
Dependent mean if own hist.=1	0.1724	0.1378		0.1915	0.1387		0.1619	0.1377	
Dependent mean if own hist.=0	0.0415	0.0253		0.0334	0.0267		0.0367	0.0250	
Observations	1964	8826	10790	2108	10184	12292	3066	7745	10811

Note: This table replicates the main results (Table 2.4) but in the subsamples indicated at the top of the table (i.e. by whether the youths ever get put in isolation or run away during their first placement spell). The dependent variable (indicated in the panel heading) is any adverse event related to substance abuse or self-harm (respectively) during the 1-12 months after exit from the first placement spell at NBIC. Each dependent variable is regressed on first-day peer exposure interacted with the youth's own history. Periods of isolation were not registered before 2004. Hence, youths who were admitted to NBIC before 2004 are dropped from the samples in columns 1-3 and 7-9. All specifications are estimated using OLS and include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A13. *Effect of Resource-Demanding Peers, Months 1-12*

	(1) Substance Abuse Month 1-12	(2) Self-Harm Month 1-12
A: Peer History of Neurodevelopmental Disorder		
Own hist. of Y*Peer NDD	-0.1118	-0.0888
	(0.1302)	(0.2233)
No own hist. of Y*Peer NDD	-0.1430	-0.0023
	(0.1164)	(0.0473)
B: Peer History of Violent Crime		
Own hist. of Y*Peer violent crime	0.0470	-0.0979
	(0.0500)	(0.0619)
No own hist. of Y*Peer violent crime	-0.1211**	0.0155
	(0.0501)	(0.0200)
C: Peer History of Being Sentenced to Serve Time at NBIC		
Own hist. of Y*Peer sentenced to serve	0.1030	0.0547
	(0.1033)	(0.0922)
No own hist. of Y*Peer sentenced to serve	-0.0470	0.0857*
	(0.1022)	(0.0450)
Dependent mean if own hist.=1	0.4829	0.1500
Dependent mean if own hist.=0	0.1236	0.0278
Test of equality (*p*-value)	0.0030	0.7134
Observations	12372	12372

Note: Each dependent variable at the top of the table is regressed on the peer history measure indicated in the panel heading interacted with own history of substance abuse (column 1) and self-harm (column 2). The 'Main Analysis Sample' is used. All estimations include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

2.B Robustness Checks of Main Results

Table C1. *Robustness Checks of Effect of Peers on Post-Exit Outcomes I*

	Substance Abuse, Month 1-12		Self-Harm, Month 1-12	
	Coeff	Std err	Coeff	Std err
A: Baseline				
Own history*Peer history	0.1247**	0.0490	0.4032**	0.1609
No own history*Peer history	-0.0123	0.0421	-0.0040	0.0310
Observations	12372		12372	
B: With Child, Parent, Peer History, and Crime Rate Controls				
Own history*Peer history	0.0994**	0.0505	0.4330***	0.1595
No own history*Peer history	-0.0171	0.0441	0.0099	0.0329
Observations	12180		12180	
C: With Control for Leave-out Mean History Within Facility-by-Year Cell				
Own history*Peer history	0.1248**	0.0490	0.3945**	0.1621
No own history*Peer history	-0.0120	0.0421	-0.0030	0.0310
Observations	12372		12372	
D: Clustering at Facility-by-Year Level				
Own history*Peer history	0.1247**	0.0507	0.4032***	0.1485
No own history*Peer history	-0.0123	0.0458	-0.0040	0.0279
Observations	12372		12372	
E: Facility-by-Year FEs Replaced by Facility-by-Quarter FEs				
Own history*Peer history	0.1628**	0.0670	0.4259**	0.1680
No own history*Peer history	0.0404	0.0636	0.0026	0.0489
Observations	12189		12189	
F: Facility-by-Year FEs Replaced by Facility FEs & Year FEs				
Own history*Peer history	0.1078***	0.0401	0.4229***	0.1522
No own history*Peer history	-0.0212	0.0319	0.0327	0.0276
Observations	12372		12372	
G: Facility-by-Year FEs Replaced by Facility-by-History & Year FEs				
Own history*Peer history	0.0839*	0.0458	0.3651**	0.1825
No own history*Peer history	0.0024	0.0329	0.0405	0.0270
Observations	12372		12369	

Note: Panel A is the baseline with only facility-by-year FEs. Panel B includes controls for the full sets of child and parent characteristics and peer history measures (see Table 2.1) and crime rate in the child's home municipality the month before intake. Panel C controls for the leave-out mean history within the facility-by-year cell. Panel D clusters the standard errors at the facility-by-year level. Finally, in Panels E-G, the facility-by-year FEs included in the baseline are replaced by facility-by-quarter FEs; facility and year FEs; and facility-by-history and year FEs. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table C2. *Robustness Checks of Effect of Peers on Post-Exit Outcomes II*

	Substance Abuse, Month 1-12		Self-Harm, Month 1-12	
	Coeff	Std err	Coeff	Std err
A: Placements >14 Days				
Own history*Peer history	0.0921*	0.0520	0.5109***	0.1681
No own history*Peer history	-0.0434	0.0455	-0.0004	0.0318
Observations	11047		11047	
B: No Placements with Imputed Exit Dates				
Own history*Peer history	0.1262**	0.0490	0.4032**	0.1609
No own history*Peer history	-0.0111	0.0421	-0.0012	0.0309
Observations	12351		12351	
C: Placements Starting in 2003 or Later				
Own history*Peer history	0.1207**	0.0508	0.3598**	0.1647
No own history*Peer history	-0.0088	0.0442	0.0110	0.0318
Observations	11597		11597	
D: Placements Starting Before 2019				
Own history*Peer history	0.1191**	0.0496	0.3994**	0.1658
No own history*Peer history	-0.0230	0.0423	-0.0002	0.0314
Observations	11960		11960	
E: No Siblings Placed at Same Facility at the Same Time				
Own history*Peer history	0.1235**	0.0490	0.4081**	0.1614
No own history*Peer history	-0.0150	0.0422	-0.0049	0.0311
Observations	12342		12342	
F: Share of Peers With History h on Day After Entry				
Own history*Peer history	0.1133**	0.0488	0.4593***	0.1595
No own history*Peer history	-0.0232	0.0418	0.0256	0.0335
Observations	12372		12372	
G: Average Share of Peers With History h During First 0-2 Days After Entry				
Own history*Peer history	0.1105**	0.0502	0.4949***	0.1675
No own history*Peer history	-0.0282	0.0433	0.0237	0.0333
Observations	12015		12015	
H: Number of Peers With History h on First Day of Entry				
Own history*Peer history	0.0052**	0.0021	0.0211*	0.0108
No own history*Peer history	0.0010	0.0021	0.0023	0.0022
Observations	12372		12372	

Note: All estimations include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. The following samples are used: placements longer than 14 days (Panel A), placements without imputed exit dates (Panel B), placements starting in 2003 or later (Panel C), placements starting before 2019 (Panel D), and placements of youths with no full or half siblings placed at the same facility at the same time (Panel E). In Panels F-H, the main peer measure is replaced with: the share of peers with history h on the day after entry (Panel F), the average share of peers with history h during the first 0-2 days after entry (Panel G), and the number of peers with history h on the first day of entry (Panel H). * $p < .1$. ** $p < .05$. *** $p < .01$.

Table C3. *Robustness of Main Results to Dropping a Facility I*

	Substance Abuse, Month 1-12		Self-Harm, Month 1-12	
	Coeff	Std err	Coeff	Std err
arnhem				
Own history*Peer history	0.1346***	0.0498	0.4102**	0.1693
No own history*Peer history	-0.0160	0.0427	0.0012	0.0333
backebro				
Own history*Peer history	0.1247**	0.0490	0.4032**	0.1609
No own history*Peer history	-0.0123	0.0421	-0.0040	0.0310
bergsmansgården				
Own history*Peer history	0.1151**	0.0497	0.3714**	0.1667
No own history*Peer history	-0.0275	0.0425	0.0067	0.0317
björkbacken				
Own history*Peer history	0.1127**	0.0496	0.4100**	0.1638
No own history*Peer history	-0.0124	0.0425	-0.0020	0.0313
brättegården				
Own history*Peer history	0.1200**	0.0494	0.4464***	0.1689
No own history*Peer history	-0.0102	0.0425	-0.0095	0.0314
bärby				
Own history*Peer history	0.1261**	0.0502	0.3990**	0.1615
No own history*Peer history	-0.0252	0.0428	-0.0011	0.0312
dockan				
Own history*Peer history	0.1204**	0.0492	0.4143***	0.1608
No own history*Peer history	-0.0081	0.0424	-0.0030	0.0315
eknäs				
Own history*Peer history	0.1221**	0.0503	0.3925**	0.1626
No own history*Peer history	-0.0168	0.0429	0.0009	0.0317
fagared				
Own history*Peer history	0.1234**	0.0493	0.3964**	0.1622
No own history*Peer history	-0.0203	0.0424	-0.0059	0.0312
familjehuset				
Own history*Peer history	0.1233**	0.0490	0.4032**	0.1609
No own history*Peer history	-0.0129	0.0422	-0.0040	0.0310
folåsa				
Own history*Peer history	0.1274***	0.0494	0.4066**	0.1621
No own history*Peer history	-0.0078	0.0423	-0.0032	0.0316
perstorp				
Own history*Peer history	0.1257**	0.0490	0.4034**	0.1609
No own history*Peer history	-0.0082	0.0421	-0.0041	0.0310
fridegård				
Own history*Peer history	0.1245**	0.0516	0.3944**	0.1644
No own history*Peer history	-0.0008	0.0448	0.0150	0.0323

Note: The main regressions are reestimated while leaving out the facility indicated in the panel heading. All estimations include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table C4. *Robustness of Main Results to Dropping a Facility II*

	Substance Abuse, Month 1-12		Self-Harm, Month 1-12	
	Coeff	Std err	Coeff	Std err
granhult				
Own history*Peer history	0.1291***	0.0490	0.4152***	0.1608
No own history*Peer history	-0.0096	0.0421	-0.0040	0.0314
holmängens				
Own history*Peer history	0.1261**	0.0491	0.4056**	0.1627
No own history*Peer history	-0.0121	0.0421	-0.0055	0.0311
hässleholm				
Own history*Peer history	0.1606***	0.0516	0.4017**	0.1616
No own history*Peer history	0.0113	0.0460	-0.0059	0.0317
håkanstorps				
Own history*Peer history	0.1259**	0.0492	0.4024**	0.1614
No own history*Peer history	-0.0091	0.0421	-0.0031	0.0312
högantorps				
Own history*Peer history	0.1124**	0.0506	0.3993**	0.1633
No own history*Peer history	-0.0332	0.0439	-0.0045	0.0319
johannisberg				
Own history*Peer history	0.1357***	0.0494	0.4081**	0.1615
No own history*Peer history	-0.0060	0.0423	-0.0063	0.0312
klarälvsgården				
Own history*Peer history	0.1293***	0.0491	0.4023**	0.1610
No own history*Peer history	-0.0125	0.0422	-0.0039	0.0311
perstorp				
Own history*Peer history	0.1257**	0.0490	0.4034**	0.1609
No own history*Peer history	-0.0082	0.0421	-0.0041	0.0310
klockbacka				
Own history*Peer history	0.1268**	0.0497	0.4026**	0.1612
No own history*Peer history	-0.0103	0.0430	-0.0014	0.0313

Note: The main regressions are reestimated while leaving out the facility indicated in the panel heading. All estimations include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table C5. *Robustness of Main Results to Dropping a Facility III*

