

Life After Divorce: Effects of Joint Custody on Parents and Children*

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Abstract: Divorce reshapes family life, yet little is known about one of its most consequential features: the allocation of child custody. We study the impact of joint versus sole custody on both parents and children using rich administrative data from Sweden linked to over 25 years of newly-collected court custody rulings. To address selection concerns, we exploit random assignment of custody disputes to judges who differ sharply in their propensity to grant joint custody. For fathers, joint custody substantially raises earnings and improves mental health, consistent with sustained paternal involvement enhancing labor market attachment and psychological well-being. In contrast, there are no measurable labor market or mental health effects for mothers. Turning to children, joint custody increases standardized test scores and school quality without affecting mental health outcomes. Joint custody increases fathers' chances of remarriage, keeps separated parents in closer geographic proximity, and has no effect on intimate partner violence allegations against either partner. These findings inform longstanding debates over the role of child custody in shaping post-divorce family life.

Keywords: Child custody, divorce

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1 Introduction

Divorce is a pervasive feature of modern family life. Over the second half of the 20th century, the share of children experiencing parental divorce rose substantially across the developed world. Alongside this demographic transformation, policymakers, courts, and scholars have devoted considerable attention to understanding how divorce affects both children and parents. Yet one of the most consequential aspects of the divorce process—the allocation of child custody—has received remarkably little systematic study. When parents separate, the legal system must decide whether children will live primarily with one parent (sole custody) or continue to be raised jointly by both (joint custody). This decision can have profound implications for children’s upbringing, parents’ economic and psychological well-being, and the functioning of families more broadly.

Historically, custody law treated children as the property of their fathers, but by the mid-19th century courts began adopting the “tender years” doctrine, which presumed that young children were best cared for by their mothers and thus led to maternal sole custody as the norm (Mason 1994; Mnookin 1975). Over the course of the 20th century, this doctrine was replaced by the “best interests of the child” standard, which emphasized children’s welfare over parental claims (Scott and Emery 2014). In recent decades, many jurisdictions have interpreted a child’s best interests as involving sustained relationships with both parents, leading to the rise of joint custody as a legal default (Elrod 2006).

This shift raises fundamental questions about the consequences of joint relative to sole custody. Proponents argue that joint custody promotes children’s well-being by preserving bonds with both parents. Yet critics caution that such arrangements may come at a cost: children may be more frequently exposed to inter-parental conflict, face inconsistent parenting styles, and bear emotional burdens and stress associated with divided living arrangements. For fathers and mothers, joint custody may help sustain one’s identity as a parent and distribute caregiving responsibilities more evenly, but it can also constrain geographic mobility, prolong unwanted contact with former partners, and heighten stress during an already difficult transition. Despite the importance of these tradeoffs, there is a lack of credible evidence on whether joint custody yields better outcomes for children and parents than sole custody.

A central challenge in answering this question is the scarcity of suitable data. While most countries maintain high-quality registers on marriage, childbearing, and divorce,

these records typically do not include custody outcomes. Large-scale surveys such as censuses or household panels provide extensive demographic and socioeconomic data, but they also lack information on custody arrangements. By contrast, surveys which capture custody arrangements are small, cross-sectional, or limited to specific populations. As a result, researchers have had little empirical basis on which to assess its consequences. We address this limitation by collecting the near-universe of custody rulings from Swedish district courts over several decades, digitizing the documents into structured form using a combination of text-processing algorithms and manual validation, and linking them to population-wide administrative registers. The resulting dataset provides, for the first time, systematic information on custody arrangements together with rich longitudinal measures of parents' and children's economic, health, and educational trajectories.

A second, equally important challenge is causal identification. Custody arrangements are not randomly assigned: parents who end up with joint custody are likely to differ systematically from those who do not, both on observable dimensions such as education and earnings and on unobservable traits such as conflict levels, parenting styles, and willingness to cooperate. These differences create serious concerns about selection bias, as simple comparisons may confound the effects of joint custody with pre-existing differences between families. To address this problem, we exploit a feature of the Swedish legal system. When separating parents cannot agree on custody, the case is brought before a district court and assigned to a judge at random. Crucially, judges differ markedly in their propensity to award joint custody, with some granting it in fewer than one-fifth of cases and others in more than half.

We leverage this heterogeneity in judicial preferences to implement an instrumental variables (IV) design that isolates exogenous variation in custody arrangements. We show that judicial preferences strongly predict custody rulings but are uncorrelated with predetermined characteristics of parents and children. Thus, our design allows us to compare otherwise similar families whose custody arrangement differs only because they happened to be assigned to a stricter or more lenient judge.

Our first set of findings indicates that custody decisions have heterogeneous effects on fathers versus mothers. For fathers, joint custody leads to large and sustained improvements in both economic and psychological well-being. Fathers awarded joint custody experience persistent increases in earnings, a finding driven by those in the bottom of the

earnings distribution. For example, three years after being awarded joint custody, fathers are 25 percentage points more likely to be earning above the 25th percentile. We also find striking improvements in mental health: fathers with joint custody are significantly less likely to use medication for depression in the years following the custody ruling. Three years after the ruling, fathers awarded joint custody are 17 percentage points less likely to use antidepressants. Moreover, we find that the fathers whose earnings improve are the same ones who experience better mental health. These results are consistent with the idea that joint custody is central in a fathers life: it allows fathers to preserve their parental identity and responsibilities, which fosters stability and purpose in both work and family life. For mothers, in contrast, we find no systematic evidence that joint custody affects either their earnings or mental health. This asymmetry likely reflects different counterfactuals. For fathers, the alternative to joint custody is often losing day-to-day involvement with their children. For mothers, the alternative is usually sole maternal custody, with our results suggesting that joint custody does not have the positive effect of easing time constraints nor the negative effect of exacerbating stress (although, of course, the two could cancel each other out).

Turning to children, economic models of parental investment provide a natural framework for understanding how custody arrangements may shape post-divorce outcomes. In the Becker–Tomes framework (Becker and Tomes 1979; Becker and Tomes 1986; Page 2024), children’s human capital is produced through parental investments, suggesting that joint custody may improve children’s long-run outcomes by preserving the involvement of both parents and facilitating investments in children’s human capital. Custody arrangements may also affect parents’ own well-being and labor market outcomes, thereby improving the conditions for investments in children. However, as emphasized by Cunha and Heckman (2007), the productivity of parental investments depends on the quality and consistency of those investments, implying that any gains from greater parental involvement may be offset by coordination problems or continued parental conflict.

Consistent with human capital improvements, our results show that children assigned to joint custody exhibit 47% of a standard deviation higher test scores. Children are also more likely to attend higher-quality primary schools, as measured by school value-added. These findings align with both greater parental involvement in education and improved economic resources, as fathers’ increased earnings expand the set of feasible investments

and choices. In terms of mental health, we do not detect any impact on prescriptions for depression, anxiety, or ADHD. The absence of adverse mental health effects indicates that concerns about instability from different parenting styles or moving between households do not translate into measurable harm for children. Combined, these results indicate that joint custody is a net positive for children: it benefits them academically without detectable costs to their mental health.

Finally, we examine the consequences of joint versus sole custody in domains extending beyond earnings, health, and education. A frequently cited concern is that the continued interaction between former spouses necessitated by joint custody may impede their ability to fully disengage and move forward with their lives. Empirically, joint custody does change the geography of the family, with the parents being more likely to live in the same municipality. Even though geographic mobility falls, joint custody appears to facilitate, rather than hinder, fathers starting anew. Joint custody increases the likelihood that fathers remarry in the years following the ruling: three years after the ruling, they have a 14 percentage point higher chance of remarriage. The higher rate for fathers is driven by those whose mental health improves due to joint custody. A related concern about joint custody is the possibility that it prolongs mothers' and children's exposure to abuse from former partners. Our estimates provide no evidence in support of this claim, although the estimates are imprecise.

Our results carry important implications for policy. Joint custody has long been the subject of controversy, reflecting not only opposing views on what arrangements best serve children but also broader conflicts over gender roles and equality. Some scholars have argued that a default of joint custody provides fathers a means of ensuring continued paternal identity and involvement after divorce, which in turn benefits children (Braver and Lamb 2018). Others contend that statutes favoring joint custody obscure pre-divorce disparities in caregiving and may weaken women's bargaining position following separation (Scott and Emery 2014). In addition, concerns have been raised that, in cases involving domestic abuse, strong presumptions in favor of joint custody may expose women and children to continued coercion after separation (Meier 2020). Our findings shed light on these competing perspectives. Joint custody substantially improves fathers' labor market attachment and mental health, while also benefiting children's educational performance. Importantly, these gains do not come at a detectable cost to mothers and joint custody

facilitates new marriages for fathers. Combined, this evidence supports the view that joint custody is not only in children’s interests but may also promote broader family well-being.¹

Our study contributes to several lines of research. A large empirical literature studies the consequences of divorce, to which our work relates at a broad level. One strand exploits divorce law reforms as sources of quasi-experimental variation. The shift from mutual consent to unilateral divorce in the United States during the 1970s is among the most influential episodes: studies find that it sharply increased divorce rates (Friedberg 1998) and was associated with worse long-term outcomes for children (Gruber 2004). Yet such estimates cannot be interpreted as the causal effect of divorce itself. Because reforms alter the outside options within marriage, they affect not only families that eventually separate but also intact families, by shifting intra-household bargaining power and reducing incentives to invest in marital stability (Stevenson 2007). In contrast, a second strand of work seeks to study the effects of divorce at the individual level. These studies employ designs such as sibling comparisons that exploit differential exposure across children within the same family (e.g., Björklund and Sundström 2006; Francesconi, Jenkins, and Siedler 2010), event studies around the timing of divorce (Johnston, Jones, and Pope 2025), child-age-specific divorce restrictions (Hertegård 2025), and IV strategies using workplace sex composition (Frimmel, Halla, and Winter-Ebmer 2024; Grätz and Härkönen 2024).

More directly, our study contributes to the literature on child custody arrangements. Policy reforms again provide useful variation: the adoption of joint custody in the United States decreased women’s bargaining power within marriage (Nunley and Seals 2011; Halla 2013), while Spain’s reform reduced domestic violence (Fernández-Kranz, Nollenberger, and Roff 2025). In more recent work, Adams et al. (2025) find that the adoption of joint custody laws reduced the interstate mobility of fathers in the US, while Kuske (2025) shows that a reform in the Netherlands encouraging co-parenting decreased mother’s wages due to reduced geographical mobility. These papers answer a different question than we do, as their findings capture the equilibrium effects of the policy environment rather than the causal impact of joint versus sole custody for divorcing families. For example, as discussed in Halla (2013), these reforms might change who decides to divorce

¹This conclusion applies to compliers – those couples whose custody arrangements could change if they were assigned a different judge. These compliers exclude, for example, never-taker cases where all judges would agree that sole custody best serves family well-being (e.g., where domestic violence is a concern).

or chooses to co-parent in the event of a divorce.

Other studies examine custody arrangements at the individual level, finding that joint custody is positively associated with children’s school, behavioral, and psychological outcomes (Bauserman 2002; Amato, Kane, and James 2011). Observational studies from Sweden report remarkably similar patterns, with children in joint physical custody exhibiting better psychological well-being and fewer psychosomatic symptoms than children living primarily with one parent after separation (Bergström, Modin, et al. 2013; Bergström, Fransson, et al. 2015). Yet, as this literature recognizes, strong evidence of positive selection into joint custody cautions against interpreting these associations as causal. In sum, while the existing evidence suggests that custody regimes matter, we still lack causal estimates of how joint custody versus sole custody shapes the lives of families after divorce.

Our contribution is to provide rigorous evidence on how custody arrangements shape family life after divorce. Within family economics, the judge-based IV design has been applied in the context of court-ordered child removal (Doyle 2007; Doyle 2008), with a recent study documenting its effects on health and crime outcomes (Helénsdotter 2026). Related work exploits variation across investigators to study the impacts of foster care (Gross and Baron 2022; Baron and Gross 2025). More broadly, judge IV strategies have been central to the economics of crime—used to examine incarceration (Kling 2006; Aizer and Doyle 2015; Bhuller, Dahl, Løken, et al. 2020; Norris, Pecenco, and Weaver 2021; Arteaga 2023; Bhuller, Dahl, Løken, et al. 2018; Grönqvist et al. 2025), pre-trial detention (Dobbie, Goldin, and Yang 2018), and electronic monitoring (Di Tella and Schargrodsky 2013)—and, outside crime, to analyze disability insurance receipt (Maestas, Mullen, and Strand 2013; Dahl, Kostøl, and Mogstad 2014; French and Song 2014; Autor et al. 2019) and consumer bankruptcy (Dobbie and Song 2015).

The paper is structured as follows: Section 2 describes how child custody is decided in Sweden, the randomization of judges to cases, and the data. Section 3 outlines the empirical strategy. Section 4 presents our main results for fathers, mothers, and children, followed by several specification checks in Section 5. Results for additional family outcomes appear in Section 6. Section 7 offers concluding remarks.

2 Institutional Setting and Data

In this section, we first explain how child custody is determined in Sweden after parents divorce or separate. We then describe our newly constructed database of child custody cases and the administrative records we link to these cases.

2.1 Child Custody in Sweden

In Sweden, all children under the age of 18 must be under the legal custody of one or two adults (Government Offices of Sweden 2025). At birth, a child is under the legal custody of both parents if they are married. If the parents are not married at the time of birth, the mother initially has sole custody until paternity is established and the parents sign a joint custody agreement through the municipal social services office. Among cohabiting unmarried parents, 97% complete this process within the child’s first year of life (Schiratzki 2014).

When parents separate or divorce, joint custody remains the default arrangement. Divorcing or separating parents who wish to establish a different custody arrangement can turn to their municipality’s social welfare board for mediation and assistance in drafting custody agreements. If the parents reach an agreement, it is filed with the social welfare board and becomes legally binding. If parents cannot reach an agreement, either parent can file a legal case with the district court where the child resides. Because joint custody is the default arrangement following parental separation, most legal-custody cases reaching the courts involve at least one parent requesting sole custody rather than the continuation of joint custody. To initiate a dispute, one parent files a formal custody petition outlining their preferred custody arrangement and the reasons behind their request. This parent becomes the plaintiff and the other parent the defendant. When the petition is submitted to the district court, it is assigned a case number and a judge. Obtaining legal representation is not mandatory but common, and individuals with low income qualify for state-funded legal assistance.²

For married parents, custody disputes may arise alongside divorce proceedings. In Sweden, divorce is no-fault, meaning either spouse can request a divorce without stating

²Most custody cases are resolved at the district court level, as civil disputes generally cannot be appealed to a higher court (SFS 1949:381, 29 kap. 11 §). After a case is closed, parents are allowed to file a new case with the district court as long as the child is under age 18. This study is limited to the first case between two parents.

a reason. When only one spouse requests the divorce or children younger than 16 live in the household, a six-month reconsideration period applies before the divorce can be finalized. Custody disputes, however, are governed separately under the Children and Parents Code and may proceed in parallel with the divorce proceedings. In contrast to countries such as the U.S., alimony and child support payments are rarely part of judicial custody proceedings in Sweden.³

Once a custody case is filed with the district court, the assigned judge is responsible for determining a custody arrangement. The judge initially calls the parents to a preliminary hearing. At this stage, the judge clarifies the parties' claims and typically encourages the parents to reach an agreement without having the case proceed to a main hearing. If the parents reach an agreement during or after the preliminary hearing, it is formalized as a court ruling and signed by the judge. If no agreement is reached, the case proceeds to a main hearing before the same judge and two lay jurors.⁴ Parents may testify during this hearing, and additional witnesses can also be called, with the judge presiding over the proceedings. After the main hearing, the court issues a written ruling specifying a legally binding custody arrangement.

During the proceedings, judges can also take several procedural actions that shape the course of the case. First, judges can refer parents to cooperation talks with municipal family services, which occurs in 16% of cases. Second, judges may order the municipality to conduct a formal investigation into the child's well-being and living situation, which happens in 40% of cases. These investigations are typically carried out by a social worker who interviews the parents and the child and provides a comprehensive assessment to the court. In our empirical analysis, we examine whether these additional judicial decisions affect our results on joint custody and find that they do not.

³The Swedish Social Insurance Agency typically determines and enforces child support payments rather than the courts. Amounts are based on parents' reported incomes and child expenses (which depend on the fraction of time a child lives with each parent) and are usually below 2,000 SEK per month (less than 200 US dollars) (SFS 1996:1030). Civil disputes over payments are rare and generally arise only when a plaintiff seeks a significantly higher amount due to exceptional conditions. In our data, child support rulings ordering payments from mothers or fathers appear in only 5% of cases.

⁴Each district court maintains a large pool of appointed lay jurors, who serve a role similar to juries in the American and British legal systems. They typically serve around 10 to 15 days per year. Any individual over the age of 18 is eligible to become a lay juror (Ahlsjö, Niknami, and Palme 2024). While we do not observe the identity of lay jurors, this should not create bias, as they are randomly assigned to cases within a district.

2.2 Defining Joint Custody

In custody disputes, which are classified as civil cases, judges may adjudicate one or more of the following issues: (i) legal custody (*vårdnad*), which determines who has decision-making authority over the child. Most importantly, legal custody governs who has the authority to decide where the child lives; it also encompasses broader parental decision-making responsibilities such as those related to schooling and medical care; (ii) residence (*boende*), which governs where the child lives and is determined when parents share legal custody but dispute the living arrangements; and (iii) contact or visitation rights, which regulate the child’s contact with the non-resident parent. In the Swedish legal framework, residence is conceptually subordinate to legal custody, as custodial parents retain ultimate decision-making authority. This implies that when one parent is granted sole legal custody, the other parent may retain contact rights but lacks decision-making authority and therefore plays a more limited role in the child’s day-to-day life.

Judges have substantial discretion in adjudicating these issues, and the law requires that decisions be made in the “best interests of the child” (SFS 1949:381, 6 kap.). In making this determination, judges consider factors such as the child’s need for close relationships with both parents, the parents’ ability to cooperate, the risk of neglect or abuse, and the child’s own preferences depending on age and maturity.

In our data, the primary margin of adjudication is legal custody: in approximately 85% of cases, judges decide whether legal custody should be joint or assigned to one parent. In the remaining cases, legal custody is not at issue, and judges instead rule on the child’s residence. Such rulings arise in cases where parents share legal custody but disagree over living arrangements, as courts adjudicate residence separately only when legal custody is joint.

To maximize statistical power and simplify our estimation strategy, we construct a single measure of joint custody that encompasses both types of cases. Specifically, in our baseline specification, we define joint custody as either (i) joint legal custody when legal custody is contested, or (ii) shared residence when parents have joint legal custody but dispute the child’s residence. This definition captures cases in which both parents have substantial involvement in the child’s life, either through joint decision-making authority or through significant time spent with the child through joint residence. Because our empirical design leverages judicial discretion over the specific issue at hand, our treatment

captures the judge’s decision over whether both parents remain substantially involved in the child’s life on the contested custody margin. As we demonstrate in Section 5.2, restricting our sample strictly to legal-custody disputes yields similar results, confirming that our results are not driven by bundling these margins.

Using this definition, joint custody is assigned in 37% of cases in our estimation sample. In the remaining cases with sole custody, the mother is awarded custody in 79% of cases, with the remainder corresponding to sole paternal custody.

Ideally, one would distinguish between the full set of custody arrangements by estimating a model with separate endogenous indicators for joint custody and sole maternal custody, with sole paternal custody as the omitted category. This would allow the effects of different custody regimes to be estimated simultaneously relative to a common baseline. In practice, however, this approach is not feasible because sole paternal custody is rare, leaving insufficient variation to identify effects in a multi-treatment IV framework.

We therefore adopt a more parsimonious approach and focus on a single treatment margin. In our baseline specification, treatment is defined as joint custody, with the comparison group consisting of cases in which one parent is granted sole custody. Because maternal custody accounts for the large majority of sole custody rulings, the resulting estimates primarily reflect a comparison between joint custody and sole maternal custody, although paternal custody is also included in the control group. This captures a natural and policy-relevant margin: whether both parents remain actively involved in a child’s life versus a regime in which one parent—typically the father—has limited day-to-day involvement.

To assess the robustness of this definition, we also consider an alternative specification where treatment is defined as sole maternal custody. In this case, the comparison group consists of arrangements in which fathers retain involvement through either joint custody or sole paternal custody. This alternative definition captures an important margin of parental involvement post divorce, namely, whether a father continues to play a substantial role in a child’s life. As we show below, both approaches yield qualitatively similar conclusions, suggesting that the key difference picked up in our estimates is joint versus sole maternal custody.

2.3 Assignment of Cases to Judges

There are currently 48 district courts in Sweden, each covering a specific geographical area. Most district courts are organized into divisions and sections, although smaller courts may consist only of sections. A typical section comprises one judge, one law clerk, and several administrative staff. Our data identify judges and the district courts to which they are assigned, but not their divisions or sections. District court judges are appointed for life and adjudicate both criminal and civil cases. Eligibility requires a law degree followed by judicial training within the Swedish court system.

Available institutional evidence indicates that civil cases were allocated through procedures designed to ensure random assignment throughout our main sample period (2005-2021). Starting in 2003, Swedish law explicitly required that “the allocation of cases and matters shall be done by the drawing of lots.”⁵ To implement this requirement, Swedish courts have used computerized case management systems or procedures which closely approximate random assignment. In 2018, the law was amended (SFS 2018:430) and no longer explicitly required random assignment, but most courts have since adopted the computerized case management system VERA, which randomly allocates cases upon filing.⁶ Under these assignment procedures, judges should receive comparable cases on average. In Section 3.2, we assess this implication by testing for balance in predetermined case characteristics.

2.4 Data

Our analysis draws on a newly constructed data set of the near universe of Swedish custody cases from 1992 to 2021. We combine the court data with administrative records containing information on demographics, earnings, health, test scores and intimate partner violence.

Custody Rulings. We collected 56,188 court rulings in Sweden for cases where parents were disputing legal or physical custody of their children. We issued formal requests

⁵A 2002 amendment (SFS 2002:798) to the original tingsrättsinstruktion (SFS 1996:381) explicitly specified the drawing of lots. A 2011 government inquiry (SOU 2011:42) indicates that different forms of random allocation (e.g., sequential assignment) were already standard practice before 2003.

⁶We contacted all Swedish district courts to verify their current assignment procedures. All courts confirmed that family law cases are randomly assigned through the computerized case management system (VERA), except Gothenburg District Court, where cases are first randomly assigned to a division and case officer before the responsible judge is assigned according to the court’s scheduling system.

to all district courts for their historical data in the spring of 2021, receiving access to court documents for 45 out of the 48 district courts.⁷ Between 2010 and 2021, all courts archived data digitally (with some courts switching to digital archives even earlier); this gave us access to PDF copies of 36,697 cases. We complemented these digital records with hand-collected cases starting in 1992 from some of the largest district courts in Sweden (plus smaller courts in close proximity) as well as from documents found at the National Archives.⁸ These courts allowed us to visit their on-site physical archives where we hand-scanned hard copies of custody rulings, adding 19,491 cases to the dataset.

To transform the text of the custody rulings into a dataset we developed an algorithm in Python that relies on regular expressions, keyword search, and the Swedish version of Google’s natural language processing model BERT, trained by the National Library of Sweden. We capture (i) the personal ID numbers of parents and children involved in the case, which enables us to merge the custody ruling to administrative records, (ii) the name of the judge issuing the ruling, and (iii) which parent is assigned legal custody or physical custody (depending on what is under dispute). We also collected contextual information about the case, including which interventions were ordered by the judge in the case.

Register Data. We link the court records on custody rulings to population-wide administrative registers maintained by Statistics Sweden, the National Board of Health and Welfare (Socialstyrelsen), and the Swedish National Council for Crime Prevention (Brottsförebyggande rådet). Linkage is possible because both the court records and the administrative registers contain unique personal identification numbers.⁹ To protect confidentiality, Statistics Sweden replaced all personal identification numbers with anonymized identifiers before providing access to the linked data.

The Statistics Sweden registers cover the entire Swedish population between 1972 and 2022. They include demographic characteristics (birth year, gender, and country of birth), annual information on place of residence, earnings, employment, educational attainment,

⁷Falu, Hälsingland, and Lycksele did not share court documents with us.

⁸These district courts are Attunda, Gothenburg, Helsingborg, Hässleholm, Lund, Malmö, the National Archives in Lund, Stockholm, Uppsala, and Varberg. Eight of the district courts we have historical data for no longer exist, in most cases because they were merged with a larger district court still in operation today. The total number of distinct district courts in our sample is 53.

⁹In a small number of cases, the personal identification number of either a parent or a child is omitted from the custody ruling, either for security reasons or at the request of the parents. We recover these missing identifiers using the Multi-Generation Register from Statistics Sweden. For only children, we recover the child’s identifier directly. For parents, we recover the identifier of the other biological parent using the observed parent-child link.

and detailed family linkages. The education registers also contain information on national standardized tests in grades 3, 6, and 9, as well as school identifiers, allowing us to construct school value-added measures.

To measure mental health, we use the Prescribed Drug Register maintained by the National Board of Health and Welfare, which records all dispensed prescription medications in Sweden from 2005 onward. The register contains the date of prescription, the dispensing date, and the Anatomical Therapeutic Chemical (ATC) classification code.¹⁰

To identify intimate partner violence, we use the Swedish Suspicion Register maintained by the Swedish National Council for Crime Prevention. The register includes all individuals aged 15 or older who were identified as criminal suspects following a police or prosecutorial investigation from 1995 onward. Because only a small fraction of intimate partner violence cases result in criminal convictions, we instead focus on cases where individuals are suspected of a crime. For violent and sexual offenses, the register records the relationship between the victim and the suspect, allowing us to measure offenses committed against a current or former intimate partner.

2.5 Sample Restrictions

Our empirical design relies on two related samples: one used to construct the judge propensity instrument and another used in the empirical analyses below. We therefore impose a sequence of restrictions designed to isolate discretionary custody decisions and ensure reliable measurement of both the instrument and the outcome variables.

Sample Used to Construct the Instrument. Our research design requires constructing a measure of each judge’s propensity to assign joint custody. This requires us to focus on cases in which judges exercise meaningful discretion over joint versus sole custody assignments. We therefore exclude three types of cases in which judicial discretion is effectively absent or substantially constrained.

First, we exclude special cases in which a parent who has recently migrated to Sweden with children seeks sole custody from a parent who resides abroad and/or is unreachable.¹¹

¹⁰We use ATC code N06A for antidepressants, N05B and N05C for anxiolytics and sedatives, and N06B for ADHD medication.