	Substance Abuse, Month 1-12		Self-Harm, Month 1-12	
	Coeff	Std err	Coeff	Std err
ljungaskog				
Own history*Peer history	0.1255**	0.0492	0.4028**	0.1624
No own history*Peer history	-0.0126	0.0421	-0.0055	0.0310
ljungbacken				
Own history*Peer history	0.1193**	0.0495	0.4047**	0.1622
No own history*Peer history	-0.0041	0.0424	0.0022	0.0314
lunden				
Own history*Peer history	0.1223**	0.0491	0.4322**	0.1700
No own history*Peer history	-0.0122	0.0421	-0.0093	0.0324
långanäs				
Own history*Peer history	0.1159**	0.0493	0.4033**	0.1614
No own history*Peer history	-0.0099	0.0422	-0.0068	0.0312
lövsta				
Own history*Peer history	0.1202**	0.0495	0.3999**	0.1627
No own history*Peer history	-0.0149	0.0424	-0.0075	0.0314
margretelund				
Own history*Peer history	0.1102**	0.0500	0.3914**	0.1627
No own history*Peer history	-0.0132	0.0429	-0.0036	0.0313
nereby				
Own history*Peer history	0.1252**	0.0499	0.4048**	0.1616
No own history*Peer history	-0.0178	0.0428	-0.0070	0.0311
perstorp				
Own history*Peer history	0.1257**	0.0490	0.4034**	0.1609
No own history*Peer history	-0.0082	0.0421	-0.0041	0.0310
rebecka				
Own history*Peer history	0.1241**	0.0493	0.3918**	0.1615
No own history*Peer history	-0.0180	0.0422	0.0014	0.0311
ryds_brunn				
Own history*Peer history	0.1326***	0.0501	0.3959**	0.1618
No own history*Peer history	-0.0022	0.0429	-0.0032	0.0312
råby				
Own history*Peer history	0.1277**	0.0502	0.4163**	0.1630
No own history*Peer history	-0.0120	0.0428	-0.0130	0.0317

Note: The main regressions are reestimated while leaving out the facility indicated in the panel heading. All estimations include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table C6. *Robustness of Main Results to Dropping a Facility IV*

	Substance Abuse, Month 1-12		Self-Harm, Month 1-12	
	Coeff	Std err	Coeff	Std err
sirius				
Own history*Peer history	0.1243**	0.0493	0.4257***	0.1637
No own history*Peer history	-0.0125	0.0421	0.0049	0.0326
perstorp				
Own history*Peer history	0.1257**	0.0490	0.4034**	0.1609
No own history*Peer history	-0.0082	0.0421	-0.0041	0.0310
solgården				
Own history*Peer history	0.1270**	0.0499	0.4231***	0.1631
No own history*Peer history	-0.0108	0.0429	-0.0097	0.0320
stigby				
Own history*Peer history	0.1190**	0.0494	0.3958**	0.1618
No own history*Peer history	-0.0089	0.0423	-0.0035	0.0311
sundbo				
Own history*Peer history	0.1265**	0.0493	0.4044**	0.1619
No own history*Peer history	-0.0113	0.0422	-0.0027	0.0313
sävastgården				
Own history*Peer history	0.1222**	0.0507	0.4322***	0.1645
No own history*Peer history	-0.0210	0.0434	-0.0138	0.0314
tunagården				
Own history*Peer history	0.1256**	0.0490	0.4032**	0.1609
No own history*Peer history	-0.0107	0.0421	-0.0040	0.0310
tysslinge				
Own history*Peer history	0.1221**	0.0494	0.3941**	0.1617
No own history*Peer history	-0.0134	0.0422	-0.0056	0.0311
vemyra				
Own history*Peer history	0.1236**	0.0492	0.3635**	0.1746
No own history*Peer history	-0.0149	0.0423	-0.0101	0.0306
villa_ljungbacken				
Own history*Peer history	0.1265**	0.0493	0.3991**	0.1616
No own history*Peer history	-0.0135	0.0424	-0.0043	0.0313
åbygården				
Own history*Peer history	0.1240**	0.0490	0.4032**	0.1609
No own history*Peer history	-0.0109	0.0421	-0.0040	0.0310
öxnevalla				
Own history*Peer history	0.1294***	0.0495	0.3583**	0.1616
No own history*Peer history	-0.0083	0.0425	-0.0137	0.0313

Note: The main regressions are reestimated while leaving out the facility indicated in the panel heading. All estimations include facility-by-year FEs. Standard errors are clustered at the facility-by-day-of-entry level. * $p < .1$. ** $p < .05$. *** $p < .01$.

2.C Data Dictionary

Peer Variables

Peers: All other youths living in the facility that the youth arrives at on the first day of their placement while leaving out all youths who were in isolation or on the run on the day of the youth's arrival.

Peer size: Number of peers (see peers definition).

Above median peer size: An indicator taking the value 1 if the peer size is above the median peer size within that facility-by-year cell.

Peer female: Share of female peers (see peer definition), leaving out the focal youth.

Peer foreign: Share of peers (see peer definition) born outside Sweden, leaving out the focal youth.

Peer $<$15 y.o.: Share of peers (see peer definition) who were younger than 15 years old at the time they were placed at NBIC, leaving out the focal youth.

Peer $\geq$18 y.o.: Share of peers (see peer definition) who were at least 18 years old at the time they were placed at NBIC, leaving out the focal youth.

Peer substance abuse: Share of peers (see peer definition) with a pre-placement history of substance abuse (see substance abuse history definition), leaving out the focal youth.

Peer self-harm: Share of peers (see peer definition) with a pre-placement history of self-harm (see self-harm history definition), leaving out the focal youth.

Peer crime: Share of peers (see peer definition) with a pre-placement history of crime (see crime history definition), leaving out the focal youth.

Outcome Variables

Substance abuse: An indicator taking the value 1 if the youth experiences any of the following events: (1) is hospitalized with accidental drug/alcohol poisoning (ICD10-codes X40-X45), mental and behavioral disorders due to psychoactive substance use (ICD10-codes F1), or alcoholic liver disease (K70) listed as the main cause of harm or diagnosis; (2) dies from accidental drug or alcohol poisoning (ICD10-codes X40-X45); (3) commits an offense under The Swedish Penal Law on Narcotics; or (4) is readmitted to NBIC on the basis of substance abuse. Start date of crime is used (not conviction date).

Self-Harm: An indicator taking the value 1 if the youth experiences any of the following events: (1) is hospitalized with intentional self-harm (ICD10-codes X60-X84) or possible self-harm (ICD10-codes Y10-Y34) listed as the main cause of harm or (2) dies from intentional self-harm (ICD10-codes X60-X84) or possible self-harm (ICD10-codes Y10-Y34).

Crime: An indicator taking the value 1 if the youth experiences any of the following events: (1) commits an offense under The Swedish Criminal Code, (2) is readmitted to NBIC on the basis of criminal behavior, or (3) is sentenced to serve time at NBIC for an offense punishable by prison. Start date of crime is used (not conviction date).

Death: An indicator taking the value 1 if individual dies.

Death (suicide): An indicator taking the value 1 if individual dies and the underlying cause is intentional self-harm (ICD10-codes X60-X84).

Death (overdose): An indicator taking the value 1 if individual dies and the underlying cause is accidental drug or alcohol poisoning (ICD10-codes X40-X45).

Hospitalization due to mental health: An indicator taking the value 1 for hospitalizations with intentional self-harm (ICD10-codes X60-X84) or a mental and behavioral disorder (ICD10-codes F2-F9) listed as the main cause of harm or diagnosis, excluding mental and behavioral disorders due to psychoactive substance use (ICD10-codes F1).

Hospitalization due to substance abuse: An indicator taking the value 1 for hospitalizations with accidental drug/alcohol poisoning (ICD10-codes X40-X45), mental and behavioral disorders due to psychoactive substance use (ICD10-codes F1), or alcoholic liver disease (K70) listed as the main cause of harm or diagnosis.

Non-narcotic crime: An indicator taking the value 1 if individual committed any offense under The Swedish Criminal Code. Start date of crime is used (not conviction date).

Crime against person: An indicator taking the value 1 if individual committed an offense under Chapter 3-7, Section 5-6 of Chapter 8, or Section 1 of Chapter 17 of The Swedish Criminal Code. Start date of crime is used (not conviction date).

Narcotic crime: An indicator taking the value 1 if individual committed an offense under The Swedish Penal Law on Narcotics. Start date of crime is used (not conviction date).

Control Variables

Female: An indicator taking the value 1 if the youth is female.

Age at placed: Youth age in years at the time of the placement based on date of birth.

$<$ 15 *y.o.*: An indicator taking the value 1 if the youth was less than 15 at the time of placement.

$\geq$ 18 *y.o.*: An indicator taking the value 1 if the youth was at least 18 at the time of placement.

Foreign: An indicator taking the value 1 if the youth is born in another country than Sweden.

Involuntary placement: An indicator taking the value 1 if the youth is placed at NBIC following a court order to provide compulsory care on the basis that the youth's own behavior (substance abuse, criminal behavior, or other destructive behavior) poses a significant risk to her health or development, i.e. under Section 3 of the Care of Young Persons Act.

Voluntary placement: An indicator taking the value 1 if the youth is placed at NBIC voluntarily.

Sentenced: An indicator taking the value 1 if the youth is sentenced to serve time at NBIC for committing a crime punishable by prison.

Mandatory schooling: An indicator taking the value 1 if the youth is of mandatory-schooling age at the time of placement (i.e., placed before July the year the youth turns 16).

Finished compulsory schooling: An indicator taking the value 1 if the youth had graduated from compulsory schooling at the time of placement.

Missing personal identity number: An indicator taking the value 1 if the youth does not have an accurate personal identity number.

History of: Substance abuse: An indicator taking the value 1 if the youth was placed at NBIC because the youth abuses substance or during any of the 24 months prior to placement: (1) was placed at NBIC because the youth abuses substance; (2) was hospitalized with accidental drug/alcohol poisoning (ICD10-codes X40-X45), mental and behavioral disorders due to psychoactive substance use (ICD10-codes F1), or alcoholic liver disease (K70) listed as the main cause of harm or diagnosis; or (3) committed an offense under The Swedish Penal Law on Narcotics. Start date of crime is used (not conviction date).

History of: Self-harm: An indicator taking the value 1 if the youth was hospitalized in any of the 24 months prior to placement with intentional self-harm (ICD10-codes X60-X84) or possible self-harm (ICD10-codes Y10-Y34) listed as the main cause of harm.

History of: Crime: An indicator taking the value 1 if the youth was placed at NBIC because the youth engages in criminal behavior, was sentenced to serve time at NBIC for an offense punishable by prison, or during any of the 24 months prior to placement committed an offense under The Swedish Criminal Code. Start date of crime is used (not conviction date).

Any birth parent: Dead: An indicator taking the value 1 if any birth parent was dead before placement.

Any birth parent: $<$18 y.o. at birth of youth: An indicator taking the value 1 if any birth parent was under the age of 18 at the time of the youth's birth.

Any birth parent: Married, yr t-1: An indicator taking the value 1 if any birth parent was married at the end of the calendar year prior to placement.

Any birth parent: No labor income, yr t-1: An indicator taking the value 1 if any birth parent had no labor income during the full calendar year prior to placement.

Any birth parent: Hosp. d.t. mental health, yr t-1: An indicator taking the value 1 if any birth parent was hospitalized in the calendar year prior to placement with intentional self-harm (ICD10-codes X60-X84) or a mental and behavioral disorder (ICD10-codes F2-F9) listed as the main cause of harm or diagnosis, excluding mental and behavioral disorders due to psychoactive substance use (ICD10-codes F1).

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

Any birth parent: Any crime, yr t-1): An indicator taking the value 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 placement. Start date of crime is used (not conviction date).

Any birth parent: Missing Xs, yr t-1: An indicator taking the value 1 if data is missing for any birth parents in the calendar year prior to placement.

Chapter 3

Making Better Choices: The Role of Learning in the Judicial System

E. Jason Baron, Joseph J. Doyle, Jr., Ronja Helénsdotter[1]

A large literature documents substantial variation in decision-making for otherwise similar cases across decision-makers and over time in an array of government institutions, including the judicial system. This paper studies the drivers of variation in decision-making, with a focus on judges' learning under limited information. The analysis is based on over 20,000 Swedish child protection court cases from 2001 to 2019, which are linked with rich register data and novel data on appellate court decisions. Using quasi-random assignment of cases, we find strong and robust evidence that judges become more stringent with experience, conditional on judge fixed effects. This increase in removal tendency with experience is driven by male judges. The behavior change is not consistent with skill improvements as children who are randomly assigned to more experienced judges are more likely to die by the year they turn 19. The lack of learning is likely rooted in the limited access to information about the consequences of the court's decision. A potential driver of the positive relationship between stringency and experience can be signals from appellate courts. Indeed, we find that judges respond to appellate courts' decisions to reverse the judges' previous judgment to not remove a child from home by increasing their stringency. However, this effect is short-term and there is no detectable effect after one month. A more likely explanation is a change in judge preferences.

[1]Baron: Duke University, Department of Economics, 228B Social Sciences Building, Durham, NC 27708. E-mail: jason.baron@duke.edu. Doyle: MIT Sloan School of Management, 100 Main Street, E62-516, Cambridge, MA 02142. E-mail: jjdoyle@mit.edu. Helénsdotter: University of Gothenburg, Department of Economics, Vasagatan 1, SE 405 30, Gothenburg. E-mail: ronja.helensdotter@economics.gu.se. We thank Randi Hjalmarsson, Andreea Mitrut, and the participants and discussants at Arne Ryde Workshop on Gender and Family Wellbeing; The Crime and Victimization Workshop; and University of Gothenburg for many helpful comments and suggestions. Ronja Helénsdotter gratefully acknowledges financial support from The Royal Swedish Academy of Sciences. This research has been approved by the Swedish Ethical Review Authority.

3.1 Introduction

Fair treatment of all individuals by governmental institutions is a basic principle of any democracy. Nevertheless, a sizeable literature documents concerning variation in decision-making across agents and over time within an array of government institutions, including the judicial system. To enhance consistency and fairness, a common practice in government institutions is to assign high-stakes or complex cases to more experienced agents. This potential remedy relies on the assumption that experienced agents are more skilled, which is not evident, especially in settings with limited feedback. For example, according to the Convention on the Rights of the Child, the best interests of the child should be the primary consideration when public and private institutions make decisions concerning children. However, it is not uncommon for agents to lack information about the short- and long-term consequences of their decision, much less the counterfactual.[2] In such settings, it is challenging to evaluate and learn from previous decisions. In this paper, we consider one such setting: the child protection system.

More than 1 in 3 U.S. children are investigated for maltreatment at some point before their 18$^{\text{th}}$ birthday (Kim et al., 2017). At the same time, a growing body of literature shows that the decisions made by agents in the child protection system can have severe consequences (for a review, see Bald et al., 2022) and may even result in death (Helénsdotter, 2023). Given the high stakes, it is essential that child protection agents make fair and consistent decisions that are based on the merits of the case. However, as in other government institutions, substantial variation in decision-making has been documented in the child protection system.

In this paper, we investigate the role of judges' experience in the decision to remove children from their homes and the accuracy of these decisions. To further deepen our knowledge about the causes of variation in decision-making, we also examine how judges respond to decisions made by appellate courts.

Two key empirical challenges make it difficult to address these questions. First, if judges are not randomly assigned to cases, selection bias becomes an issue. Hence, we leverage rich data based on Swedish court files handed down between 2001 and 2019.[3] Due to Swedish law, the assignment of court cases

[2]Limited and noisy feedback is common in court systems. This issue might be especially prevalent in systems where the assigned judge does not process future cases involving the same parties and cases involving evaluations of future outcomes, e.g., involuntary provision of psychiatric or addiction care, child arrangements (e.g., custody and visitation), and refugee applications. Beyond the judicial system, limited and noisy feedback may hamper decision-maker learning in, e.g., emergency calls. Suppose an emergency call worker decides to not send an ambulance to a location and the patient dies. The call worker cannot learn from the situation if there is no system in place to provide feedback to emergency call workers about the outcomes of their decisions.

[3]Helénsdotter (2023) also uses data on Swedish child protection court cases but that paper does

to judges is quasi-random (both at the trial and appellate level) conditional on court-by-year fixed effects (FEs). While this feature rids our estimates of omitted variable bias stemming from case selection, individual judge characteristics and shocks can be correlated. Therefore, in our main specification, we only exploit temporal variation within each judge by including judge FEs. This is possible since judges often handle many cases over long periods of time.[4]

Another important empirical challenge is the lack of information about the 'correct' decision. Some studies use the outcome of appellate courts (e.g., Bhuller and Sigstad, 2022; Norris, 2022), but judges at a higher level do not necessarily make a superior decision. Fortunately, in this setting, the goal is clearly stated in law: make the best decision for the child in terms of their health and development. In addition, we have access to mortality data with national coverage for each child in our data set, which allows us to examine judge performance using an unambiguous outcome: surviving childhood.

Our results offer several important insights. First, conditional on court-by-year and judge FEs, judge experience significantly increases the probability of ordering removal in quasi-randomly assigned cases. One more year of experience as a judge increases the probability of removal by about 1.8 percentage points (relative to a dependent mean of 88.4%). This increase in stringency with experience is entirely driven by male judges: one more year of experience increases the probability of a male judge ordering removal by approximately 3.3 percentage points. For female judges, the point estimate is close to zero and lacks statistical significance. The difference in effect size is statistically significant at the 5% level.

There are no significant differences in the effects of judge experience by child characteristics (gender, foreign background, petition grounds, or age) and all point estimates are positive.

The increase in stringency is *not* consistent with skill improvements. Children who are randomly assigned to a judge who has one more year of experience are 0.4 percentage points more likely to die by the year they turn 19 (relative to a dependent mean of 0.7%). This is consistent with the findings of Heléndotter (2023), which shows that child removal sharply increases mortality in Sweden.

Why do judges not make better decisions with experience? As we elaborate on in later sections, a plausible explanation is the lack of feedback about the accuracy of their decisions, from the perspective of making the 'best' decision

not use data on appeals.

[4]Judge experience is correlated with other time-varying characteristics, such as age. A concern is whether the observed effects are driven by these other time-varying characteristics. We attempt to disentangle judge experience from other time-varying characteristics by examining the stability of the estimates and R^2 to the inclusion of such time-varying controls. However, even if the observed effects would be driven by some other time-varying characteristic, it is still, from a policy perspective, valuable to know whether judge experience captures a time-varying characteristic of judges that influences the probability of removal and decision accuracy.

for the child. Indeed, it is rare that judges ever observe the outcomes of the children they have decided over, and even if they do, it is challenging to assess the accuracy of the decisions as they cannot observe the counterfactual.

But why do judges make *worse* decisions with experience? In practice, judges may use decisions by appellate courts as information about the accuracy of their decisions. However, the appellate court judges often have the same information as the trial judge and can rarely observe which decision is 'correct'. Nevertheless, we find strong evidence that judges respond to one specific type of appellate court decision: reversals of their previous decision to *not* remove a child from home. Children who are quasi-randomly assigned a judge who has recently (within the last 2-4 weeks) experienced such a reversal (in an unrelated case) are around 7 percentage points more likely to be removed from home. However, the response decays quickly and there is no detectable effect beyond the first month. This short-term response can be driven by judges using appellate court signals to learn about the correct decision (Bhuller and Sigstad, 2022), but it can also be driven by emotional stress (Eren and Mocan, 2018).

Given the short-term nature of the response, learning through appellate court decisions is an unlikely explanation for the rise in stringency with experience. Anecdotal evidence suggests an alternative channel: processing many cases might affect judge preferences in the sense that they assign a lower subjective cost to removing a child from their home against the family's wishes.

Our paper relates to the large literature on decision-making in the judicial system. This literature predominately focuses on judge decision-making in criminal cases (see Mocan, 2020, for an overview).[5] It has been shown that both judge and defendant characteristics influence outcomes (e.g. Rehavi and Starr, 2014; Starr, 2015; Arnold et al., 2018; Cohen and Yang, 2019), that judges are affected by irrelevant events such as sport matches (Eren and Mocan, 2018) and birthdays (Chen and Philippe, 2019), and that there is in-group bias in judge decision-making (e.g., Gazal-Ayal and Sulitzeanu-Kenan, 2010; Depew et al., 2017; Cai et al., 2021). Beyond criminal justice, some studies investigate decision-making in, for example, immigration (Martén, 2017; Brodeur and Wright, 2019; Norris, 2022) and discrimination cases (Farhang and Wawro, 2004; Boyd et al., 2010; Boyd, 2016; Knepper, 2018). In terms of context, the closest study to this paper is Asmat and Kossuth (2020). They examine differences in male and female judges' decision-making in child support disputes.

We add to this literature, but also to the broader literature on decision-making

[5]Beyond legally trained judges, another key group of agents in the judicial system is jurors. Jury race (Anwar et al., 2012; Flanagan, 2018), gender (Anwar et al., 2019a; Hoekstra and Street, 2018), neighborhood (Anwar et al., 2022) and political affiliation (Martén, 2017; Anwar et al., 2019b) have all been shown to affect judicial outcomes. Most relevant to our paper is Anwar et al. (2014). They find that older jurors are stricter in criminal cases. Moreover, there is evidence that jury decisions are affected by irrelevant events (e.g., Bindler and Hjalmarsson, 2018; Philippe and Ouss, 2018).

in public and private institutions, by investigating the role of decision-maker experience under limited information using an unambiguous measure of decision accuracy. Prior studies (e.g., Chan et al., 2022; Norris, 2022) tend to find that decision-makers perform better with experience. However, as we show, this is not necessarily the case when it is difficult for decision-makers to learn from previous decisions.

Our paper also contributes to the literature on decision-making in the child protection system (Bartelink et al., 2018, De Haan et al., 2019). Despite the high-risk nature of these decisions, there is surprisingly little causal evidence on decision-making in this domain. An important exception is Baron et al. (2023). Using data on maltreatment investigations in Michigan, they estimate unwarranted disparities in decisions made by hotline call screeners and maltreatment investigators. To the best of our knowledge, there is no study on the causal drivers of *judicial* decision-making in child protection court cases.

Last, we provide some of the first causal evidence on the effects of appellate court decisions on judge decision-making and expand the knowledge about these effects to a non-criminal context. The only other such study is Bhuller and Sigstad (2023). To achieve identification, they exploit variation in the tendency of appellate judge panels to reverse prior judgments and find that (among judges who could have received another appellate court decision had they been assigned another panel) experiencing a random reversal has a large effect on judge sentencing decisions in the direction of the reversal using data on criminal cases in Norway. However, while we find that the judge's response decays quickly and is indistinguishable from zero after just one month, Bhuller and Sigstad (2022) present evidence that the response persists for around one year.

The remainder of this paper proceeds as follows. Section 3.2 presents the institutional setting. Section 3.3 describes the data and analysis samples. Section 3.4 discusses the empirical methodology and the validity of the identifying assumptions. Section 3.5 presents evidence on the role of experience in judicial decision-making. Section 3.6 provides a model of judicial decision-making and discusses potential mechanisms. Section 3.7 concludes.

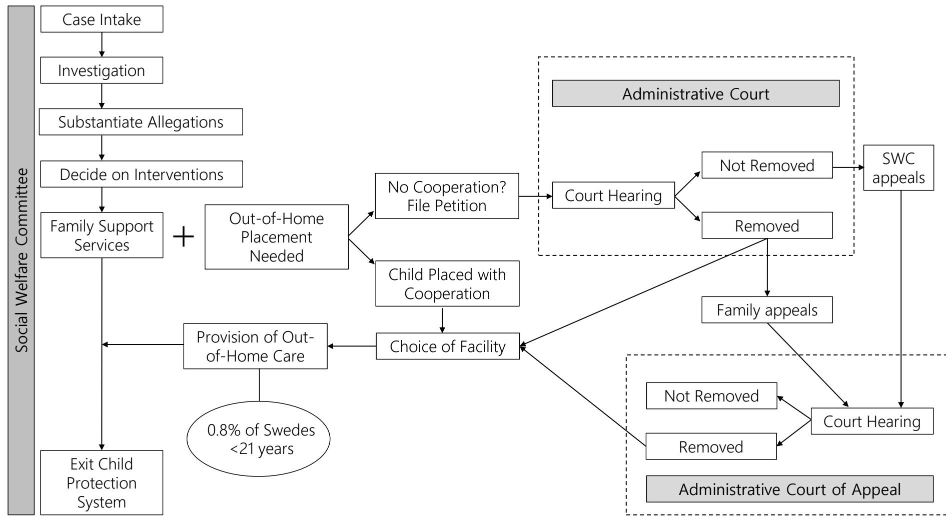
3.2 Institutional Background

Figure 3.1 illustrates the child protection process in Sweden.[6] The local Social Welfare Committee (SWC; *Socialnämnden*) holds the main responsibility for child protection (including preventive work, child maltreatment investigation, evaluation of service need, and service provision). If the SWC determines that out-of-home placement is needed but the family does not consent to placement, the SWC files a petition for removal with one of 12 administrative courts (*förvalt-*

[6]See Heléndsotter (2023) for further details about the institutional context.

ningsrätt).[7]

Figure 3.1. Child Protection Process in Sweden



Note: This figure illustrates the child protection process in Sweden. The local SWC is in charge of determining whether out-of-home care is necessary. If out-of-home placement is necessary but the family does not consent, the SWC files a petition with the court that has jurisdiction. The court then holds a hearing and decides whether to approve the petition. The SWC and the family can appeal the decision made by the court. The case is then tried by a court of appeal. Irrespective of the route to out-of-home care, the SWC is in charge of the provision of care.