¹¹These cases are special since there is only one outcome, which is to assign sole custody to the parent in Sweden. Upon arriving in Sweden, children are initially registered under the custody of both parents. However, when the parent residing in Sweden needs to make education or healthcare decisions,

This restriction excludes 7,967 cases from the initial sample of 56,188 custody disputes.

Second, we exclude cases that appear to be non-discretionary and merely formalize agreements already reached by the parents before the case proceeds in earnest.¹² This restriction excludes an additional 5,898 cases.

Third, we exclude re-opened custody disputes involving families with prior custody litigation. In such cases, judges may observe extensive prior proceedings and family histories that are unavailable in first-time custody disputes. As a result, these rulings are less likely to reflect comparable discretionary decisions and may complicate the measurement of stable judicial tendencies regarding joint versus sole custody. This restriction excludes a further 4,081 cases.

With these restrictions, the sample used to construct the instrument includes 38,242 custody rulings.

Estimation Sample. Additional restrictions are imposed to construct the estimation sample used in the empirical analysis. First, to improve the precision of the leave-out judge propensity measure and mitigate finite-sample bias that can arise when judge tendencies are estimated from very small numbers of cases, we restrict the sample to custody disputes decided by judges who handle at least 50 other custody cases over the full sample period. This restriction reduces the number of custody cases used for estimation from 38,242 to 13,986. While this restriction substantially reduces the sample size, it ensures that judge propensities are measured using a sufficiently large number of cases to provide reliable identifying variation.

Second, because our empirical specifications include court-by-year fixed effects, identification relies on variation in custody decisions across judges operating within the same court and year. We therefore exclude court-years in which all cases are handled by a single judge. This restriction further reduces the estimation sample to 13,959 custody cases.

We next impose a set of restrictions designed to ensure consistent linkage across the administrative registers. Specifically, we omit cases in which either parent's ID is missing

sole custody simplifies the process. As a result, they may file for sole custody, often without the foreign-residing parent, the defendant, being present in the proceedings. The court automatically assigns the defendant a legal representative, who attempts to make contact, often through an embassy. In many cases, these efforts are unsuccessful, and the defendant is deemed "unreachable".

¹²We identify these cases based on ruling length. Decisions shorter than two pages consist almost exclusively of standardized procedural language and formalization of pre-existing parental agreements, whereas rulings involving substantive disagreement typically contain additional factual discussion and legal reasoning that extend beyond this threshold.

or cannot be linked to the administrative records.¹³ This restriction reduces the estimation sample from 13,959 to 13,140 custody cases.

Our main outcomes for parents are labor market earnings and mental health, while for children we focus on educational achievement and mental health. Information on mental health outcomes, measured using prescription drug records, is only available beginning in 2005. We therefore restrict our main estimation sample to custody cases decided in 2005 or later, which ensures that all main outcomes can be measured consistently across families. This restriction reduces the estimation sample from 13,140 to 11,027 custody cases. In robustness exercises, we additionally present estimates for parents' earnings and children's educational outcomes using custody cases decided prior to 2005, when mental health outcomes are unavailable.

With these restrictions, our main estimation sample consists of 11,027 cases involving 17,007 children. Summary statistics for our main estimation sample are found in Appendix Table A1.

Regression weights. Linked to the 17,007 children are 10,902 mothers and 10,899 fathers.¹⁴ While rare, judges can issue different custody rulings for children who are siblings in the same custody case. In our child-level regressions, each child appears once. For the parent-level regressions, we organize the data at the child level, so that parents with multiple children involved in the same custody dispute appear multiple times. To ensure that each parent contributes equally to the estimation, we weight observations so that the weights sum to one within each parent. For example, if a father has three children in a custody dispute, he contributes three observations, each with weight $1/3$, so that his total weight equals one. These weights are intended to prevent parents with larger numbers of children from receiving disproportionate influence in the estimation.

¹³When possible, we use the multi-generation registers to recover missing IDs. For children, this is possible if they are an only child. For parents, we can recover the ID of the other biological parent when the ID of one parent and the child are observed.

¹⁴Fathers and mothers can be linked to multiple cases, which explains why we do not have the same number of observations for fathers and mothers. For example, a father who divorces, remarries, and divorces again can be involved in two custody disputes.

3 Empirical Framework

3.1 Instrumental Variables Model

We study the effect of joint custody on children’s and parents’ outcomes in the years following a divorce or separation. The starting point is the following regression:

$$Y_{i,t} = \beta_t Joint_{i,0} + X_i' \delta_t + \varepsilon_{i,t} \quad (1)$$

where i indexes the father, mother, or child (depending on the outcome), and t denotes time, normalized such that $t = 0$ corresponds to the court ruling. $Y_{i,t}$ is the outcome of interest measured at time t after the ruling, $Joint_{i,0}$ is an indicator for a joint custody ruling, X_i is a vector of predetermined controls, and $\varepsilon_{i,t}$ is an error term. OLS estimation of equation 1 is likely to suffer from selection bias, as the assignment of joint custody is not random. For example, more cooperative parents may be more likely to be awarded joint custody; however, such parents may also have fewer financial constraints, better mental health, and more resources to support their child’s outcomes even in the absence of joint custody.

To address this issue, we exploit the quasi-random assignment of judges to custody cases within district courts and years, together with systematic differences across judges in their propensity to grant joint custody. As described in Section 2.5, judge propensities are constructed using the full set of available custody rulings handled by each judge over the sample period, after imposing restrictions designed to isolate discretionary custody decisions.

We measure a judge’s propensity as the leave-out share of cases in which that judge assigns joint custody, excluding all rulings involving any members of the focal family.¹⁵ To improve precision in the measurement of judicial tendencies and mitigate finite-sample bias in the leave-out measure, we use both past and future cases when constructing judge propensities and restrict the estimation sample to cases decided by judges who handled at least 50 other custody disputes over the sample period. A total of 191 judges satisfy this criterion.

We use a judge’s joint custody propensity as an instrument in an IV regression to

¹⁵This excludes the custody ruling of the focal child and their siblings. Some parents are involved in multiple custody cases with different co-parents; we therefore also exclude rulings involving half-siblings.

estimate the causal effect of joint custody. The first-stage regression can be written as:

$$Joint_{i,0} = \alpha Z_{j(i)} + X_i' \theta + \nu_i \quad (2)$$

where $Z_{j(i)}$ denotes the leave-out propensity of judge j to assign joint custody in individual i 's case. Since assignment occurs within district courts and years, we include fully interacted court-by-year fixed effects in all specifications. These fixed effects absorb any systematic differences across courts and over time in both judicial behavior and case composition.

The IV estimates can be interpreted as a positively weighted average effect of joint custody for compliers, provided that the judge instrument satisfies the standard IV assumptions: exogeneity, relevance, monotonicity, and exclusion. We discuss each of these conditions in turn and provide supporting empirical evidence.

3.2 Evaluating the IV Model

Exogeneity. To assess whether judges are randomly assigned to cases within district courts and years, we test for balance in pre-determined characteristics. In column (1) of Table 1, we first document that background characteristics measured two years prior to a custody ruling predict the probability of being assigned joint custody. Specifically, we regress an indicator for joint custody on court-by-year fixed effects and the set of pre-determined characteristics listed in the table. Variables such as whether the mother is the plaintiff, child age, parents' criminal histories, and parents' earnings are all significant predictors of a joint custody ruling, and the characteristics are jointly significant according to an F-test.

To assess the randomness of judge assignment, we next examine whether these same background characteristics also predict a judge's propensity to grant joint custody. Column 3 regresses the judge instrument on the same set of characteristics and court-by-year fixed effects. In contrast to column 1, the estimated coefficients are small and generally statistically insignificant. Indeed, only three of the 25 coefficients are statistically significant, which is roughly what would be expected due to chance. Moreover, we fail to reject the null hypothesis that the coefficients are jointly equal to zero (p-value = 0.520), indicating no systematic relationship between case characteristics and the joint custody

propensity of the assigned judge.

These results show that while observable characteristics strongly predict custody outcomes, they do not predict the instrument, supporting the assumption that judges are quasi-randomly assigned to cases.

Relevance. The validity of the IV design requires not only quasi-random assignment of judges, but also sufficient variation in judges’ propensities to assign joint custody. To assess this, Figure 1 plots the distribution of judges’ propensities after residualizing with respect to court-by-year fixed effects and recentering at the sample mean of 37%. There is substantial variation in judges’ propensities to assign joint custody, with a standard deviation of 12%. Judges at the 10th percentile assign joint custody in 28% of cases, whereas judges at the 90th percentile do so in 48% of cases.

Figure 1 also plots the probability that a family is assigned joint custody as a function of the assigned judge’s propensity to grant joint custody. The solid line shows estimates from a local linear regression, providing a flexible representation of the first-stage relationship in equation 2. The relationship is monotonically increasing, indicating that families assigned to judges who favor joint custody in general are more likely to receive joint custody in their case.

Table 2 reports the corresponding first-stage estimates. The large F-statistics indicate that the instrument is strong. The coefficient in column 2 implies that assignment to a judge with a 10 percentage point higher propensity to grant joint custody increases the probability of a joint custody ruling by 5.6 percentage points.

Monotonicity. In our setting, monotonicity requires that assignment to a judge with a higher propensity to grant joint custody (i.e., a more lenient judge) weakly increases the probability of receiving joint custody. Frandsen, Lefgren, and Leslie (2023) show that, for continuously valued judge instruments (as in our case), this condition need only hold on average across the support of the instrument rather than at every point. Although this assumption is not directly testable, we provide several pieces of evidence consistent with it.

First, we examine whether the first-stage relationship is non-negative across observable subpopulations. Specifically, we estimate first-stage regressions separately for subsamples defined by key characteristics, such as children’s gender and age or parents’ earnings

and education. Table A2 reports these estimates. Across all subsamples, the first-stage coefficients are positive and statistically significant, indicating that assignment to a more lenient judge increases the probability of joint custody in all groups. This pattern is consistent with average monotonicity.

Second, we implement a reverse-sample instrument test. We construct the judge propensity measure using one subsample (e.g., families with younger children) and use this instrument to estimate the first stage in the complementary subsample (e.g., families with older children). This exercise addresses concerns that the instrument is driven by subgroup-specific decision rules and provides an additional test of whether more lenient judges consistently assign joint custody at higher rates across different populations. Table A3 shows that the first-stage relationship remains positive and significant across all such splits, further supporting average monotonicity.

As an alternative, we follow Frandsen, Lefgren, and Leslie (2023) and implement a test that jointly assesses exclusion and strict monotonicity. The intuition behind this approach is that, under strict monotonicity and exclusion, outcomes should vary smoothly with judge propensity, with slopes bounded by the variation in the instrument. We test whether the relationship between judge propensity and outcomes is consistent with these restrictions. The results, reported in Appendix Table A4, do not reject the null hypothesis implied by the test once the relationship is allowed to be sufficiently flexible, providing support for the stronger strict version of monotonicity and exclusion. Taken together, these results provide evidence that the monotonicity assumption is satisfied in our setting.

Exclusion. Our IV design requires that a judge’s propensity to award joint custody affects parents’ and children’s outcomes only through the custody ruling. Although judges’ primary task is to determine a custody arrangement in the best interests of the child, they can also influence other aspects of the case. As discussed in Section 2, two important margins of influence are referring parents to cooperation talks at their local municipality and ordering formal investigations into the child’s living conditions.

The exclusion restriction would be violated if judges with a higher propensity to award joint custody also affect outcomes through these additional decisions, rather than solely through the custody arrangement. A second concern is that a judge’s propensity to award joint custody may be correlated with other judge characteristics that directly affect parents’ and children’s outcomes. After discussing our main results, we present a series of

robustness tests based on these two additional margins which indicate that the exclusion restriction is likely to hold.

4 Results

This section presents our main findings on the impact of joint custody on parents and children. We begin by examining parental labor market and mental health outcomes. These understudied outcomes are substantively important in their own right and provide useful context for interpreting the subsequent effects on children. We then turn to children’s outcomes, which reflect both the direct impact of custody arrangements and indirect effects operating through changes in parental behavior and circumstances. Finally, we explore how joint custody reshapes the geography of the family, future parental partnerships, and intimate partner violence.

When feasible given the available data, we estimate effects on parents’ and children’s outcomes separately for each of the first five years following the custody ruling in order to trace the dynamics of the treatment effects over time. Because our administrative data end in 2021, the estimation sample becomes progressively smaller at longer post-ruling horizons. For example, parents whose custody disputes were decided in 2019 can only be observed through the third year after the ruling. As a result, estimates for later post-ruling years are based on fewer observations.

4.1 Fathers

For fathers, child custody decisions are particularly consequential. Our baseline joint custody treatment allows them to remain actively involved in their child’s life, whereas in the counterfactual of sole custody—awarded to the mother in 79% of cases—they often lose day-to-day contact with their child. As we show in the robustness section, redefining treatment to be sole maternal custody (where the comparison group is fathers retaining involvement through either joint custody or sole paternal custody) leads to similar results, albeit with the expected opposite signs.

Earnings. We begin by analyzing the effect of joint custody on fathers’ earnings, measured as indicators for whether earnings exceed different percentiles of the age-adjusted,

gender-specific earnings distribution during the sample period. We focus on these distributional measures given the potential for heterogeneous responses across fathers and to accommodate the presence of zero post-separation earnings. Specifically, we regress the probability a father’s earnings exceeds a given quartile on our treatment variable of joint custody, court-by-year fixed effects, and the pre-determined control variables listed in Table 1 which are measured two years prior to the custody ruling. We estimate separate regressions for one to five years after the custody ruling.