According to the Care of Young Persons Act, the court is to rule in favor of removal if (i) at least one condition of the home environment implies a palpable threat to the health or development of the child (known as environment cases) or (ii) the child endangers their health or development through criminality, substance abuse, or other destructive behavior (known as behavior cases). However, what is best for the child in terms of the child's health and development is to be decisive.

As required in the Administrative Courts Act, court cases must promptly be assigned to a judge following predetermined and objective criteria. At the Administrative Court of Gothenburg, the registration office registers all incoming petitions in the case management system.[8] The case is quasi-randomly assigned to a department via a rotating system and then to a judge within the department (again via a rotating system).[9] As described in Heléndsotter (2023), the only

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

[8]The courts in Falun, Malmö, and Stockholm provide similar descriptions of the case assignment process and confirm that quasi-random assignment has been used during the years covered in our data.

[9]A departmental structure is employed in the four largest courts. Each department has a chief

exception to the assignment process is 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.[10] The balance test and results are robust to excluding cases that are typically not assigned to junior judges.

The court must offer each family member a lawyer and hold an oral hearing within 2 weeks of receiving the petition for child removal. The court administrator decides the date of the hearing based on courtroom availability and the calendars of the lawyers, judge, and law clerk. Judges are expected to be available Monday-Friday during office hours. No hearings are held after office hours or on weekends. When the date of the hearing is set, the case is randomly assigned three jurors (*nämndemän*) from the pool of available jurors. The judge cannot influence the choice of jurors.

All concerned parties are invited to the hearing but attendance is not mandatory and whether a party attends should not influence the outcome of the case. The judge's identity is revealed before the hearing. In contrast to the setting studied in Ash and Nix (2023), there are no public statistics on judges' decision tendencies.[11]

The judge and three jurors hold deliberations immediately after the hearing. The deliberations tend to be shorter than 15 minutes and end with a vote. Each vote is given equal weight and the judge holds the tiebreaker. The sole task of the court is to decide if the child is to be placed in out-of-home care, i.e. there is only one judiciary outcome. The SWC is in charge of making all other decisions (e.g., where the child should be placed and for how long). See Helénsdotter (2023) for further details about child protection in Sweden and a cross-country comparison.

The court's ruling can be appealed by the SWC, a parent, or the child to one of Sweden's four administrative appellate courts (*kammarrätten*).[12] When needed for clarity, we refer to the former as the trial court. If the trial court decides to remove the child from home, the child is kept in out-of-home care until the appellate court has reached a new decision.

Information about how to appeal is provided as a 1-page appendix to the

judge and a team of judges. Typically, one department is solely focused on tax cases and the remaining departments are assigned all other cases. Immigration cases are solely processed at specific departments in Stockholm, Gothenburg, and Malmö. The results are robust to the use of department-by-year FEs.

[10]The 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 attention by the media; concerns security issues; or will likely require special experience to not delay proceedings.

[11]The SWC can change its claims before or during the hearing. Background variables such as petition grounds are based on the initial petition (i.e. before the judge is assigned).

[12]A third and last appeal can be made to the Supreme Administrative Court (*Högsta förvaltningsdomstolen*). However, a review permit is required and such a permit is rarely granted.

judgment. All parties have the right to appeal free of charge but the appeal must be filed in writing and received by the trial court within 3 weeks of the judgment. The appeal should include information about how and why the concerned party believes the court's judgment should be changed. If the appeal is received on time, the trial court submits the appeal to the appellate court with jurisdiction. There is no additional screening of appeals and no review permit is required for the appellate court to try the case.

Trial and appellate courts are all subject to the Administrative Courts Act and use the same case management software. Hence, the case processing is almost identical in trial and appellate courts. In particular, incoming cases are first assigned to a department and then to judges within the department following a two-level rotating system. A key difference is that three judges are assigned to appellate court cases. Once the judges, law clerk, and lawyers are assigned, the court administrator promptly schedules an oral hearing and assigns two jurors. Judges and jurors who serve at the trial level cannot serve at the appellate court level at the same time. After concluding the hearing, the court deliberates and votes on whether the trial court's judgment to remove or not remove the child from home should be changed. Each judge and juror has one vote of equal weight.

The median number of days between the trial court's and the appellate court's judgment is 84 days.[13] Once the appellate court's judgment is finalized, it is sent to the trial court in full. The trial court administrator then sends the judgment to the trial judge. This process can take a few days.

3.3 Data

3.3.1 Data Description

Our analysis exploits novel data that we collected from Swedish appellate courts and several existing sets of data linked using unique case, judge, child, and parent identifiers. Our primary source of Swedish court data at the trial level is the database built by Helénsdotter (2023). This database contains 26,577 child-by-case observations spanning 2001 to 2019 (with national coverage from February 14, 2010). An array of case characteristics are included (e.g., decision date, date of oral hearing, courthouse, presence of siblings, petition grounds).[14] In addition, we observe judge gender, year of birth, and position. Unfortunately, only the date of first employment as a *regular* judge is available. Hence, we focus on the number of years since the first employment as a regular judge at any Swedish court as our measure of judge experience.

[13]See Figures A1-A2 for the distribution of days between decisions and the median number of days separately by appellate court.

[14]See Helénsdotter (2023) for a detailed data description and variable definitions.

The database is linked with several national registers kept by Statistics Sweden, the National Board of Health and Welfare, and the National Council for Crime Prevention. Thereby, we have standard demographic information about each child and birth parent (e.g., gender, date of birth, emigration/immigration dates, labor income, marital status) but also national data on all deaths (date and cause; 1997-2022), hospitalizations (date and cause; 1997-2020), and legal proceedings (date of crime, date of decision, and section of the law; 1997-2021).[15]

We supplement the database used in Helénsdotter (2023) with novel data on appeals. We collected and transcribed the universe of judgments handed down by each appellate court in Sweden from January 1, 2005, to December 31, 2019. From the appellate court judgments, we extract, e.g., appeal case number, trial case number, decision date, judgment, and appellate judge names using scripts.[16] The full appeal database contains 8,974 unique appeals. However, since the trial court database does not have national coverage until early 2010, only 7,426 appeals are matched with the trial court database.

In our main analysis, we focus on the effect of an appellate court handing down a judgment in the 2-4 weeks before the date of deliberation in an unrelated case handled by the same trial judge. We exclude the 7 days immediately before the date of deliberation because it takes time for the appellate court's judgment to reach the trial judge (see Section 3.2). There are four types of appeal judgments: overturned prior non-removal, affirmed prior removal, overturned prior removal, and affirmed prior non-removal. We refer to these types of appeal judgments as '*Wrong, remove*', '*Right, remove*', '*Wrong, do not remove*', '*Right, do not remove*'.

3.3.2 Sample Creation and Descriptive Statistics

In this section, we describe the analysis subsamples, which vary depending on the availability of register data. Throughout the paper, we drop children who we have almost no background information on as we cannot observe them in Statistics Sweden's register data (N=1,576).[17] The baseline sample is then reduced from 26,577 to 25,001 observations.

In our analysis of the influence of judge experience, we drop cases assigned to a judge for whom we cannot observe years of experience as a regular judge (N=5,170), i.e. judges who have not been promoted to regular judges.[18] We also drop cases in court-by-year cells containing only one active judge (N=56) and judges who handle less than 2 cases (N=19). The final sample (N=19,756) consists of 15,802 unique cases (18,606 unique children) assigned to one of 381 judges. We refer to it as the 'Judge Sample'.

When studying the effects of appellate court decisions, we restrict the sample of children who are observable in Statistics Sweden's register data to those whose case is handed down at least one month after the start of our appeal data (January 1, 2005). Thereby, we drop 528 observations. This restriction is imposed because we cannot observe if judges who handle cases in the first weeks of 2005 received a signal from an appellate court in the prior 2-4 weeks. We also drop cases in court-by-year cells containing only one active judge (N=1) and judges who handle less than 2 cases (N=59). The sample (N=24,413) is referred to as the 'Appeal Sample'.

Table 3.1 displays descriptive statistics at the child and birth parent level (Panel A), appeal level (Panel B), and judge level (Panel C) for each analysis sample. For comparison purposes, the first column shows statistics for the full court sample conditional on being observed in Statistics Sweden's register. The child, parent, and appeal statistics are almost identical across columns. Almost 90% of children whose case goes to court are removed from home by the trial court. At the same time, 40% of the decisions are appealed but only 4% of all decisions are eventually overturned by the appellate court.

In Panel C, we first present the average judge removal tendency, which is calculated as the judge's mean removal rate in all other cases (leaving out the focal case).[19] In line with the random assignment condition, the average judge removal tendency is almost identical across columns. However, the other judge characteristics vary slightly by sample. The reason is that in column 2, we restrict the sample to cases assigned to regular judges, who tend to be older and less likely to be female than non-regular judges.

In Table A1, we provide descriptive statistics by whether the focal case is appealed. As expected, there is selection into the appeal group along an array of child and parent characteristics. For example, appealed cases are more often exclusively based on deficiencies in the home environment, involve siblings, and result in child removal without the consent of any parent.

[15]The legal proceedings register contains all crimes committed in Sweden (conditional on guilt having been established), including convictions, penalty orders without a court hearing, and waivers of prosecution.

[16]When possible, we cross-checked each variable (e.g., judgment and trial case number) with administrative data provided by the appellate courts.

[17]These children have missing or inaccurate personal identity numbers. Almost all have recently been born or immigrated to Sweden and, therefore, have not been given their personal identity numbers yet.

[18]We only have complete data on the date hired as a regular judge. Regular judges may have varying experience as junior judges (i.e., judges in training) before being promoted to regular judges.

[19]Figure A3 illustrates the substantial variation (mean 0.885; std. dev. 0.062) in behavior across judges that exists in our sample, even after accounting for court-by-year FEs. In particular, a judge at the 1st percentile removes 69.7% of children while a judge at the 99th percentile removes 99.5%. Our judge removal tendency is not comparable with the behavior tendency reported in studies using the decisions of child protection caseworkers because, in our setting, the caseworkers have *already* decided to submit a petition for removal.

Table 3.1. *Descriptive Statistics*

	All in Registry	Judge Sample	Appeal Sample
A: Child & Parent Characteristics			
Removed	0.89	0.88	0.89
Girl	0.46	0.47	0.46
Age at judgment	10.83	10.72	10.79
Sibling case	0.32	0.33	0.32
Foreign background	0.38	0.39	0.38
Behavior petition	0.29	0.27	0.28
Environment petition	0.61	0.62	0.61
Double grounds petition	0.11	0.10	0.11
Child consents to removal	0.64	0.65	0.64
At least 1 parent consents to removal	0.36	0.35	0.36
Case largely based on child mental health	0.04	0.04	0.04
Non-junior case type	0.16	0.17	0.17
Committed (yrs t-1 to t-3):			
Crime against person	0.09	0.09	0.09
Narcotic crime	0.10	0.10	0.10
Other crime	0.11	0.11	0.11
Hospitalized (yrs t-1 to t-3) due to:			
Mental health	0.06	0.06	0.06
Substance use	0.05	0.05	0.05
Missing, yrs t-1 to t-3	0.23	0.24	0.24
Any birth parent:			
Dead	0.05	0.05	0.05
<18 y.o. at birth of child	0.02	0.02	0.02
Married, yr t-1	0.45	0.45	0.45
No labor income, yr t-1	0.63	0.63	0.63
Hosp. d.t. mental health, yr t-1	0.07	0.07	0.07
Hosp. d.t. substance use, yr t-1	0.05	0.05	0.06
Any crime, yr t-1	0.16	0.16	0.16
Missing Xs, yr t-1	0.24	0.24	0.24
B: Appeal Characteristics			
Case appealed	0.40	0.41	0.40
Days between trial decision and appellate decision	90.56	90.32	90.58
Appellate overturned prior approval	0.03	0.03	0.03
Appellate overturned prior denial	0.01	0.01	0.01
C: Judge Characteristics			
Judge removal tendency	0.89	0.89	0.88
Female judge	0.53	0.50	0.53
Judge age	49.81	53.01	49.86
Judge experience	7.96	7.91	8.00
Unique judges	847	381	777
Unique cases	20206	15802	19700
Unique children	23184	18606	22643
Unique birth parents	31665	25551	30857
Observations	25001	19756	24413

Note: This table presents descriptive statistics on child, parent, appeal, and judge characteristics for all children who are observed in Statistics Sweden's register and for each analysis sample as described in Section 3.3.2. Statistics are shown for observations with non-missing information.

3.4 Empirical Methodology

3.4.1 Empirical Specification

The main aim is to investigate how judge characteristics affect the probability of court-ordered care. The specification that we bring to the data can be written as:

$$R_{i,c,t} = \beta X_{j(c,t)} + \alpha_{h,t} + \delta_j + \epsilon_{i,c,t}, \tag{3.1}$$

where $R_{i,c,t}$ is an indicator equal to 1 if the court orders child i to be removed from their home in year t, $X_{j(c,t)}$ is a time-varying characteristic of judge j who is assigned case c, $\alpha_{h,t}$ are court-by-year FEs, δ_j are judge FEs, and $\epsilon_{i,c,t}$ is an error term.

We include court-by-year FEs because the randomization of cases to judges occurs within the pool of available judges at each court. By including such FEs, we account for differences in child and judge characteristics across courts and over time.[20]

While conditional randomization is enough to disentangle the effect of $X_{j(c,t)}$ from case characteristics, it is not enough to disentangle the causal effect of $X_{j(c,t)}$ from all other characteristics of judge j. For example, if more senior judges tend to be male, and male judges behave differently than female judges of similar seniority, β suffers from omitted variable bias. To avoid omitted variable bias stemming from such differences in time-invariant characteristics across judges, we include judge FEs. However, other time-varying characteristics still pose an issue. Judge experience is correlated with age. Therefore, β might be picking up the effect of being assigned an older judge. Hence, we follow Altonji et al. (2005) and Oster (2019) and examine the stability of the coefficient and R-squared to the inclusion of additional time-varying judge characteristics.

Throughout the paper, we cluster the standard errors at the case level because judges are quasi-randomly assigned to cases that may contain siblings (see Abadie et al., 2023, Chyn et al., 2023).

3.4.2 Random Assignment

To isolate the effect of judge characteristics on removal from case selection, we require quasi-random assignment of judges to cases. Given the features of the institutional setting, judges are expected to be assigned to cases quasi-randomly conditional on observable controls (see Section 3.2). To test the validity of this assumption, we inspect the balance of case characteristics.

[20] Our results are robust to the use of other FEs, including department-by-year FEs, court-by-year FEs together with day-of-week FEs and SWC FEs, and court-by-year FEs together with male-judge-by-year FEs.

Table 3.2. Test of Random Assignment

	Removed		Judge Removal Tendency	
	Coeff	Std err	Coeff	Std err
Girl	-0.0063	0.0047	0.0010	0.0009
Age at judgment	0.0035***	0.0008	-0.0001	0.0002
Sibling case	-0.0264***	0.0081	0.0005	0.0016
Foreign background	0.0282***	0.0066	0.0011	0.0014
Behavior petition	0.0154**	0.0075	0.0014	0.0017
Environment petition	-0.0998***	0.0093	-0.0018	0.0019
Child consents to removal	0.2425***	0.0095	-0.0005	0.0015
At least 1 parent consents to removal	0.0674***	0.0064	-0.0016	0.0014
Missing consent data	0.1416***	0.0213	-0.0001	0.0043
Case largely based on child mental health	-0.0385***	0.0149	0.0007	0.0028
Non-junior case type	-0.0104	0.0078	0.0008	0.0015
Committed (yrs t-1 to t-3):				
Crime against person	0.0097	0.0078	0.0010	0.0020
Narcotic crime	0.0460***	0.0071	0.0005	0.0019
Other crime	0.0104	0.0075	-0.0014	0.0019
Hospitalized (yrs t-1 to t-3) due to:				
Mental health	0.0006	0.0096	0.0011	0.0021
Substance use	0.0123	0.0090	-0.0005	0.0024
Missing, yrs t-1 to t-3	0.0282***	0.0075	0.0009	0.0016
Any birth parent:				
Dead	0.0294**	0.0122	0.0013	0.0025
<18 y.o. at birth of child	-0.0049	0.0174	-0.0018	0.0036
Married, yr t-1	0.0097	0.0067	-0.0003	0.0014
No labor income, yr t-1	-0.0012	0.0067	-0.0002	0.0014
Hosp. d.t. mental health, yr t-1	0.0158	0.0127	-0.0014	0.0026
Hosp. d.t. substance use, yr t-1	0.0147	0.0141	0.0029	0.0027
Any crime, yr t-1	0.0229**	0.0093	-0.0007	0.0017
Missing Xs, yr t-1	0.0013	0.0091	-0.0013	0.0018
F-statistic	38.55		0.44	
p-value	0.00		0.99	
N	19756		18291	

Note: Test of random assignment of judge removal tendency to cases using the 'Judge Sample'. In column 2, observations with missing judge removal tendency are (naturally) excluded. 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. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table 3.2 provides strong empirical evidence that judges are randomly assigned conditional on court-by-year FEs. The first column regresses removal on 25 child and parent background variables. Half of the variables are significant predictors of removal (joint *F*-statistic of 38.55). However, in accordance with random assignment, the estimated coefficients are close to zero, lack individual significance, and are not jointly significant (*F*-statistic: 0.44) when leave-out mean judge removal tendency is regressed on the same set of variables. This implies that child and parent characteristics that predict removal are not correlated with judges' tendency to remove children from home. For more than half of the variables, the coefficient signs are not even the same in the two regressions.

Additional randomization tests are provided in Tables A2-A3. In Table A2, we test for random assignment using judge characteristics (judge gender, age, experience) and judge-specific temporal shocks (appellate court decisions). Next, we vary sample and specification decisions (Table A3). We consistently document small *F*-statistics.

3.5 Results

3.5.1 Effect of Judge Experience

Table 3.3 presents the estimated effect of being quasi-randomly assigned a more experienced judge on the probability of removal. In column 1, we only control for court-by-year and judge FEs. The point estimate implies that judges with one more year of experience are 1.8 percentage points (significant at the 5% level) more likely to order child removal. When the full set of child and parent controls (see Table 3.1, Panel A) are included, the point estimates are similar (column 2). In column 3, we also include other time-varying judge characteristics. Specifically, we include judge age at the time of the judgment, judge age squared, categorical versions of judge age (25-35, 36-55, 56-75), and indicators for the four types of appellate court judgments (conditional on the judgment occurring within 2-4 weeks before the date of deliberation). There is no meaningful change in the point estimate, which suggests that bias stemming from omitted time-varying judge characteristics may be of limited concern (Altonji et al., 2005; Oster, 2019).[21]

[21] Following Altonji et al. (2005) and Oster (2019), we calculate the degree of selection on unobserved variables relative to observed variables that would be required to attribute the entire effect of judge experience to omitted variable bias. Assuming that the outcome can be fully explained by the treatment and a complete set of observed and unobserved controls (maximum $R^2 = 1$) yields a ratio of 4.9. This value implies that the selection on unobserved controls must be almost 5 times as large as the selection on observed controls to explain away our estimated effect. The suggested robustness standard is 1 (i.e. equal selection). Hence, omitted variable bias might be of limited concern.

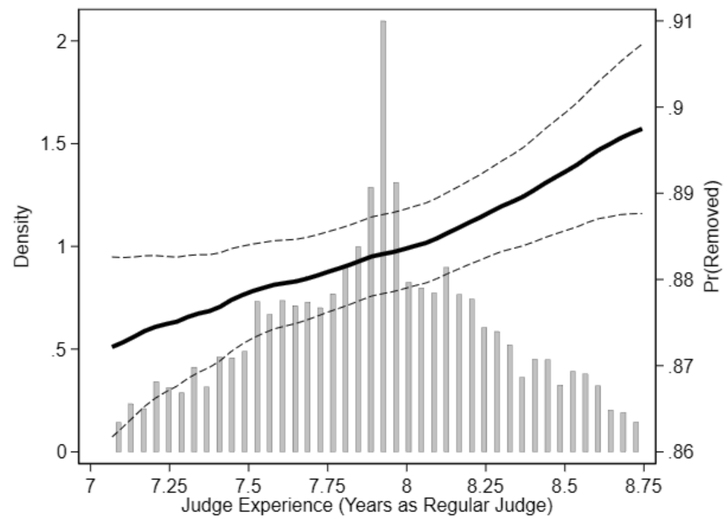
Table 3.3. Effect of Judge Experience on Child Removal

	Full Sample			By Judge Gender		
	(1) Baseline	(2) Case Controls	(3) Judge Controls	(4) Female	(5) Male	(6) Δ
Judge experience	0.0178**	0.0187***	0.0179***	0.0003	0.0326***	0.0323**
	(0.0070)	(0.0068)	(0.0069)	(0.0095)	(0.0104)	(0.0141)
Dependent mean	0.8840	0.8840	0.8838	0.8875	0.8806	
Child & parent controls	No	Yes	Yes	No	No	No
Time-varying judge controls	No	No	Yes	No	No	No
N	19756	19756	19435	9864	9888	19752

Note: The dependent variable is an indicator for child removal. The 'Judge Sample' is used. All estimations include court-by-year and judge FEs. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

A flexible regression of removal on judge experience is shown in Figure 3.2. The probability of child removal increases approximately linearly with judge experience. Figure 3.2 also depicts the variation in judge experience after accounting for court-by-year and judge FEs, i.e. the variation exploited for identification. After residualization, the mean number of years as a regular judge is 7.91 (std. dev. 0.38; min. 6.83; max. 8.98).

Figure 3.2. *Relationship Between Removal and Judge Experience*



Note: This figure depicts the relationship between removal and judge experience. The histogram shows the density of judge experience (leaving out the top and bottom 1%). The 'Judge Sample' is used (see Section 3.3.2). The solid line shows a Kernel-weighted local polynomial regression of removal on judge experience, while the dashed lines show 90% confidence bands. Removal and judge experience are residualized using court-by-year and judge FEs. Settings: triangle Kernel, degree 0, and bandwidth 0.5.

These aggregate results mask meaningful heterogeneity. In columns 4-5, Table 3.3, we split the sample by judge gender. The increase in removal tendency with experience is entirely driven by male judges. For female judges, the point estimate is close to zero while the point estimate in column 5 implies that being randomly assigned a male judge with one more year of experience increases the probability of removal by 3.3 percentage points (1% level). The difference in effect sizes is statistically significant at the 5% level.[22] Figure A4 provides a scatter plot

[22]Possible time trends in removal tendency by judge gender are explored in Figure A5. Specifically, we regress removal on indicators for each case decision year (in the 'Judge Sample' and the subsample of female and male judges), court FEs, and the child and parent characteristics listed in Table 3.1, Panel A. The point estimates and 95% confidence intervals are presented. We find no evidence of time trends in removal tendency by judge gender after accounting for court FEs and observable child and parent characteristics.

of the probability of child removal and judge experience by judge gender (after residualization using court-by-year and judge FEs).

Using largely the same sample, Helénsdotter (2023) finds that child removal greatly increases the risk of death by the year the child turns 19. As expected given our finding that judges become more likely to remove children as they gain years of experience, Table 3.4 presents reduced-form estimates (with and without case controls) showing that the risk of all-cause death increases by 0.4 percentage points (relative to a mean of 0.7%; 10% significance level) if the child is randomly assigned a judge with one more year of experience.

Table 3.4. Reduced-Form Effect of Judge Experience on Child Mortality

	(1) Baseline	(2) With Controls
Judge experience	0.0040*	0.0042*
	(0.0023)	(0.0022)
Dependent mean	0.0070	0.0070
Child & parent controls	No	Yes
N	9677	9677

Note: The dependent variable is an indicator equal to one if the child dies by the year they turn 19. The 'Judge Sample' is used but restricted to children who (i) would turn 19 by the end of our mortality data (2022) and (ii) are not emigrated by the end of our migration data (2022). All estimations include court-by-year and judge FEs. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

3.5.2 Effect of Appellate Court Signals

How are judges' decision-making affected by an appellate court reversing or affirming their previous judgment? Table 3.5 presents OLS results from regressing the probability of removal on indicators for whether an appellate court handed down a judgment (in the 2-4 weeks prior to the focal case's date of deliberation) in which the appellate court either overturns or affirms the judge's previous decision. In all regressions, we include court-by-year and judge FEs to account for differences across courts and over time as well as time-invariant differences across judges.