Panel A of Table 3 reveals large and persistent increases in earnings among fathers in the lower part of the distribution. Joint custody raises the probability that a father earns above the population-wide 25th percentile of earnings by 17 percentage points in the first year after the ruling. The effect becomes larger and remains statistically significant in years 2-5. To interpret the magnitudes, consider fathers’ earnings in year 3. Only 45% of control compliers are estimated to earn above the 25th percentile.¹⁶ Being granted joint custody increases this probability by 25 percentage points, so that treated fathers have a 70% chance of earning more than the 25th percentile. In other words, joint custody makes treated fathers who otherwise would have had very low earnings prospects look similar to the full Swedish population.

At the median of the earnings distribution (panel B), the estimated impacts of joint custody range from 5 to 17 percentage points, but only the estimates for the first three years after the ruling are statistically significant. Thus, while the point estimates are positive throughout, the evidence for effects at the median is weaker than for the lower tail. At the top of the earnings distribution (panel C), we find little evidence that joint custody affects labor market earnings in the years following the custody ruling. Combined, these results indicate that the earnings response is concentrated among fathers in the lower part of the distribution.

In non-reported regressions, we do not find statistically significant effects on log earnings for the sample of fathers with positive earnings. Combined with the distributional results, this suggests that joint custody primarily affects fathers with weaker labor market attachment, shifting them out of the lower tail of the earnings distribution rather than

¹⁶Control complier means are estimated using the approach of (Abadie 2003) generalized to settings with continuously-valued judge instruments as in (Frandsen, Lefgren, and Leslie 2023). Specifically, for each outcome, we regress the outcome interacted with the complement of treatment status on the complement of predicted treatment status obtained from the leave-out judge propensity instrument, conditional on the same controls used in the main specifications. The resulting coefficients can be interpreted as average outcomes for untreated compliers.

increasing earnings proportionally across the distribution.

As a robustness check, we re-estimate the earnings effects using the full sample of custody cases, including those decided prior to 2005 for which mental health data are unavailable. Appendix Table A5 shows that the results remain qualitatively similar, with positive effects on the probability of earning above the 25th percentile. The magnitudes are somewhat smaller, but the overall pattern—strong effects in the lower part of the earnings distribution—remains unchanged.

Mental Health. We next examine the impact of joint custody on fathers’ mental health. Table 4 reports effects on the probability of using antidepressant and antianxiety medication in a given year following the custody ruling, as well as on the continuous use of these medications over multiple years. As background, the use of these medications is relatively high among fathers engaged in a custody dispute, with roughly 12% of fathers using antidepressants and 14% antianxiety drugs in the first year after a custody ruling. The control complier means are generally even higher.

Joint custody leads to substantial improvements in fathers’ mental health. In the year-by-year estimates in Panel A, the effects are largest for antidepressant use. Joint custody reduces the probability of antidepressant use by 13 percentage points in year 1, 16 percentage points in year 2, 17 percentage points in year 3, and 12 percentage points in year 4, before becoming close to zero in year 5. Relative to control complier means of between 25 and 27% in years 1 through 4, these estimates correspond to reductions of about 46 to 68 percent. These are large effects, indicating substantial reductions in psychological distress following a joint custody ruling. For antianxiety medication, the year-by-year estimates are smaller and imprecisely estimated, with no statistically significant effects in any individual year.

A similar pattern emerges when focusing on continuous use, which captures more persistent mental health problems. Joint custody reduces the probability of continuous antidepressant use for one to five years by between 8 and 16 percentage points, corresponding to reductions of roughly 47 to 75 percent relative to control complier means. For antianxiety medication, the continuous-use estimates are all negative but only statistically significant for the 4- and 5-year time horizons.

Putting the Pieces Together. The earnings and mental health results presented above raise a natural question: are these improvements driven by the same fathers, or do they reflect changes along different margins for different individuals? To address this, we construct year-by-year joint outcomes that combine both dimensions. Specifically, for each time horizon, we define indicators equal to one if, in a given year after the ruling, a father both (i) earns above the 25th percentile and (ii) either uses or does not use antidepressant medication in that same year. These composite outcomes allow us to assess whether improvements in labor market attachment and mental health occur jointly within the same individuals.

Figure 2 presents the estimated effects of joint custody on these joint outcomes for years 1–5 after the ruling. In each year, the first estimate reproduces the baseline effect on earning above the 25th percentile. The subsequent estimates consider two joint outcomes: (i) earning above the 25th percentile while not using antidepressants in that year, and (ii) earning above the 25th percentile while using antidepressants. Together, these outcomes decompose the event of earning above the 25th percentile. The figure shows that the positive earnings effects are almost entirely driven by increases in the probability of earning above the 25th percentile without antidepressant use. In contrast, there is little to no effect on the probability of earning above the 25th percentile while simultaneously using antidepressants.

These results indicate that the improvements in earnings and mental health are linked and occur within the same set of fathers within a given year.¹⁷ Joint custody does not simply increase earnings for one group and improve mental health for another; rather, it shifts fathers into a joint state of stronger labor market attachment and better contemporaneous mental health.

Interpretation. The earnings and mental health results point to a consistent pattern: joint custody has large and lasting positive effects on fathers’ economic and psychological well-being. The earnings effects are concentrated among fathers in the lower part of the

¹⁷To see this, let E denote earning above the 25th percentile, M denote antidepressant use, and J denote joint custody. By definition, $P(E) = P(E \cap \text{not} M) + P(E \cap M)$. We estimate that $dP(E)/dJ > 0$ is driven by $dP(E \cap \text{not} M)/dJ > 0$, since $dP(E \cap M)/dJ \approx 0$ for each of the five years after the ruling. Since $dP(E \cap \text{not} M)/dJ > 0$, some fathers who would not be in the joint state of earning above the 25th percentile and not using antidepressants under sole custody enter that state when assigned joint custody. Combined with the negative effects on antidepressant use documented above, this implies that the earnings gains are concentrated among fathers whose contemporaneous mental health also improves.

distribution, while the mental health effects are particularly pronounced for antidepressant use.

One interpretation is that joint custody allows fathers to maintain an active parental role after divorce or separation. Continued involvement may strengthen incentives to secure stable employment and increase earnings, particularly among fathers with initially weak labor market attachment, while also mitigating the psychological distress associated with losing contact with one’s child. The concentration of effects among lower-earning fathers and in measures of persistent mental health problems suggests that joint custody is especially important for those most vulnerable to adverse shocks following divorce.

It is worth noting that the baseline estimates compare joint custody to a counterfactual that includes both maternal and paternal sole custody. From the perspective of fathers, these represent distinct states: maternal sole custody typically implies losing access to the child, whereas paternal sole custody implies full responsibility. In Section 4.5, we show that redefining the treatment as maternal sole custody—so that the counterfactual consists of arrangements in which fathers retain access to their child—yields qualitatively similar results. This suggests that the main effects for fathers are driven by whether they maintain involvement in their child’s life, rather than by differences between joint and sole paternal custody.

4.2 Mothers

We next examine whether similar patterns arise for mothers. In contrast to fathers, custody decisions for mothers typically do not involve a loss of access to the child. When sole custody is awarded, it is granted to the mother in the majority of cases (79%). As a result, the relevant margin for mothers is not access per se, but rather the extent to which childcare responsibilities and interaction with the ex-partner are shared under joint custody.

Earnings. Table 5 reports the effect of joint custody on mothers’ earnings, measured again as indicators for whether earnings exceed different percentiles of the age-adjusted, gender-specific earnings distribution. In contrast to the results for fathers, we find no evidence that joint custody systematically affects mothers’ earnings.

At the lower end of the earnings distribution, the estimated effects on the probability

of earning above the 25th percentile are small overall. The point estimates are negative in the first two years (-7.6 and -15.4 percentage points, with the latter statistically significant at the 10 percent level) but become close to zero from year 3 onwards. A similar pattern emerges at the median of the earnings distribution, where the estimates are again negative in the first two years (-6.6 and -13.4 percentage points, with the latter significant at the 10 percent level) and likewise fade out in later years. At the top of the distribution, the estimates are statistically insignificant across all years.

The lack of persistence and the absence of a consistent pattern over time suggest that these short-run fluctuations do not reflect systematic effects of joint custody on mothers' earnings. As a robustness check, Appendix Table A6 reports estimates using the full sample of custody cases, including those decided prior to 2005. In this larger sample, the negative point estimates in the first two years are smaller in magnitude and no longer statistically significant, while estimates in later years remain close to zero. Taken together, these results suggest that joint custody does not have a systematic impact on mothers' earnings.

Mental Health. We next examine the impact of joint custody on mothers' mental health. Table 6 reports the effects on the probability of using antidepressant and anti-anxiety medication, both on a year-by-year basis and for continuous use over multiple years.

Consistent with the earnings results, we find little evidence that joint custody affects mothers' mental health. The year-by-year estimates for both antidepressant and anti-anxiety medication are small, generally negative, and statistically insignificant in all years following the custody ruling. While the point estimates suggest a modest reduction in medication use, the effects are imprecisely estimated and not distinguishable from zero.

The results for continuous medication use similarly show no statistically significant effects. Estimates are close to zero across all horizons, indicating that joint custody does not meaningfully affect the likelihood of persistent mental health problems among mothers.

Putting the Pieces Together. We next repeat the joint outcome exercise for mothers. Specifically, for each time horizon, we construct indicators equal to one if, in a given year after the ruling, a mother both (i) earns above the 25th percentile and (ii) either uses or

does not use antidepressant medication in that same year. Appendix Figure A1 presents the corresponding estimates.

In contrast to the patterns observed for fathers, we find little evidence of any effect of joint custody on these composite outcomes. The estimates are small and statistically insignificant across all time horizons, with no systematic differences between mothers who do and do not use antidepressants. This absence of effects largely reflects the lack of impacts on the underlying margins. As shown above, joint custody does not meaningfully affect mothers' earnings or mental health separately, and thus there is little scope for joint improvements to emerge.

Interpretation. Overall, the results indicate that joint custody has no measurable impact on mothers' economic outcomes or mental health. One interpretation is that the relevant counterfactual differs fundamentally between mothers and fathers. For fathers, sole custody often implies losing day-to-day involvement with their child, so joint custody represents a large change in both responsibilities and parental identity. For mothers, in contrast, the alternative to joint custody is typically sole maternal custody, so that in both states of the world they remain highly involved in their child's life.

The scope for changes in mothers' labor supply may also be limited by Sweden's institutional context. Extensive public childcare and preschool provision, together with informal childcare, already enable high rates of maternal labor force participation, reducing the extent to which changes in custody arrangements translate into changes in earnings. At the same time, any reduction in childcare responsibilities under joint custody may be offset by coordination costs and continued interaction with the former partner. The absence of detectable effects suggests that, on average, these forces largely balance out.

The absence of measurable effects for mothers—both along individual margins and in joint outcomes—stands in stark contrast to the large improvements observed for fathers. This asymmetry suggests that custody arrangements primarily operate through changes in paternal involvement and incentives, with potentially important downstream consequences for children. We therefore turn next to children's outcomes.

4.3 Children

We now turn to the effects of joint custody on children. Economic models of parental investments do not make a clear prediction about how custody arrangements affect children’s outcomes. In the Becker–Tomes framework (Becker and Tomes 1979; Becker and Tomes 1986), children’s human capital is produced through parental investments of time, financial resources, and other inputs, implying that custody arrangements may affect children’s long-run outcomes by altering these investments. But if these inputs are not coordinated, or if joint custody increases conflict or stress, the production of child human capital may deteriorate. Similarly, in a dynamic skill-formation framework (Cunha and Heckman 2007; Heckman and Mosso 2014), parental investments are dynamic and complementary. Custody arrangements may therefore matter because they shape continuing parental engagement, school choice, monitoring, and emotional stability. The positive effects on fathers earnings and mental health documented above suggest increased parental investments, but whether these potential gains outweigh any costs arising from coordination problems or parental conflict is ultimately an empirical question that we examine next.

Educational Outcomes. We begin by examining children’s academic achievement. Table 7 reports the effect of joint custody on standardized national test scores for the first national test taken after the custody ruling.¹⁸ Column (1) shows the effect on average test scores, while columns (2) and (3) report results separately for the Swedish and math subcomponents. Joint custody leads to large and statistically significant improvements in children’s performance. Column (1) shows the effect on average test scores, while columns (2) and (3) report results separately for the Swedish and math subcomponents. Joint custody leads to large and statistically significant improvements in children’s performance. On average, test scores increase by 0.47 standard deviations, with similar effects for Swedish (0.36) and math (0.48). This is a large effect relative to the control complier mean of -0.59 standard deviations. In other words, complier children involved in custody disputes perform substantially below the average achievement level of the overall Swedish student population, but joint custody closes most of this achievement gap.

We also examine the quality of the schools children attend. To do so, we construct a

¹⁸Among the 10,814 children in our main estimation sample of custody cases decided in or after 2005, 58% take their first post-ruling national test in grade 3, 24% in grade 6, and 18% in grade 9.

residual school quality measure designed to approximate school value added. Specifically, using the full population of students (not just those in our estimation sample), we regress each student's average test score on child gender and age, the migration status of the child and both parents, parents' highest completed education, and parents' earnings, separately by grade and year. We then compute school-level leave-out residuals, which capture the extent to which students in a given school perform better or worse than predicted based on their observed characteristics. This yields a measure of school quality that is meant to capture differences in school effectiveness rather than differences in student composition. Table 8 shows that joint custody increases this school quality measure in grade 3 by 0.23 standard deviations. Relative to a control complier mean of -0.13, this represents a large improvement. Put differently, the estimated effect is large enough to shift complier children from schools with below-average value added to schools with slightly above-average value added. In contrast, the estimated effects are statistically insignificant for grades 6 and 9.