Table 3.5. *Effect of Appellate Court Signals on Child Removal*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Appellate Court Signal (2-4 Weeks Prior):								
Wrong, remove	0.0731***	0.0753***	0.0635***			0.0746***	0.0628***	0.0637***
	(0.0213)	(0.0214)	(0.0202)			(0.0214)	(0.0202)	(0.0213)
Right, remove		0.0082	0.0083			0.0077	0.0078	0.0056
		(0.0068)	(0.0063)			(0.0068)	(0.0063)	(0.0068)
Wrong, do not remove				0.0002	-0.0034	0.0027	-0.0010	-0.0046
				(0.0234)	(0.0236)	(0.0234)	(0.0236)	(0.0252)
Right, do not remove				-0.0961	-0.0847	-0.0932	-0.0819	-0.0326
				(0.0585)	(0.0609)	(0.0587)	(0.0610)	(0.0573)
Dependent mean	0.8853	0.8853	0.8853	0.8853	0.8853	0.8853	0.8853	0.8840
Child & parent controls	No	No	Yes	No	Yes	No	Yes	Yes
Time-varying judge controls	No	No	No	No	No	No	No	Yes
Test of equality (*p*-value)		0.0021	0.0076	0.1253	0.2120	0.0050	0.0205	0.0463
N	24413	24413	24413	24413	24413	24413	24413	19490

Notes: The dependent variable is an indicator for child removal. The 'Appeal Sample' is used in columns 1-7. Column 8 uses the 'Judge Sample'. All estimations include court-by-year and judge FEs. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

In the first column, we only include one type of appellate court judgment: judgments in which the appellate court reverses the trial judge's prior decision to *not* remove a child from home (i.e. signals that the trial judge made the wrong choice and should have ordered removal). The point estimate for 'Wrong, remove' implies that being quasi-randomly assigned a judge who has recently received a signal that they made an incorrect decision to not remove a child from home increases the probability of court-ordered removal by 7.3 percentage points (1% significance level).

In column 2, we also include whether an appellate court affirmed the judge's previous decision to remove a child from home ('Right, remove'). The estimated effect of the reversal is essentially unchanged. In contrast to the effect of being assigned a judge who has recently learned that they made an incorrect decision, the point estimate for 'Right, remove' is small and not statistically significant. The difference in effect size is significant at the 1% level. This implies that judges react more if the appellate court signals that they made an incorrect decision. The estimates are similar when including a full set of child and parent controls (column 3).

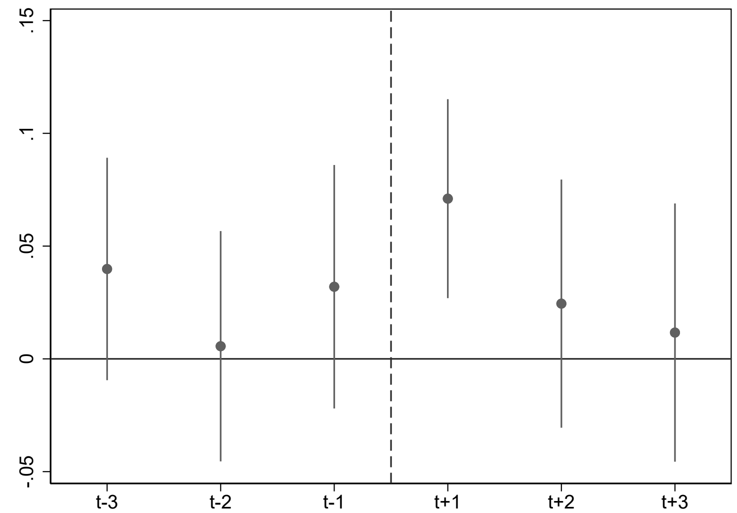
In columns 4-5, we regress removal on indicators for whether the appellate court overturned (affirmed) the judge's previous decision to (not) remove a child from home (with and without child and parent controls). The point estimate for 'Wrong, do not remove' is close to zero while the point estimate for 'Right, do not remove' is negative and sizable but imprecisely estimated.

In columns 6-8, we include all four potential appellate court signals with and without child, parent, and judge controls. Across all columns, the point estimates are similar.

To shed light on how persistent the effect of an appellate court reversing a prior decision to not remove a child from home is on judge behavior, we regress removal in case c on indicators for an appellate court handing down such a judgment in the 2-4, 5-8, and 9-12 weeks prior to the date of deliberation in case c. We also include indicators for an appellate court handing down such a judgment in the 2-4, 5-8, and 9-12 weeks after the date of deliberation in case c as a falsification test. Figure 3.3 depicts the estimated effects (with 95% confidence intervals). As expected, the estimated effects of appeal signals that are sent after the date of deliberation (t+1 to t+3) are not significant.[23] Interestingly, the effect of an appeal signal sent before the date of deliberation decays quickly, and the estimated effect is not statistically significant after only one month. However, tests of equality only yield a statistically significant difference (p-value=0 .052) in point estimates at t-2 and t+1. A test of equality for the estimates at t-1 and t+1 yields a p-value of 0.108.

[23] The focal case is excluded when constructing the indicators for appeal signals.

Figure 3.3. *Effect of Reversal of Judge's Prior Decision to not Order Removal*



Note: This figure shows the OLS point estimates and 95% confidence intervals from regressing removal in case c on six indicators that capture whether a court of appeal overturned a case d at weeks 2-4, 5-8, and 9-12 *prior* and weeks 2-4, 5-8, and 9-12 *after* the date of deliberation in case c, conditional on case $c \neq d$ being handled by the same trial judge j. Court-by-year and judge FEs are included in the regressions. The 'Appeal Sample' is used. The dashed line indicates the date the appellate court's judgment is handed down. The coefficients at t-1, t-2, and t-3 capture the (placebo) effect of appellate court judgments handed down *after* the deliberation date of the focal trial case. The coefficients at t+1, t+2, and t+3 capture the effect of appellate court judgments handed down *before* the deliberation date of the focal trial case.

3.5.3 Heterogeneity

Results by child characteristics (gender, foreign background, placement grounds, and age) are presented in Tables 3.6-A5. Table 3.6 regresses removal on judge experience in each subsample indicated at the top of the table. The estimates are fairly similar to the main results and there are no statistically significant differences across subgroups.

Table 3.6. *Heterogeneity of Effect of Judge Characteristics on Removal*

	Gender			Background			Placement Grounds			Age at Placement		
	(1) Girl	(2) Boy	(3) Δ	(4) Foreign	(5) Native	(6) Δ	(7) Environ.	(8) Behavior	(9) Δ	(10) $<$13 yrs	(11) $\geq$13 yrs	(12) Δ
Judge experience	0.0226**	0.0145*	0.0082	0.0140	0.0226**	-0.0086	0.0149	0.0081	0.0068	0.0140	0.0211***	-0.0071
	(0.0099)	(0.0085)	(0.0117)	(0.0101)	(0.0092)	(0.0136)	(0.0105)	(0.0078)	(0.0131)	(0.0120)	(0.0072)	(0.0134)
Dependent mean	0.8771	0.8899		0.9032	0.8719		0.8521	0.9410		0.8500	0.9167	
N	9195	10530	19725	7617	12099	19716	12297	5391	17688	9698	10025	19723

Note: The dependent variable is an indicator for child removal. We limit the 'Judge Sample' to the subgroup specified at the top of each column. All estimations include court-by-year and judge FEs. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A5 regresses removal on an indicator for whether the assigned judge received a recent signal from an appellate court that they made the wrong decision in a previous case and should have removed the child from home. The point estimate is positive in each subsample except in behavior cases. In particular, the response is 11.5 percentage points higher if the judge is quasi-randomly assigned to an environment case rather than a behavior case (significant at the 1% level). Since children with behavioral problems tend to be older, it is unsurprising that there is a large difference in effect size by the age of the child as well (1% level).

Moreover, we inspect heterogeneity in the effects of appeal signals by judge characteristics (gender, age, and experience) in Table A6. The response to an appellate court reversing the judge's decision not to remove a child from home is positive in all subsamples and there are no statistically significant differences.

3.5.4 Robustness Checks

We present robustness checks related to sample and specification decisions in Tables A4-A7. The main results are robust to dropping each court. Baseline results are provided in Panel A of each table 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 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; and judges who handle at least 20 cases. We also show robustness to three-way clustering on judge, child, and case level; replacing court-by-year FEs with department-by-year FEs; adding FEs for judgment day of the week and SWC in charge; and adding FEs for male-judge-by-year FEs. In Table A7, we also demonstrate robustness to excluding junior judges from the 'Appeal Sample' and replacing court-by-year FEs with appellate-court-by-year FEs.

3.6 Mechanisms

To understand the drivers of variation in judicial decision-making, it is useful to first model judge behavior.

The judicial objective is stated in the very first section of the Care of Young Persons Act: the court should order out-of-home care ($R_{i,j} = 1$) if court-ordered care is 'best' for the child, under the condition that (i) the home environment implies a palpable threat to the health or development of the child ($C_i^e = 1$) or (ii) the child endangers their health or development through criminality, substance abuse, or other behavior ($C_i^b = 1$). Hence, even if $C_i^e = 1 \vee C_i^b = 1$, the court should *not* order child removal if it is better for the child (from the perspective of the child's health and development) to receive care in the home environment

(Swedish Government, 1989). This decision rule can be formalized as:

$$R_{i,j} = \begin{cases} 1 & \text{if } \left(C_i^e = 1 \vee C_i^b = 1\right) \wedge \left(u(H_{i,1}, D_{i,1}) > u(H_{i,0}, D_{i,0})\right) \\ 0 & \text{otherwise,} \end{cases} \tag{3.2}$$

where $u(H_{i,r}, D_{i,r})$ is the future utility of child i, which depends on the child's future health ($H_{i,r}$) and development ($D_{i,r}$) when $R_{i,j} = r$. However, in practice, the judge must form beliefs about C_i^e, C_i^b, $u(H_{i,1}, D_{i,1})$, and $u(H_{i,0}, D_{i,0})$ under limited information.

In many settings, decision-makers accumulate information and improve their skill as they become more experienced. For example, physicians who fail to diagnose a patient may observe the disease progression (false-negative) and physicians who wrongly diagnose a patient may observe the lack of an effect of the prescribed treatment on the patient's symptoms (false-positive).[24] By learning about the correct decision ex-post, the physician can improve their skill and make more accurate assessments in the future. However, in the current context, the judges have very limited access to any information about the child ex-post and even if the judge has information about the child, the judge can only learn about the 'correct' decision in extreme cases since only one state of the world is realized.[25]

What sources of information can affect judge learning in the current context? One potential source is incoming cases. However, only around 7% of cases concern children who have been part of a petition for removal before. In a minority of these cases, the first petition was denied. If the situation progresses, the SWC can submit a new petition for removal. Because the petition cannot be based on the same circumstances, it takes on average 2 years until the next petition is submitted. Nevertheless, these new petitions can serve as information for the judge that the previous decision to deny removal was incorrect.

Moreover, children whose first petition was approved can still be part of a new case if (i) the child was removed based on deficiencies in the home environment and the child has now developed behavioral problems that warrant a petition for placement on those grounds as well or (ii) out-of-home care was terminated and a new need for care has arisen.[26] Whether such repeat cases inform judges that the first decision to order out-of-home placement was correct

[24]See Chan et al. (2022) for an excellent study on decision-making and diagnostic skills.

[25]As discussed in Helénsdotter (2023), some decisions made by the SWC can be appealed. If the family requests that care be terminated and the SWC denies the request, the family can appeal to the trial court, 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. Other appeals are treated as standalone cases and are quasi-randomly assigned to any judge.

[26]The SWC needs to submit a petition for placement on the grounds of behavioral problems to place the child in institutional care and use coercive measures such as isolation and body searches.

or incorrect is not evident.

The perhaps most salient source of information that can facilitate learning is decisions made by appellate courts. By affirming or reversing trial judges' decisions, appellate courts send signals informing the judges whether they made the right or wrong decision, from the appellate court's perspective. However, just like trial judges, appellate court judges can only observe noisy information (which largely overlaps with the information available to the trial judge) on which they make their assessment.[27]

An example of an extreme case in which the ex-post (weakly) 'correct' decision is observable is when the child dies. However, if the child dies, the appellate court cannot hand down a judgment, nor can the SWC file a petition for removal. Hence, it is plausible that judges receive little to no information about child mortality. This lack of information can explain why we do not find that premature child mortality decreases if their case is randomly assigned to a more experienced judge. But why do removal and mortality rates *increase* with the experience of the assigned judge?

We show that trial judges respond to an appellate court reversing their prior decision to not remove children from home by removing more children in the following weeks. This response to appellate court reversals could generate a gradual increase in removal tendency with experience. However, the effect diminishes quickly and is not statistically significant after just one month. Hence, it is unlikely that judges 'learn' to remove more children with experience through repeated signals from appellate courts.[28]

Anecdotal evidence suggests that judges might become 'hardened' over time and lower their threshold for when child removal is the dominant choice. In particular, judges with more experience might assign a lower subjective cost (c_j) to removing a child from their home against the family's wishes. This phenomenon would be consistent with our findings.