Appendix Table A7 examines heterogeneity by the child's gender. Panel A shows that the positive effects of joint custody on overall national test scores are primarily driven by girls. This gender difference is particularly pronounced for Swedish test scores, where girls experience large and statistically significant gains following a joint custody ruling, while the corresponding estimate for boys is small, negative, and statistically insignificant. In contrast, the effects on math performance are more similar across genders, with both boys and girls exhibiting improvements in test scores when their parents are granted joint custody.

One possible interpretation is that joint custody partly offsets gendered responses to family disruption. Prior research suggests that boys' adjustment problems after divorce are often more externalizing, while girls may be more likely to exhibit internalizing responses, although evidence on gender differences is mixed (Lansford 2009). The stronger effects for girls in Swedish may therefore reflect improvements in socio-emotional stability or parental support that are especially relevant for language-intensive outcomes, while the shared gains in math and school quality suggest that both boys and girls benefit from improved parental involvement and educational environments.

For school quality, however, we find little evidence of gender heterogeneity. Panel B of Appendix Table A7 shows that both boys and girls attend higher-quality schools in grade

3 following a joint custody ruling, with estimates that are similar in magnitude across genders. These findings suggest that while the educational achievement effects of joint custody differ somewhat between boys and girls, improvements in school environments are broadly shared across children.

As a robustness check, Appendix Table A8 replicate the main estimates for test scores and school quality using the full sample of custody cases, including those decided prior to 2005 for which mental health data are unavailable. The results are qualitatively similar to those obtained in the narrower estimation sample. Joint custody continues to increase children’s academic performance and improve the quality of schools attended, although some estimates are somewhat smaller in magnitude.

Jointly, the test score and school quality results indicate that joint custody improves both academic performance and access to higher-quality schools.

Mental Health. We now estimate effect on children’s mental health. Table 9 reports the effects of joint custody on the probability of using antidepressant, antianxiety, and ADHD medication in the years following the custody ruling, as well as on continuous use over multiple years.

In the year-by-year estimates in Panel A, we find little evidence that joint custody affects children’s mental health. The estimated effects on antidepressant and antianxiety medication are generally close to zero and statistically insignificant in all years following the custody ruling, although relatively imprecise. For ADHD medication, the estimates are also small and not statistically significant in any year.

Panel B considers continuous medication use, which captures more persistent conditions requiring pharmacological treatment. Across all three measures, the estimated effects remain small and statistically insignificant. Overall, these results provide no evidence that joint custody affects the incidence of clinically treated mental health conditions among children. Because pharmacological treatment for these conditions is relatively uncommon among children, these outcomes primarily capture more severe mental health problems. Consequently, our findings do not rule out effects on milder aspects of children’s psychological well-being that do not lead to medication use.

Interpretation. The combined results reveal that joint custody leads to substantial improvements in children’s educational outcomes without detectable effects on mental

health. One interpretation is that joint custody improves children’s outcomes by preserving the involvement of both parents. The positive effects for fathers—both in terms of earnings and mental health—likely translate into greater parental investment, improved household resources, and more stable family environments. At the same time, the absence of adverse effects on children’s mental health suggests that potential concerns about instability from moving between households do not translate into measurable harm along these dimensions.

5 Comparison to OLS, Robustness, and Exclusion

This section compares the IV estimates to OLS, explores robustness, and presents tests for the exclusion restriction. For conciseness, the tables referred to in this section focus on a core set of outcomes and time periods. Specifically, for fathers and mothers we look at outcomes three years after the court ruling, focusing on the outcomes of earning above the 25th percentile, antidepressant drug use, and anti-anxiety drug use. For children, we focus on test scores, school value added, and ADHD drug use.

5.1 Comparison to OLS

In Table A9, we present OLS estimates for fathers, mothers, and children. OLS often yields different estimates compared to our IV estimates. For example, the effects of joint custody on father’s earnings are roughly half as large for OLS versus IV, and there is no effect on fathers’ antidepressant use or childrens’ test scores or school value added for OLS, but large and statistically significant effects in the IV specifications.

There are two possible explanations for why the OLS and IV estimates diverge: (i) heterogeneous effects, with compliers being impacted differently compared to never-takers and always takers or (ii) endogeneity in which families are granted joint custody. To explore the first possibility, we compare how the estimated characteristics of compliers differ from the entire sample of parents contesting custody (Table A10).¹⁹ If anything, compliers are positively selected in terms of higher earnings, higher education, and lower prior criminal activity, but the differences are small. Given this, it is not surprising

¹⁹Complier characteristics are estimated using the same procedure as in Amaral et al. (2023, Appendix B). The method combines estimates of compliance-type shares with observed sample moments to recover characteristics of compliers under the standard LATE framework.

that when we reweight the OLS regressions to have similar observable characteristics as compliers, this has little impact on the estimates. Hence, there are either unobservable differences in complier characteristics or there is selection into which couples are granted joint versus sole custody.

5.2 Robustness

Defining Treatment as Sole Maternal Custody. As robustness, we first explore two alternative treatment definitions. Our baseline treatment is joint custody, where the counterfactual is sole custody by either parent (usually the mother). As an alternative, we instead define treatment as sole maternal custody with a counterfactual of joint custody or sole paternal custody.²⁰

Using this alternative treatment definition yields qualitatively similar estimates for all of our main outcomes, but with the expected opposite sign compared to baseline.²¹ For example, the first column in Appendix Table A11 shows that maternal sole custody reduces the probability a father’s earnings are greater than the 25th percentile three years after the court ruling by 32 percentage points. For comparison, our baseline treatment of joint custody yields an increase of 26 percentage points (see Table 3). While not shown, the pattern of estimates and their statistical significance is similar to baseline for the other earnings quartiles and time periods. Columns 2 and 3 report effects for father’s continuous use of antidepressant and antianxiety drugs 3 years after the ruling. The estimate for antidepressants is 0.197, which compares to -0.13 using our baseline treatment definition. While not shown, this similarity in absolute values is representative of what we find for other time periods and for year-by-year drug use.

For mothers, we find no statistically significant effects on earnings or prescription drug use. This is true both for the estimate reported in the table, as well as for the other measures and time periods corresponding to our baseline estimates in Tables 5 and 6. These patterns are again consistent with our baseline estimates using joint custody as the treatment. Finally, for children, sole maternal custody decreases academic achievement,

²⁰As explained in Section 2.2, we do not have sufficient variation to estimate a model with separate endogenous indicators for joint custody and sole maternal custody in a multi-treatment IV framework, but instead need to focus on a single treatment margin.

²¹Using this alternative definition of treatment to construct the judge propensity instrument yields similar findings as Table 1, which tests for random assignment of cases to judges. While family characteristics strongly predict maternal sole custody, they do not joint predict judge propensities (F-statistic=0.864, p-value=0.697).

lowers the quality of school a child attends in grade 3, and has no impact on ADHD medication. These are analogous to what we found when using joint custody as the treatment, but with opposite-signed effects.

The fact that both treatment definitions reach similar conclusions suggests that the primary margin picked up in either set of estimates is joint versus sole maternal custody. In other words, the effects we estimate appear to be driven by whether fathers retain significant involvement in the day-to-day lives of their children.

Focusing on Legal Custody Cases Only. As an alternative robustness check, we use a sharper definition of joint custody. As a reminder, our baseline defines joint custody as either (i) joint legal custody when legal custody is contested, or (ii) shared residence when parents have joint legal custody but dispute the child’s residence. This definition captures cases in which both parents have substantial involvement in the child’s life, either through joint decision-making authority or through significant time spent with the child through joint residence. To probe robustness, we limit the sample to cases where legal custody is contested. Relative to the baseline definition, this approach isolates a sharper margin of parental involvement following divorce, but comes at the cost of a smaller sample. Estimates using this narrower definition are similar in magnitude to our baseline results, although less precisely estimated (see Appendix Table A12).

Varying the Judge Case Restriction. As another robustness check, we vary the minimum number of cases a judge is required to have handled to be included in our estimation sample. Our baseline restriction is that a judge handles 50 or more cases. If we lower this to 45 cases or raise it to 55 cases, the results are qualitatively similar (see Appendix Table A13). While it would be interesting to consider an even wider range, this is not feasible given the data we have.²²

5.3 Evidence Corroborating the Exclusion Restriction

As a final exercise, we test the exclusion restriction that judges only affect outcomes through the custody ruling. In our setting, a violation of the exclusion restriction would

²²On the one hand, raising the restriction from 50 to 55 reduces the sample of cases by as much as 24% for some outcomes; raising the restriction higher would cut the sample size even further. On the other hand, lowering the restriction too far below 50 runs the risk of creating a mechanical bias since we use a leave-out mean to construct the instrument.

occur if judges with higher joint custody rates take other actions which help parents and children reach better outcomes. As discussed in Section 2, judges can take two main procedural actions that shape the course of the case. First, judges can refer parents to cooperation talks with municipal family services ("mediation"), which happens in 16% of cases. Second, judges can ask the municipality to conduct a formal investigation by a social worker into the child's well-being and living situation ("investigation"), which happens in 40% of cases. To assess the validity of the exclusion restriction, we conduct two complimentary tests based on these additional judicial actions.

To conduct our tests, we first calculate each judge's mediation and investigation propensities, following the same approach as we did for joint custody. There is significant independent variation in the three propensities; a judge's mediation and investigation propensities have correlations of 0.1 and 0.01, respectively, with a judge's joint custody propensity.

Our first test adds indicator variables for whether a judge ordered mediation or an investigation. Since these actions is likely to be endogenous, we instrument for them using the judge's mediation and investigation propensities. In other words, we run a regression with three potentially endogenous regressors—joint custody, mediation, and investigation—and instrument with the three corresponding propensities. These estimates are found in the even-numbered columns of Appendix Table A14, and are remarkably similar to our baseline estimates in Tables 3-9.²³ As a second test, we instead include the additional judge propensities, not as instruments, but as additional control variables in our baseline IV regressions. These estimates are shown in the odd-numbered columns in Appendix Table A14. Including these additional propensities also has little impact on the joint custody estimates. Based on these findings, we conclude that the exclusion restriction is likely to hold.

6 Other Family Dynamics

The results in Section 4 show that joint custody substantially reshapes the lives of fathers and children, with large effects on fathers' labor market attachment, mental health, and

²³The effects of judge-ordered mediation and investigation are interesting in their own right. There are hints that mediation has a positive impact, and investigations a negative impact, on family outcomes but the estimates are too noisy to draw firm conclusions.

children’s educational outcomes. These findings naturally raise a broader question: how does joint custody alter the organization of family life after divorce?

In this section, we examine several additional dimensions of post-separation family dynamics. We begin by studying whether joint custody affects the geographic proximity between former partners, reflecting the practical constraints and coordination demands of shared parenting. We then examine whether joint custody influences parents’ ability to form new partnerships through remarriage. Finally, we consider whether continued interaction between former partners under joint custody affects the risk of intimate partner violence. Together, these outcomes shed light on whether joint custody facilitates or hinders families’ adjustment to life after separation.

Geography of the Family. Joint custody could affect where parents live after separation, as maintaining shared caregiving can require geographic proximity. Table 10 reports the effect of joint custody on parents’ residential proximity, measured by an indicator for whether they live in separate municipalities. There are 290 municipalities in Sweden. Joint custody reduces the probability that parents live in separate municipalities by 31 percentage points in the first year after the ruling and 21 percentage points in the second year. The point estimates remain negative in years 3–5, ranging from 13 to 14 percentage points, although they are estimated less precisely. Compared to the control complier means, the estimates imply reductions of between 29% and 51%.

Panels B and C of Table 10 examine these residential patterns separately for fathers and mothers. The estimates suggest that both parents are less likely to move to a different municipality following a joint custody ruling, consistent with the view that shared custody arrangements increase the value of geographic proximity after separation. For mothers, the effects are particularly pronounced in the first two years after the ruling, while for fathers the largest effects occur immediately after the custody decision. However, the estimates are generally imprecisely estimated, with statistical significance limited to a subset of years.

Overall, the results suggest that joint custody tends to keep separated parents geographically closer, although the persistence and precise magnitude of these effects are less precisely estimated. This is broadly consistent with earlier work by Adams et al. (2025), showing that the introduction of joint custody laws in the US led to reduced mobility among fathers. This closer proximity could be one reason for the positive effects for chil-

dren, since it allows them to maintain stable school and peer environments and reduces the need for long-distance travel between parents. Interestingly, remaining geographically close does not appear to result in an earnings reduction for either parent, even though some job opportunities presumably require moving.

Parental Relationships. Joint custody may affect parents' ability to form new partnerships after separation. On the one hand, the continued interaction between former partners required under joint custody may make it more difficult to establish new relationships. On the other hand, the improvements in fathers' labor market outcomes and mental health documented in Section 4 may facilitate the formation of new partnerships. For mothers, shared caregiving responsibilities may relax time constraints and thereby ease repartnering.