An alternative explanation is that judges might be influenced by external sources of information, such as news stories.[29] Indeed, judges may learn from

[27]The investigations used for both the trial and appellate judgments are largely the same. On average, the appellate court's judgment is handed down just 3 months after the trial court's judgment.

[28]Judges' short-term response to appellate court signals might be explained by judges adapting to minimize the risk of future reversals but that new signals lead to a rapid decay in response (Bhuller and Sigstad, 2022). Another potential explanation is that trial judges are upset about the reversal, which can affect their decision-making (Eren and Mocan, 2018). We inspect whether reversing a peer's decision to not remove a child from home affects the focal judge's probability of removing a child from home in the subsequent 2-4 weeks by rerunning the regression in Table 3.5, Column 1, but using an indicator taking the value 1 if a colleague at the same court experienced such a reversal as the main regressor. We find no evidence of such peer spillovers (point estimate=0.002, std. err.=0.015).

[29]Philippe and Ouss (2018) find that media exposure to crime increases sentence lengths in criminal cases. However, the effect is only temporary.

media coverage of, e.g., severe maltreatment in the home environment and critique of the child protection system's failure to protect the child. On the other hand, there are ample news stories covering serious maltreatment in out-of-home care in Sweden during the sample period as well. However, since it is not the role of the judges to ensure the quality of out-of-home care, the media's critique of judges and judges' own sense of responsibility might be stronger when judges are exposed to news stories about cases in which child protection agents – and especially fellow judges – failed to remove a child from a harmful environment. Thereby, judges may react more to such news compared to news stories about deficiencies in out-of-home care.

3.7 Conclusion

This paper utilizes detailed Swedish data on child protection cases to explore why judges make different decisions in court cases, holding case characteristics constant. We focus on two potential drivers of variation in judicial decision-making: judge experience and signals from appellate courts regarding the appropriateness of the judge's *previous* decisions. To identify causal effects we exploit the quasi-random assignment of judges to cases together with temporal variation in experience and appellate court signals.

The analysis yields strong and robust evidence that judges become more 'stringent' as they gain experience, i.e. children who are quasi-randomly assigned judges who have one more year of experience are more likely to be removed from home conditional on judge FEs. Interestingly, this increase in judge stringency with time is entirely driven by male judges.

The change in judge behavior with experience is *not* consistent with learning in the sense of increasing the accuracy of the decisions. The decision rule (which follows the Convention of the Rights of the Child) is that children are to be placed in out-of-home care only if it is best for the child from the perspective of the child's health and development. However, children who are quasi-randomly assigned a more experienced judge are *more* likely to die by the year they turn 19.

Intuitively, experience should improve skill. However, information accumulation is central to learning. An important feature of our setting is that there is little to no feedback about the accuracy of the decisions. It is rare that judges ever observe the outcomes of the children they have decided over, and even if they do, it is difficult to assess the decision accuracy as they cannot observe the outcomes of the alternative. This feature – limited and noisy feedback – is common in the court system and might be especially salient in, for example, cases involving involuntary provision of psychiatric or addiction care, child arrangements (e.g., custody and visitation), and refugee applications.

Our results are consistent with a change in judge preferences over time,

but not with 'learning' through appellate court decisions. In particular, we find strong evidence that judges respond to reversals of their previous decision to *not* remove a child from home by removing children at higher rates in subsequent cases, but only during the first month after the reversal. The brief rise in stringency can be driven by judges trying to minimize the risk of future reversals (Bhuller and Sigstad, 2022), but it can also be driven by emotional stress (Eren and Mocan, 2018).

The results of our analysis have important policy implications. First, recent evidence (Helénsdotter, 2023) shows that court-ordered removal of children from their homes adversely affects child mortality in Europe. However, our results suggest that policymakers should not respond to these findings by redirecting more cases to highly experienced judges in the hope that it would reduce mortality. Second, it might be welfare-improving to incorporate learning into the court system by, for example, facilitating evaluation of the short- and long-term outcomes of court decisions.

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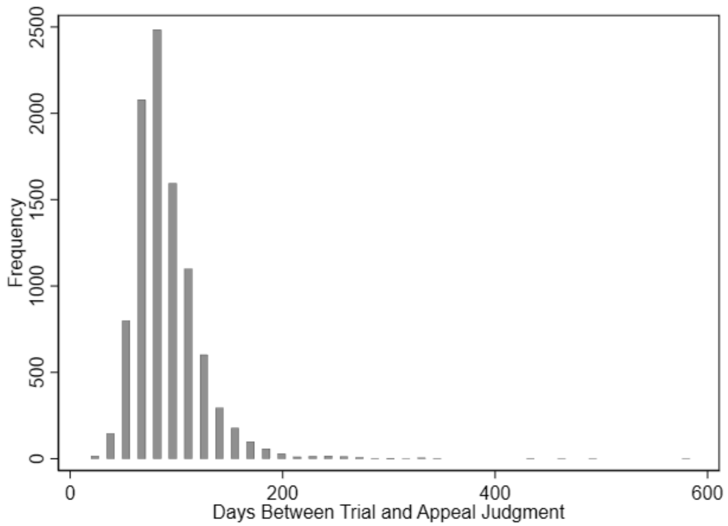
Appendix Tables and Figures

3.A Appendix Figures and Tables

Figure A1. Distribution of Time Between Judgments



Note: This figure depicts the distribution of time from the trial court's judgment to the appellate court's judgment. The 'Appeal Sample' is used.

Table A1. *Descriptive Statistics by Appeal Status*

	Full Appeal Sample	Appealed	Not Appealed
A: Child & Parent Characteristics			
Removed	0.89	0.95	0.84
Girl	0.46	0.49	0.45
Age at judgment	10.79	9.01	12.01
Sibling case	0.32	0.44	0.24
Foreign background	0.38	0.41	0.36
Behavior petition	0.28	0.14	0.38
Environment petition	0.61	0.78	0.49
Double grounds petition	0.11	0.08	0.12
Child consents to removal	0.64	0.73	0.58
At least 1 parent consents to removal	0.36	0.22	0.48
Case largely based on child mental health	0.04	0.04	0.04
Non-junior case type	0.17	0.21	0.14
Committed (yrs t-1 to t-3):			
Crime against person	0.09	0.05	0.11
Narcotic crime	0.10	0.05	0.13
Other crime	0.11	0.06	0.14
Hospitalized (yrs t-1 to t-3) due to:			
Mental health	0.06	0.04	0.08
Substance use	0.05	0.02	0.07
Missing, yrs t-1 to t-3	0.24	0.29	0.20
Any birth parent:			
Dead	0.05	0.04	0.06
<18 y.o. at birth of child	0.02	0.02	0.02
Married, yr t-1	0.45	0.46	0.45
No labor income, yr t-1	0.63	0.70	0.58
Hosp. d.t. mental health, yr t-1	0.07	0.08	0.07
Hosp. d.t. substance use, yr t-1	0.06	0.05	0.06
Any crime, yr t-1	0.16	0.18	0.15
Missing Xs, yr t-1	0.24	0.23	0.25
B: Appeal Characteristics			
Case appealed	0.40	1.00	0.00
Days between trial decision and appellate decision	90.58	90.58	.
Appellate overturned prior approval	0.03	0.07	0.00
Appellate overturned prior denial	0.01	0.03	0.00
C: Judge Characteristics			
Judge removal tendency	0.88	0.89	0.88
Female judge	0.53	0.52	0.52
Judge age	49.86	49.99	49.84
Judge experience	8.00	7.99	8.02
Unique judges	777	702	756
Unique cases	19700	7017	12155
Unique children	22643	9277	13372
Unique birth parents	30857	11851	20001
Observations	24413	9561	14124

Note: This table presents descriptive statistics on child, parent, appeal, and judge characteristics by whether the case was appealed. Descriptive statistics in the full 'Appeal Sample' are shown in the first column for comparison. Statistics are shown for observations with non-missing information.

Table A2. *Additional Tests of Random Assignment I*

	Judge Sample					Appeal Sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Removed	Judge Removal Tendency	Female Judge	Judge ≥50 y.o.	Judge Experience	Removed	Judge Removal Tendency	Appellate Court Signal: Wrong, Remove	Appellate Court Signal: Right, Remove
F-statistic	38.55	0.44	1.31	0.94	0.85	48.68	0.85	0.71	0.93
p-value	0.00	0.99	0.14	0.55	0.68	0.00	0.68	0.86	0.56
N	19756	18291	19756	19756	19756	24413	19791	24413	24413

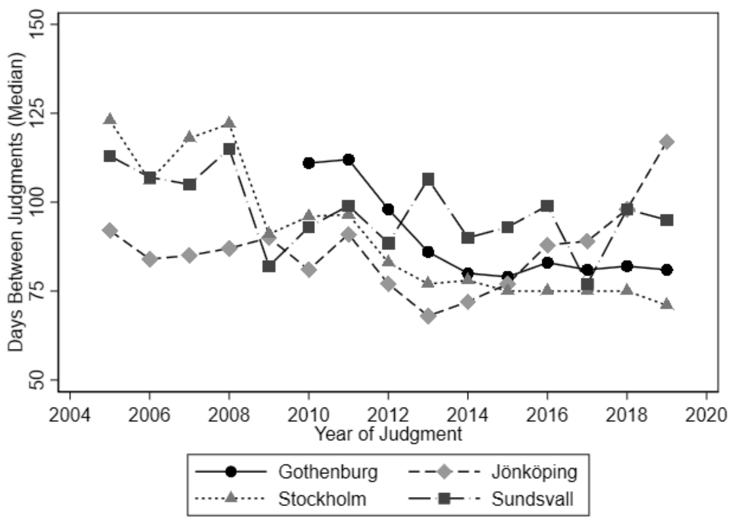
Note: Test of random assignment of judge removal tendency and characteristics to cases using the 'Judge Sample' and the 'Appeal Sample'. All estimations include the child and parent characteristics listed in Table 3.1 and court-by-year FEs. Reported *F*-statistic of joint significance is for the child and parent characteristics only. Standard errors are clustered at the case level.

Table A3. *Additional Tests of Random Assignment II*

	Judge Sample	Appeal Sample
A: Baseline		
F-statistic (*p*-value)	0.44 (0.99)	0.85 (0.68)
N	18291	19791
B: Sample With National Coverage		
F-statistic (*p*-value)	0.58 (0.95)	0.91 (0.59)
N	16685	18144
C: Excluding Non-Junior Cases		
F-statistic (*p*-value)	0.58 (0.95)	0.66 (0.89)
N	15241	16498
D: First-Time Cases		
F-statistic (*p*-value)	0.48 (0.99)	0.86 (0.67)
N	16963	18344
E: Cases Determined ≥24 Months Before Covid-19		
F-statistic (*p*-value)	0.54 (0.97)	0.82 (0.72)
N	14509	15731
*F: Cases in Court*Year Cells With ≥10 obs*		
F-statistic (*p*-value)	0.45 (0.99)	0.86 (0.66)
N	18245	19758
G: Judge Handles ≥20 cases		
F-statistic (*p*-value)	0.46 (0.99)	0.78 (0.77)
N	18056	19452
H: Three-Way Cluster at Case, Child, and Judge Level		
F-statistic (*p*-value)	0.66 (0.89)	1.04 (0.41)
N	18291	19791
I: Court-by-Year FEs Replaced With Department-by-Year FEs		
F-statistic (*p*-value)	0.54 (0.97)	0.98 (0.49)
N	18260	19757
J: Court-by-Year FEs Replaced With Appellate-Court-by-Year FEs		
F-statistic (*p*-value)	0.90 (0.60)	0.82 (0.72)
N	7189	7821
K: Add Day-of-Week and Social Welfare Committee FEs		
F-statistic (*p*-value)	0.43 (0.99)	0.86 (0.66)
N	18282	19782
L: Add Male-Judge-by-Year FEs		
F-statistic (*p*-value)	0.41 (1.00)	0.83 (0.71)
N	18291	19791

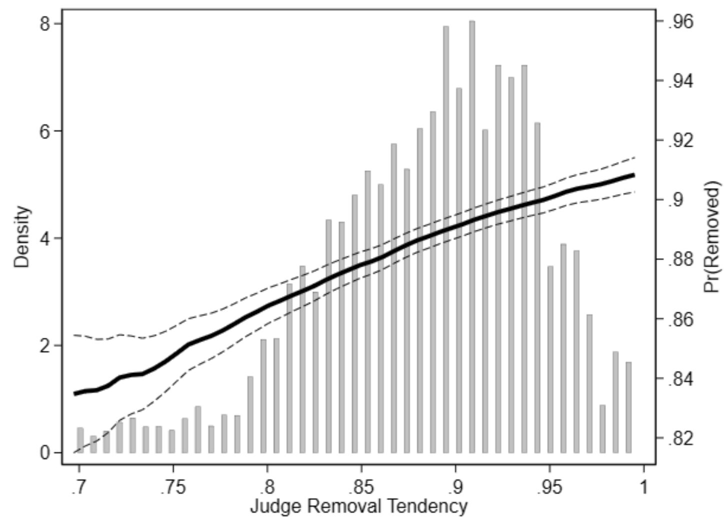
Note: In Panels B-G, we limit the baseline samples to years with universal coverage, cases that are randomly assigned to any judge within the judge pool irrespective of the judge's seniority, the first case for each child, cases decided ≥24 months before February 2020, cases in court-by-year cells with at least 10 observations, and judges who handle at least 20 cases. Decisions related to specification are varied in Panels H-L. Panel H clusters the standard errors on the case, judge, and child level. Panels I-J replace court-by-year FEs with department-by-year FEs and appellate-court-by-year FEs, respectively. Panel K adds FEs for judgment day of the week and SWC. Panel L adds FEs for male-judge-by-year FEs. All estimations include the child and parent characteristics listed in Table 3.1. Reported *F*-statistic (*p*-value) of joint significance is for the child and parent characteristics only. Standard errors are clustered at the case level.

Figure A2. Median Time Between Judgments (by Appellate Court)



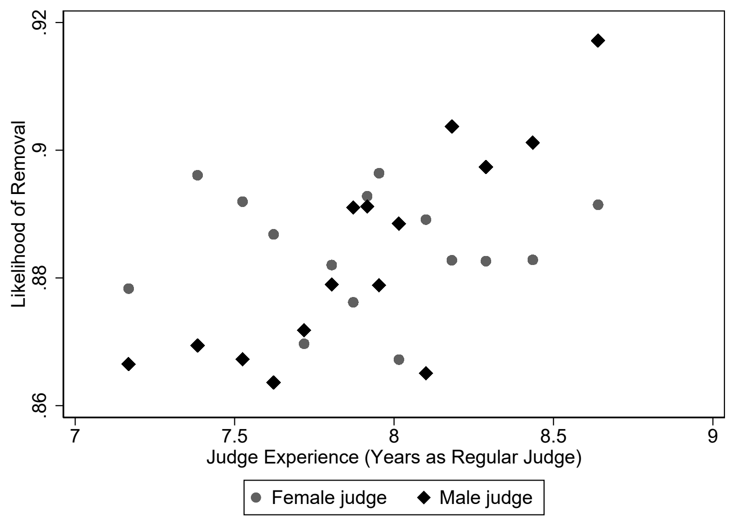
Note: This figure depicts the median time from the trial court's judgment to the appellate court's judgment. The 'Appeal Sample' is used. While the appeal database has national coverage from 2005 onward, the trial database only has national coverage from February 15, 2010, onward. Since no trial courts within the jurisdiction of the Administrative Court of Gothenburg are observable in the trial database before 2010, the median time between decisions cannot be calculated before 2010.

Figure A3. *Relationship Between Removal and Judge Removal Tendency*



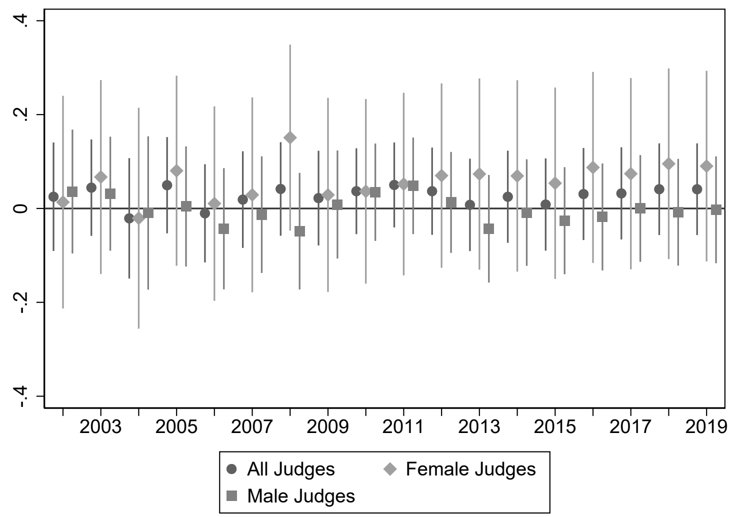
Note: This figure illustrates the variation in judge removal tendency and the relationship between removal and judge removal tendency. The histogram shows the density of judge removal tendency (leaving out the top and bottom 1%). The baseline sample is used but restricted to judges who handle at least 20 cases (see Section 3.3.2). The solid line shows a Kernel-weighted local polynomial regression of removal on judge removal tendency, while the dashed lines show 90% confidence bands. Removal and judge removal tendency are residualized using court-by-year FEs. Settings: triangle Kernel, degree 0, and bandwidth 0.10.

Figure A4. *Effect of Judge Experience on Child Removal by Gender*



Note: This figure plots mean removal rates among cases assigned to male (black diamonds) and female (gray dots) judges whose experience levels fall within the same bin (15 bins of equal size). Removal and judge experience are residualized using court-by-year and judge FEs. The 'Judge Sample' is used.

Figure A5. *Time Trend in Probability of Removal*



Note: This figure shows the OLS point estimates and 95% confidence intervals from regressing removal on indicators for case decision year in the full 'Judge Sample', cases assigned to female judges, and cases assigned to male judges. The base year is 2001. All regressions include a full set of child and parent characteristics (see Table 3.1) and court FEs. Standard errors are clustered at the case level.

Table A4. *Robustness Checks of Effect of Judge Experience on Removal*

	Removed	
	Coeff	Std err
A: Baseline		
Judge experience	0.0178**	0.0070
Observations	19756	
B: Sample With National Coverage		
Judge experience	0.0145*	0.0074
Observations	17885	
C: Excluding Non-Junior Cases		
Judge experience	0.0155**	0.0073
Observations	16458	
D: First-Time Cases		
Judge experience	0.0188***	0.0061
Observations	18324	
E: Cases Determined ≥24 Months Before Covid-19		
Judge experience	0.0238***	0.0082
Observations	15581	
*F: Cases in Court*Year Cells With ≥10 obs*		
Judge experience	0.0177**	0.0070
Observations	19701	
G: Judge Handles ≥20 Cases		
Judge experience	0.0197***	0.0072
Observations	18056	
H: Three-Way Cluster at Case, Child, and Judge Level		
Judge experience	0.0178**	0.0077
Observations	19756	
I: Court-by-Year FEs Replaced With Department-by-Year FEs		
Judge experience	0.0158**	0.0071
Observations	19728	
J: Add Day-of-Week and Social Welfare Committee FEs		
Judge experience	0.0171**	0.0069
Observations	19749	
K: Add Male-Judge-by-Year FEs		
Judge experience	0.0180**	0.0070
Observations	19756	

Note: Panels B-F limit the 'Judge Sample' to years with universal coverage, cases that are randomly assigned to any judge within the judge pool irrespective of the judge's seniority, the first case for each child, cases decided ≥24 months before February 2020, cases in court-by-year cells with at least 10 observations, and judges who handle at least 20 cases. Panel H clusters the standard errors on the case, judge, and child level. Panel G replaces court-by-year FEs with department-by-year FEs. Panel J adds FEs for judgment day of the week and SWC while Panel K adds male-judge-by-year FEs. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A5. *Heterogeneity of Effect of Appellate Court Reversal on Removal I*

	Gender			Background			Placement Grounds			Age at Placement		
	(1) Girl	(2) Boy	(3) Δ	(4) Foreign	(5) Native	(6) Δ	(7) Environ.	(8) Behavior	(9) Δ	(10) $<$13 yrs	(11) $\geq$13 yrs	(12) Δ
Appellate Court Signal (2-4 Weeks Prior):												
Wrong, remove	0.0694**	0.0822***	-0.0280	0.0800**	0.0796***	0.0144	0.1108***	-0.0064	0.1151***	0.1462***	0.0172	0.1206***
	(0.0340)	(0.0263)	(0.0412)	(0.0343)	(0.0286)	(0.0439)	(0.0308)	(0.0327)	(0.0441)	(0.0346)	(0.0275)	(0.0422)
Dep. mean	0.8773	0.8921		0.9038	0.8736		0.8530	0.9406		0.8519	0.9164	
N	11260	13034	24295	9219	15059	24284	14874	6813	21691	11739	12567	24311

Note: Removal is regressed on an indicator that takes the value 1 if an appellate court handed down a judgment 2-4 weeks before the date of deliberation in the focal case and overturned the assigned judge j's prior decision to *not* remove a child from home. We limit the 'Appeal Sample' to the subgroup specified at the top of each column. All estimations include court-by-year and judge FEs. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A6. *Heterogeneity of Effect of Appellate Court Reversal on Removal II*

	Judge Gender			Judge Age (Years)			Judge Experience (Years)		
	(1) Female	(2) Male	(3) Δ	(4) $>$50	(5) $\leq$50	(6) Δ	(7) $>$6	(8) $\leq$6	(9) Δ
Appellate Court Signal (2-4 Weeks Prior):									
Wrong, remove	0.0661**	0.0916***	-0.0117	0.0978***	0.0495*	0.0401	0.1069***	0.0349	0.0643
	(0.0274)	(0.0329)	(0.0419)	(0.0303)	(0.0299)	(0.0423)	(0.0286)	(0.0319)	(0.0425)
Dep. mean	0.8866	0.8839		0.8813	0.8891		0.8816	0.8903	
N	12844	11564	24413	11689	12719	24409	14029	10370	24402

Note: Removal is regressed on an indicator that takes the value 1 if an appellate court handed down a judgment 2-4 weeks before the date of deliberation in the focal case and overturned the assigned judge j's prior decision to *not* remove a child from home. We limit the 'Appeal Sample' to the subgroup specified at the top of each column. All estimations include court-by-year and judge FEs. Standard errors are clustered at the case level. * $p < .1$. ** $p < .05$. *** $p < .01$.

Table A7. *Robustness Checks of Effect of Appellate Court Reversal on Removal*

	Removed	
	Coeff	Std err
A: Baseline		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0731***	0.0213
Observations	24413	
B: Sample With National Coverage		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0793***	0.0213
Observations	22154	
C: Excluding Junior Judges		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0726***	0.0222
Observations	19528	
D: Excluding Non-Junior Cases		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0601***	0.0228
Observations	20348	
E: First-Time Cases		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0671***	0.0214
Observations	22605	
F: Cases Determined ≥24 Months Before Covid-19		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0689***	0.0266
Observations	19254	
*G: Cases in Court*Year Cells With ≥10 obs*		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0731***	0.0213
Observations	24371	
H: Judge Handles ≥20 obs		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0750***	0.0218
Observations	19452	
I: Three-Way Cluster at Case, Child, and Judge Level		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0731***	0.0219
Observations	24413	
J: Court-by-Year FEs Replaced With Department-by-Year FEs		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0757***	0.0218
Observations	24386	
K: Court-by-Year FEs Replaced With Appellate-Court-by-Year FEs		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0786***	0.0287
Observations	9480	
L: Add Day-of-Week and Social Welfare Committee FEs		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0800***	0.0215
Observations	24407	
M: Add Male-Judge-by-Year FEs		
Appellate court signal (2-4 weeks prior): Wrong, remove	0.0737***	0.0213
Observations	24413	

Note: Panels B-F limit the 'Appeal Sample' to years with universal coverage, cases handled by regular judges, cases that are randomly assigned to any judge within the judge pool irrespective of the judge's seniority, the first case for each child, cases decided ≥24 months before February 2020, cases in court-by-year cells with at least 10 observations, and judges who handle at least 20 cases. Panel I clusters the standard errors on the case, judge, and child level. Panels J-K replace court-by-year FEs with department-by-year FEs and appellate-court-by-year FEs, respectively. Panel L adds FEs for judgment day of the week and SWC while Panel M adds male-judge-by-year FEs. * $p < .1$. ** $p < .05$. *** $p < .01$.

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