Table 11 presents the effects of joint custody on the probability that parents remarry in the years following the custody ruling. For fathers (panel A), the estimates are positive throughout all five years after the ruling and statistically significant from year 2 onwards. The magnitude of the effects is substantial: joint custody increases the probability of remarriage by 14 to 22 percentage points. An important feature of these results is that baseline remarriage rates among control compliers are very low. As a result, joint custody shifts treated fathers toward remarriage rates that are close to those observed in the overall estimation sample.

In contrast, the estimates for mothers (panel B) are substantially smaller than those for fathers and generally imprecisely estimated. While some point estimates are positive, the results provide little evidence that joint custody systematically affects mothers' remarriage probabilities.

To better understand the mechanisms underlying the remarriage effects for fathers, we next examine whether these increases are linked to changes in earnings or mental health. Figure 3 decomposes the remarriage effects along earnings and mental health dimensions. Panel A splits the probability of remarriage into fathers who earn above the 25th percentile and those who do not, while Panel B splits remarriage by whether fathers use antidepressant medication in a given year. Panel A shows that while joint custody increases the probability of remarriage, these effects are not systematically concentrated among fathers with higher earnings until years 4 and 5. Turning to panel B, the remarriage effects are concentrated among fathers who do not use antidepressants in all years. In other

words, fathers who remarry following a joint custody ruling are also those who experience improvements in mental health and, in later years, increases in earnings. These patterns suggest that the relationship between joint custody and remarriage operates primarily through improvements in psychological well-being and partly through changes in labor market outcomes.

A corresponding decomposition for mothers reveals no clear patterns linking remarriage to either earnings or mental health outcomes (Appendix Figure A2). This is unsurprising given the earlier findings that joint custody does not systematically affect mothers' earnings, mental health, or remarriage probabilities.

Intimate Partner Violence. A central concern in the debate over joint custody is whether it exacerbates conflict by forcing continued interaction between former partners. In high-conflict separations, it could potentially lead to an increase in intimate partner violence. Alternatively, by stabilizing the post-divorce economic and emotional environment—as suggested by the improvements in fathers' outcomes—joint custody might mitigate the stressors that often trigger such conflict.

To study this, in Table 12 we use information from Swedish crime registers which record whether an individual is the suspect of crime against a former or current partner.²⁴ We find no indication that either fathers or mothers are more likely to be a suspect of intimate partner violence if joint custody is awarded, despite the fact that joint custody increases the frequency of interactions between parents. This null finding, albeit imprecisely estimated, is consistent with fathers' improved economic and mental health. It is also consistent with joint custody reducing conflict by creating a strong behavioral incentive for parents to maintain a cooperative relationship to preserve their custodial rights. An important caveat is that we are estimating effects for compliers, where judges differ in the assignment of joint custody. The impact of joint custody for never-taker cases—such as abusive partnerships where no judge would assign joint custody—could well have dramatically different effects.

Taken together, the findings in this section suggest that joint custody reshapes family life not only through economic and psychological channels, but also by altering parents' spatial proximity and opportunities to form new partnerships.

²⁴Information on the identity of the victim is not directly recorded in the registers.

7 Conclusion

Custody arrangements play a central role in shaping family life after divorce or separation, yet there has been little evidence on their causal effects. We address this gap by constructing a new dataset linking the near universe of Swedish custody rulings spanning multiple decades to rich administrative records on parents and children. Exploiting the quasi-random assignment of custody disputes to judges who differ in their propensity to award joint custody, we estimate the causal effect of joint versus sole custody on a broad set of economic, educational, and mental health outcomes.

Our findings show that joint custody has substantial consequences for post-divorce family life. For fathers, joint custody leads to large and persistent improvements in both labor market attachment and mental health. Fathers awarded joint custody experience higher earnings and are substantially less likely to use antidepressant medication following the custody ruling. These gains are concentrated among fathers with relatively weak pre-existing labor market attachment, suggesting that maintaining an active parental role after separation may promote both economic stability and psychological well-being.

For mothers, in contrast, we find little evidence that joint custody affects either earnings or mental health. This asymmetry likely reflects different counterfactuals. For fathers, sole custody often implies losing day-to-day involvement with their children, whereas mothers typically retain primary caregiving responsibilities regardless of the legal arrangement. Joint custody therefore appears to alter fathers' parental involvement much more fundamentally than mothers'.

Turning to children, we find that joint custody improves educational outcomes without detectable adverse effects on mental health. Children assigned to joint custody perform substantially better on standardized tests and are more likely to attend higher-quality primary schools. At the same time, we find no evidence that joint custody increases the use of antidepressant, antianxiety, or ADHD medication among children.

Beyond earnings, education, and mental health, our findings suggest that joint custody reshapes broader patterns of post-divorce family life. Parents assigned joint custody are more likely to remain geographically close to one another, consistent with continued parental involvement requiring coordination and proximity. We also find evidence that joint custody facilitates entry into new marriages for fathers. Importantly, despite concerns that continued interaction between former partners may prolong conflict, we find

no evidence that joint custody increases intimate partner violence. Taken together, these findings suggest that joint custody may help stabilize family relationships after separation rather than exacerbate conflict.

More broadly, our findings speak to longstanding debates over the role of joint custody in post-divorce family life. Discussions of custody policy often emphasize potential tradeoffs across family members, where arrangements that benefit one parent may come at the expense of the other parent or the child. Our results provide little evidence of such tradeoffs for the marginal cases affected by judicial discretion. Joint custody substantially improves fathers' economic and psychological well-being while also improving children's educational outcomes, and we do not detect offsetting adverse effects on mothers' outcomes, children's mental health, or family conflict.

Of course, our findings do not imply that joint custody should be granted in all cases. Our estimates do not apply to families in which all judges would agree that joint custody is inappropriate, such as cases involving severe conflict or abuse. Rather, the evidence supports a more limited but policy-relevant conclusion: a modest expansion in the use of joint custody among disputed, marginal cases is likely to improve family welfare.

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Tables and Figures

Table 1: Testing for Random Assignment of Custody Cases to Judges

	Joint custody		Judge Propensity	
	(1)	(2)	(3)	(4)
Plaintiff is female	-0.0864***	(0.0112)	-0.0044**	(0.0021)
Child is a girl	0.0031	(0.0073)	0.0012	(0.0014)
Child's age	-0.0081***	(0.0012)	-0.0006**	(0.0002)
Father's age	-0.0002	(0.0009)	-0.0002	(0.0002)
Mother's age	-0.0026**	(0.0010)	0.0003	(0.0002)
Child foreign born	0.0247	(0.0185)	0.0052	(0.0037)
Father foreign born	-0.0744***	(0.0127)	0.0001	(0.0025)
Mother foreign born	0.0055	(0.0127)	0.0009	(0.0026)
Parents cohabiting	-0.0137	(0.0115)	-0.0022	(0.0023)
Number of children in case	-0.0061	(0.0062)	0.0020	(0.0012)
Father positive earnings	-0.2490***	(0.0522)	0.0130	(0.0104)
Father log earnings	0.0350***	(0.0051)	-0.0012	(0.0010)
Mother positive earnings	-0.0724	(0.0505)	-0.0094	(0.0103)
Mother log earnings	0.0102**	(0.0051)	0.0009	(0.0010)
Father education level: Compulsory	-0.0185	(0.0332)	0.0057	(0.0076)
Father education level: High School	0.0455	(0.0331)	0.0058	(0.0076)
Father education level: College	0.0754**	(0.0337)	0.0075	(0.0076)
Mother education level: Compulsory	-0.0076	(0.0349)	0.0066	(0.0064)
Mother education level: High School	0.0048	(0.0347)	0.0083	(0.0064)
Mother education level: College	0.0410	(0.0352)	0.0086	(0.0065)
Child mental health medication	-0.0483*	(0.0265)	0.0014	(0.0051)
Father mental health medication	-0.0386***	(0.0134)	0.0005	(0.0026)
Mother mental health medication	-0.0048	(0.0120)	0.0018	(0.0024)
Father ever convicted	-0.0749***	(0.0122)	0.0043*	(0.0025)
Mother ever convicted	-0.0748***	(0.0221)	0.0044	(0.0041)
Joint F	19.211		0.969	
Prob > F	0.000		0.520	
Court×Year FE	Yes		Yes	

Notes: This table shows estimates from regressing children's and parents' characteristics on the case ruling and judge propensity. The sample consists of 17,007 children, 10,899 fathers and 10,902 mothers. Background variables are defined two years before the custody ruling. The omitted category for education level for mothers and fathers is education level missing. Reported F -statistic refers to joint tests of the null hypothesis for all variables. All estimates control for court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** $p < .01$, ** $p < .05$, * $p < .1$.

Table 2: First Stage of Joint Custody on Judge Propensity

	Joint Custody	
	(1)	(2)
Judge propensity	0.557*** (0.042)	0.557*** (0.041)
Controls	No	Yes
F-stat	178	188
Observations	17,007	17,007

Notes: Column (2) includes controls for the variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** $p < .01$, ** $p < .05$, * $p < .1$.

Table 3: Joint Custody and Fathers' Earnings

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
<i>Panel A: Earning above the 25th percentile (age-adjusted)</i>					
Joint custody	0.169** (0.079)	0.272*** (0.080)	0.254*** (0.084)	0.231*** (0.082)	0.182** (0.087)
Control complier mean	0.546 (0.067)	0.456 (0.068)	0.451 (0.069)	0.482 (0.068)	0.437 (0.073)
Dep. Mean	0.540	0.537	0.535	0.546	0.544
Observations	16,726	16,184	14,830	13,422	12,024
<i>Panel B: Earning above the 50th percentile (age-adjusted)</i>					
Joint custody	0.161** (0.074)	0.166** (0.075)	0.130* (0.077)	0.047 (0.077)	0.067 (0.082)
Control complier mean	0.241 (0.058)	0.214 (0.059)	0.288 (0.061)	0.366 (0.060)	0.334 (0.065)
Dep. Mean	0.285	0.286	0.287	0.292	0.296
Observations	16,726	16,184	14,830	13,422	12,024
<i>Panel C: Earning above the 75th percentile (age-adjusted)</i>					
Joint custody	0.088 (0.056)	0.052 (0.056)	0.025 (0.058)	0.056 (0.057)	0.022 (0.061)
Control complier mean	0.096 (0.040)	0.095 (0.041)	0.144 (0.042)	0.186 (0.042)	0.187 (0.046)
Dep. Mean	0.121	0.120	0.119	0.122	0.124
Observations	16,726	16,184	14,830	13,422	12,024

Notes: This table shows IV estimates of joint custody on the probability of the fathers' individual earnings being above the stated quartile of the same gender and age reference group of the full Swedish population in year t after the custody ruling. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level.*** $p < .01$, ** $p < .05$, * $p < .1$.

Table 4: Joint Custody and Fathers' Mental Health

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
<i>Panel A: Year-by-year medication use</i>					
Antidepressants					
Joint custody	-0.125** (0.056)	-0.161*** (0.056)	-0.169*** (0.058)	-0.116** (0.059)	0.043 (0.059)
Control complier mean	0.262 (0.047)	0.267 (0.047)	0.247 (0.047)	0.250 (0.049)	0.137 (0.047)
Dep. Mean	0.122	0.116	0.117	0.117	0.118
Observations	16,726	16,184	14,830	13,422	12,024
Antianxiety					
Joint custody	-0.038 (0.058)	-0.039 (0.057)	-0.005 (0.058)	-0.006 (0.062)	0.018 (0.063)
Control complier mean	0.167 (0.048)	0.205 (0.047)	0.149 (0.048)	0.189 (0.051)	0.151 (0.052)
Dep. Mean	0.143	0.136	0.133	0.138	0.137
Observations	16,726	16,184	14,830	13,422	12,024
<i>Panel B: Continuous medication use</i>					
Antidepressants					
Joint custody	-0.125** (0.056)	-0.156*** (0.047)	-0.127*** (0.043)	-0.103*** (0.038)	-0.075** (0.035)
Control complier mean	0.262 (0.047)	0.218 (0.040)	0.170 (0.036)	0.147 (0.032)	0.118 (0.029)
Dep. Mean	0.122	0.076	0.057	0.045	0.037
Observations	16,726	16,184	14,830	13,205	12,024
Antianxiety					
Joint custody	-0.038 (0.058)	-0.046 (0.045)	-0.066 (0.040)	-0.073* (0.038)	-0.094** (0.038)
Control complier mean	0.167 (0.048)	0.129 (0.038)	0.099 (0.034)	0.099 (0.033)	0.106 (0.032)
Dep. Mean	0.143	0.085	0.062	0.052	0.042
Observations	16,726	16,184	14,830	13,174	12,024

Notes: The Table shows IV estimates of joint custody on the probability of fathers having a prescription in year t after the custody ruling. Continuous estimates show the probability of having a prescription in all years 1- t after the ruling. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** $p < .01$, ** $p < .05$, * $p < .1$.

Table 5: Joint Custody and Mothers' Earnings

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
<i>Panel A: Earning above the 25th percentile (age-adjusted)</i>					
Joint custody	-0.076 (0.081)	-0.154* (0.083)	-0.020 (0.082)	0.069 (0.081)	0.008 (0.087)
Control complier mean	0.596 (0.067)	0.681 (0.068)	0.616 (0.068)	0.527 (0.067)	0.574 (0.072)
Dep. Mean	0.603	0.610	0.618	0.621	0.632
Observations	16,861	16,404	15,076	13,728	12,374
<i>Panel B: Earning above the 50th percentile (age-adjusted)</i>					
Joint custody	-0.066 (0.079)	-0.134* (0.080)	-0.019 (0.080)	0.056 (0.080)	0.013 (0.086)
Control complier mean	0.375 (0.065)	0.443 (0.067)	0.335 (0.066)	0.300 (0.065)	0.303 (0.070)
Dep. Mean	0.322	0.328	0.333	0.342	0.353
Observations	16,861	16,404	15,076	13,728	12,374
<i>Panel C: Earning above the 75th percentile (age-adjusted)</i>					
Joint custody	-0.061 (0.061)	-0.040 (0.063)	0.096 (0.063)	0.020 (0.062)	-0.003 (0.067)
Control complier mean	0.224 (0.050)	0.209 (0.051)	0.098 (0.050)	0.138 (0.050)	0.174 (0.055)
Dep. Mean	0.132	0.136	0.135	0.145	0.150
Observations	16,861	16,404	15,076	13,728	12,374

Notes: This table shows IV estimates of joint custody on the probability of the mothers' individual earnings being above the stated quartile of the same gender and age reference group of the full Swedish population in year t after the custody ruling. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level.*** $p < .01$, ** $p < .05$, * $p < .1$.

Table 6: Joint Custody and Mothers' Mental Health

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
<i>Panel A: Year-by-year medication use</i>					
Antidepressants					
Joint custody	0.021 (0.069)	-0.022 (0.071)	-0.033 (0.071)	-0.022 (0.070)	-0.089 (0.073)
Control complier mean	0.191 (0.056)	0.209 (0.057)	0.236 (0.057)	0.255 (0.057)	0.254 (0.059)
Dep. Mean	0.199	0.199	0.198	0.203	0.201
Observations	16,861	16,404	15,076	13,728	12,374
Antianxiety					
Joint custody	-0.069 (0.072)	-0.070 (0.072)	-0.038 (0.072)	-0.082 (0.071)	-0.058 (0.076)
Control complier mean	0.233 (0.058)	0.274 (0.058)	0.254 (0.058)	0.281 (0.058)	0.287 (0.061)
Dep. Mean	0.201	0.199	0.194	0.197	0.201
Observations	16,861	16,404	15,076	13,728	12,374
<i>Panel B: Continuous medication use</i>					
Antidepressants					
Joint custody	0.021 (0.069)	-0.008 (0.060)	0.000 (0.052)	-0.026 (0.048)	-0.052 (0.046)
Control complier mean	0.191 (0.056)	0.157 (0.048)	0.105 (0.041)	0.099 (0.038)	0.106 (0.036)
Dep. Mean	0.199	0.130	0.100	0.085	0.068
Observations	16,861	16,404	15,076	13,309	12,374
Antianxiety					
Joint custody	-0.069 (0.072)	-0.033 (0.059)	0.006 (0.052)	-0.020 (0.048)	-0.029 (0.048)
Control complier mean	0.233 (0.058)	0.144 (0.048)	0.080 (0.042)	0.084 (0.039)	0.094 (0.039)
Dep. Mean	0.201	0.121	0.092	0.077	0.066
Observations	16,861	16,404	15,076	13,360	12,374

Notes: The Table shows IV estimates of joint custody on the probability of mothers having a prescription in year t after the custody ruling. Continuous estimates show the probability of having a prescription in all years 1- t after the ruling. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** $p < .01$, ** $p < .05$, * $p < .1$.

Table 7: Joint Custody and Children's Test Scores

	(1) First Test After Ruling Average Score	(2) First Test After Ruling Swedish Score	(3) First Test After Ruling Math Score
Joint custody	0.466** (0.229)	0.360 (0.230)	0.477** (0.228)
Control complier mean	-0.589 (0.182)	-0.473 (0.181)	-0.609 (0.182)
Dep. Mean	-0.301	-0.256	-0.313
Observations	10,814	10,814	10,814

Notes: The table presents IV estimates of joint custody on children's national test scores at any point after the ruling. Average score refers to average of Swedish and Math scores. Only children who have both scores are included in the estimation. Mean of the Dependent Variable shows the sample mean of children included in the estimation sample, test scores were standardized (mean 0, SD 1) on the full population of test takers. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

Table 8: Joint Custody and School Quality

	(1) Residual School Quality Grade 3	(2) Residual School Quality Grade 6	(3) Residual School Quality Grade 9
Joint custody	0.232*** (0.088)	0.093 (0.078)	-0.028 (0.064)
Control complier mean	-0.132 (0.069)	-0.133 (0.060)	0.066 (0.052)
Dep. Mean	-0.016	-0.019	0.002
Observations	5,261	4,882	4,324

Notes: The table shows IV estimates of joint custody on the quality of schools that children in the court data attend after ruling. The school quality measure is constructed by regressing each student's average math and Swedish score on the gender and age of the child, the migration status of the child and both parents, parents' highest completed education, and parents' earnings, separately by grade and year of the test. For this measure we include the full population of students in Sweden from whom we have math and Swedish scores. We take the residuals of this regression and calculate the leave-out residual. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

Table 9: Joint Custody and Children's Mental Health

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
<i>Panel A: Year-by-year medication use</i>					
Antidepressants					
Joint custody	-0.012 (0.014)	0.008 (0.016)	-0.001 (0.020)	0.017 (0.023)	0.026 (0.026)
Control complier mean	0.017 (0.011)	-0.003 (0.014)	0.010 (0.016)	0.016 (0.019)	0.033 (0.022)
Dep. Mean Observations	0.008 17,005	0.014 16,740	0.018 15,623	0.023 14,363	0.030 13,028
Antianxiety					
Joint custody	0.001 (0.027)	0.032 (0.029)	-0.005 (0.035)	0.036 (0.037)	0.053 (0.041)
Control complier mean	0.025 (0.023)	0.004 (0.024)	0.059 (0.030)	0.031 (0.031)	0.050 (0.033)
Dep. Mean Observations	0.034 17,005	0.040 16,740	0.055 15,623	0.061 14,363	0.070 13,028
ADHD					
Joint custody	0.024 (0.026)	0.027 (0.030)	0.029 (0.034)	-0.005 (0.037)	-0.001 (0.041)
Control complier mean	0.004 (0.023)	-0.001 (0.026)	0.010 (0.030)	0.024 (0.032)	0.039 (0.034)
Dep. Mean Observations	0.033 17,005	0.042 16,740	0.052 15,623	0.062 14,363	0.072 13,028
<i>Panel B: Continuous medication use</i>					
Antidepressants					
Joint custody	-0.012 (0.014)	0.000 (0.011)	-0.002 (0.010)	-0.002 (0.008)	0.000 (0.008)
Control complier mean	0.017 (0.011)	0.007 (0.009)	0.005 (0.008)	0.003 (0.007)	0.003 (0.008)
Dep. Mean Observations	0.008 17,005	0.006 16,581	0.004 15,271	0.003 13,906	0.002 12,586
Antianxiety					
Joint custody	0.001 (0.027)	0.025 (0.021)	0.022 (0.018)	0.016 (0.015)	0.015 (0.015)
Control complier mean	0.025 (0.023)	-0.011 (0.018)	-0.006 (0.015)	-0.003 (0.014)	-0.002 (0.014)
Dep. Mean Observations	0.034 17,005	0.021 16,581	0.014 15,271	0.010 13,881	0.007 12,586
ADHD					
Joint custody	0.024 (0.026)	0.032 (0.025)	0.016 (0.023)	0.005 (0.020)	0.016 (0.021)
Control complier mean	0.004 (0.023)	-0.011 (0.022)	0.000 (0.021)	0.007 (0.019)	-0.002 (0.020)
Dep. Mean Observations	0.033 17,005	0.028 16,581	0.023 15,271	0.019 13,865	0.016 12,586

Table 10: Joint custody and the Geography of the Family

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
<i>Panel A: Parents living in different municipalities</i>					
Joint custody	-0.313*** (0.094)	-0.205** (0.092)	-0.144 (0.090)	-0.142 (0.096)	-0.125 (0.101)
Control complier mean	0.721 (0.075)	0.636 (0.073)	0.589 (0.072)	0.568 (0.076)	0.555 (0.080)
Dep. Mean	0.459	0.472	0.481	0.484	0.492
Observations	16,542	15,217	13,872	12,532	11,165
<i>Panel B: Fathers moving municipalities</i>					
Joint custody	-0.139* (0.079)	-0.098 (0.079)	-0.138* (0.081)	-0.112 (0.080)	-0.048 (0.087)
Control complier mean	0.379 (0.066)	0.331 (0.065)	0.346 (0.066)	0.309 (0.066)	0.310 (0.070)
Dep. Mean	0.239	0.241	0.253	0.261	0.264
Observations	15,966	15,966	14,715	13,421	12,130
<i>Panel C: Mothers moving municipalities</i>					
Joint custody	-0.254*** (0.079)	-0.139* (0.079)	-0.122 (0.080)	-0.109 (0.079)	-0.140 (0.086)
Control complier mean	0.416 (0.065)	0.340 (0.065)	0.358 (0.066)	0.340 (0.065)	0.361 (0.071)
Dep. Mean	0.231	0.237	0.249	0.262	0.270
Observations	16,268	16,268	14,984	13,659	12,356

Notes: The Table shows IV estimates of joint custody on the probability of moving municipalities. The outcome in Panel A is an indicator of whether parents live in different municipalities. The outcomes in Panels B and C are indicators for living in a different municipality than before the separation for fathers and mothers respectively. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

Table 11: Joint Custody and Remarriage

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
<i>Panel A: Fathers</i>					
Joint custody	0.088 (0.069)	0.148** (0.074)	0.135* (0.079)	0.148* (0.080)	0.219** (0.094)
Control complier mean	0.084 (0.054)	0.093 (0.058)	0.106 (0.063)	0.126 (0.064)	0.097 (0.075)
Dep. Mean	0.163	0.202	0.232	0.265	0.293
Observations	16,242	15,694	14,346	12,991	11,648
<i>Panel B: Mothers</i>					
Joint custody	0.006 (0.064)	0.053 (0.068)	0.049 (0.073)	-0.001 (0.076)	-0.021 (0.085)
Control complier mean	0.159 (0.052)	0.138 (0.054)	0.182 (0.059)	0.238 (0.061)	0.310 (0.069)
Dep. Mean	0.123	0.153	0.185	0.217	0.244
Observations	16,629	16,161	14,832	13,486	12,174

Notes: The Table shows IV estimates of joint custody on the probability that parents remarried within 1-5 years. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

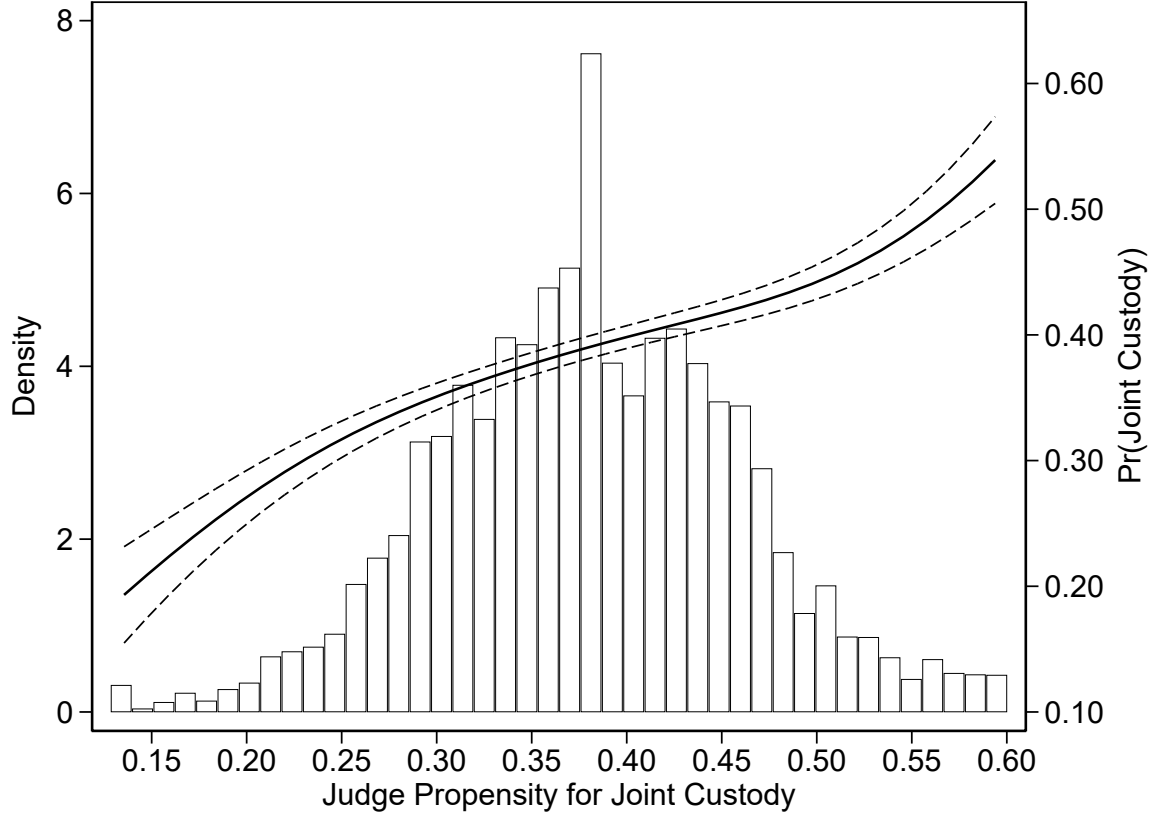
Table 12: Joint Custody and Intimate Partner Violence

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
<i>Panel A: Fathers</i>					
Joint custody	-0.031 (0.032)	-0.007 (0.040)	-0.013 (0.045)	0.009 (0.048)	0.026 (0.054)
Control complier mean	0.044 (0.027)	0.055 (0.033)	0.085 (0.037)	0.112 (0.040)	0.098 (0.045)
Dep. Mean	0.031	0.053	0.065	0.076	0.087
Observations	17,003	16,581	15,271	13,924	12,586
<i>Panel B: Mothers</i>					
Joint custody	0.003 (0.014)	-0.000 (0.020)	0.016 (0.025)	0.041 (0.028)	0.055* (0.032)
Control complier mean	0.004 (0.010)	0.017 (0.015)	0.009 (0.019)	-0.006 (0.022)	-0.018 (0.027)
Dep. Mean	0.006	0.011	0.017	0.021	0.022
Observations	17,003	16,581	15,271	13,924	12,586

Notes: The Table shows IV estimates of joint custody on the probability of being a suspect of a violent crime against a former or current partner. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

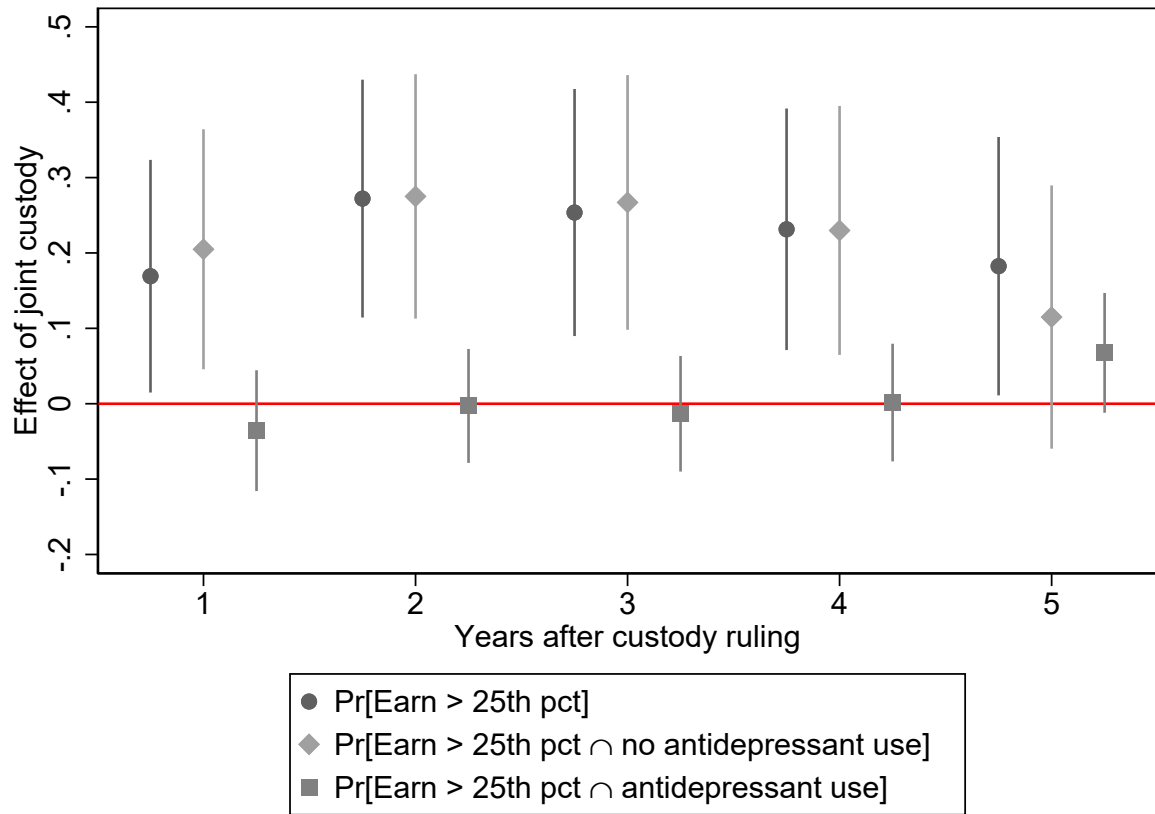
8 Figures

Figure 1: First-stage Graph of Joint Custody on Judge Propensity



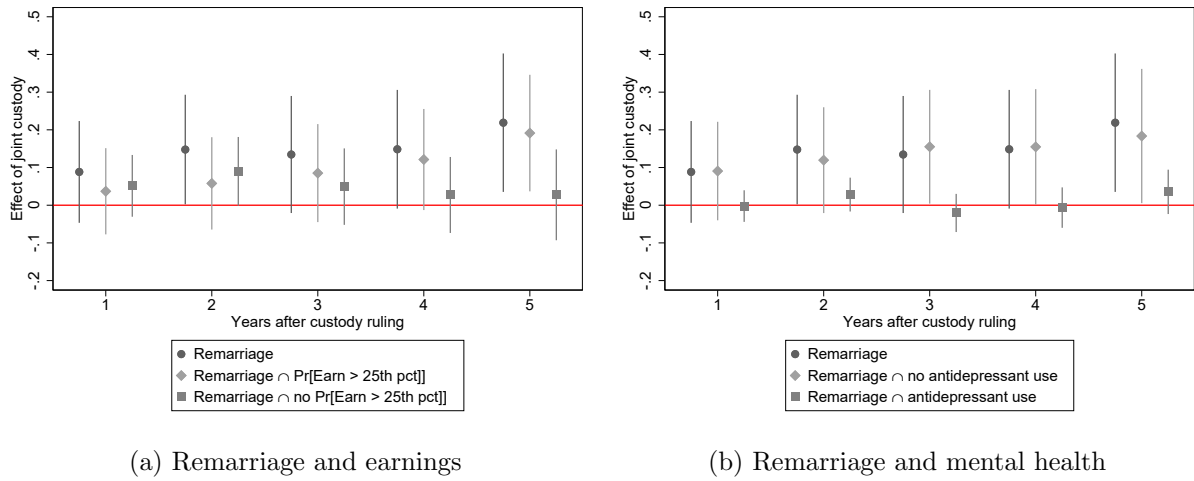
Notes: Baseline sample consisting of 11,027 custody cases occurring between 2005 and 2021. Probability of being assigned joint custody is plotted on the right y-axis against the leave-out mean judge propensity for joint custody on the x-axis. Plotted values are mean-standardized residuals from regressions on court-by-year fixed effects. The solid line shows a local linear first-stage regression of joint custody on judge propensity and court-by-year fixed effects. Dashed lines give the 95% confidence intervals. The histogram shows the density of judge propensity plotted on the left Y-axis. Top and bottom 1% of the distribution are excluded.

Figure 2: Decomposing the Effect of Joint Custody on Fathers' Earnings and Mental Health



Notes: The figure reports IV estimates of the effect of joint custody on fathers' earnings and joint earnings–mental health outcomes one to five years after the custody ruling. For each year, the first estimate shows the effect on earning above the 25th percentile of the earnings distribution. The remaining estimates show the effects on earning above the 25th percentile while not using antidepressants and while using antidepressants in the same year, respectively. The latter two outcomes partition the probability of earning above the 25th percentile. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are clustered at the case level. Error bars denote 95% confidence interval.

Figure 3: Decomposing the Effect of Joint Custody on Fathers Remarrying, Earnings and Mental Health



Notes: The figure reports IV estimates of the effect of joint custody on fathers' remarriage and decompositions of this outcome. In each panel, the first estimate shows the effect on remarriage. Panel A partitions remarriage according to whether fathers earn above or below the 25th percentile of the earnings distribution. Panel B partitions remarriage according to whether fathers do or do not use antidepressants in the same year. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are clustered at the case level. Error bars denote 95% confidence interval.

Online Appendix

“Life After Divorce: Effects of Joint Custody on Parents and
Children”

by Stella Canessa, Gordon B. Dahl, Anna Hasselqvist, Costas Meghir,
Susan Niknami, Mårten Palme, Helmut Rainer, Olof Rosenqvist, and
Pengpeng Xiao

A Additional Tables and Figures

Table A1: Summary Statistics

	(1)	(2)
	Observations	Mean
Plaintiff is female	17,007	0.683
Child is a girl	17,007	0.485
Child's age	16,916	7.931
Father's age	16,837	40.617
Mother's age	17,007	36.623
Child foreign born	16,957	0.084
Father foreign born	16,760	0.475
Mother foreign born	16,796	0.434
Parents cohabiting	16,339	0.348
Number of children in case	17,007	1.961
Father positive earnings	16,759	0.892
Father log earnings	16,759	8.950
Mother positive earnings	16,832	0.894
Mother log earnings	16,832	8.618
Father education level: Compulsory	16,544	0.257
Father education level: High School	16,544	0.476
Father education level: College	16,544	0.247
Mother education level: Compulsory	16,733	0.237
Mother education level: High School	16,733	0.450
Mother education level: College	16,733	0.289
Child mental health medication	16,900	0.022
Father mental health medication	16,909	0.166
Mother mental health medication	16,973	0.228
Father ever convicted	17,007	0.222
Mother ever convicted	17,007	0.048

Notes: The table reports summary statistics for the baseline estimation sample of 11,027 custody cases decided between 2005 and 2021. The sample comprises 17,007 children linked to 10,902 mothers and 10,899 fathers. Variables with fewer than 17,007 observations contain missing values in the administrative data. Throughout the regression analysis, missing values are handled by combining mean imputation with missing-value indicator variables.

Table A2: Test for Average Monotonicity: Main Instrument

	Case Includes			Youngest child		Children's Origin		Family Earnings	
	(1) Only Boys	(2) Only Girls	(3) Mixed Gender	(4) Age ≤6	(5) Age >6	(6) Migrant Origin	(7) Native Origin	(8) Above Median	(9) Below Median
First stage	0.463*** (0.074)	0.683*** (0.076)	0.635*** (0.067)	0.553*** (0.055)	0.593*** (0.061)	0.546*** (0.052)	0.603*** (0.068)	0.609*** (0.060)	0.532*** (0.056)
Dep. Mean	0.393	0.395	0.374	0.400	0.372	0.349	0.434	0.447	0.327
F-stat	39.5	81.2	91.0	100.1	93.2	112.3	78.6	104.6	89.5
Observations	5,559	5,091	6,283	9,102	7,866	9,497	7,465	8,501	8,468

	Parents' age gap			Parental Education				
	(10) Above Median	(11) Below Median	(12) Mom Low	(13) Mom Medium	(14) Mom High	(15) Dad Low	(16) Dad Medium	(17) Dad High
First stage	0.635*** (0.059)	0.528*** (0.057)	0.321*** (0.079)	0.702*** (0.063)	0.731*** (0.084)	0.545*** (0.085)	0.615*** (0.062)	0.551*** (0.078)
Dep. Mean	0.378	0.395	0.308	0.413	0.440	0.339	0.384	0.437
F-stat	115.0	84.9	16.5	124.9	75.1	41.6	97.9	49.6
Observations	8,101	8,867	4,197	7,867	4,044	3,913	7,514	4,808

Notes: This table shows first-stage estimates using the main instrument for different subsamples. Low, medium, and high education corresponds to compulsory, high school, and university education, respectively. All estimations include court-by-year fixed effects. Standard errors are reported in parentheses and clustered at the case level. *** $p < .01$, ** $p < .05$, * $p < .1$.

Table A3: Test for Average Monotonicity: Reverse-Sample Instrument

	Case Includes			Youngest child		Children's Origin		Family Earnings	
	(1) Only Boys	(2) Only Girls	(3) Mixed Gender	(4) Age ≤6	(5) Age >6	(6) Migrant Origin	(7) Native Origin	(8) Above Median	(9) Below Median
First stage	0.075 (0.063)	0.238 (0.069)	0.603 (0.061)	0.120 (0.044)	0.196 (0.050)	0.199 (0.038)	0.185 (0.057)	0.253 (0.052)	0.158 (0.043)
Dep. Mean	0.393	0.395	0.374	0.400	0.372	0.349	0.434	0.447	0.327
F-stat	1.4	12.0	98.9	7.6	15.4	27.4	10.4	23.6	13.4
Observations	5,559	5,091	6,283	9,102	7,866	9,497	7,465	8,501	8,468

	Parents' age gap		Parental Education					
	(10) Above Median	(11) Below Median	(12) Mom Low	(13) Mom Medium	(14) Mom High	(15) Dad Low	(16) Dad Medium	(17) Dad High
First stage	0.374 (0.055)	0.254 (0.044)	0.172 (0.068)	0.360 (0.058)	0.610 (0.084)	0.361 (0.076)	0.310 (0.056)	0.312 (0.069)
Dep. Mean	0.378	0.395	0.308	0.413	0.440	0.339	0.384	0.437
F-stat	46.2	32.6	6.3	38.7	53.3	22.4	31.0	20.6
Observations	8,101	8,867	4,197	7,867	4,044	3,913	7,514	4,808

Notes: This table shows first-stage estimates using the reverse-sample instrument for different subsamples. The reverse-sample instrument is defined as the judge's propensity to assign joint custody in all cases outside the given subsample. Low, medium, and high education corresponds to compulsory, high school, and university education, respectively. All estimations include court-by-year fixed effects. Standard errors are reported in parentheses and clustered at the case level. *** $p < .01$, ** $p < .05$, * $p < .1$.

Table A4: Joint Test for Strict Monotonicity and Exclusion

	5 Knots	10 Knots	15 Knots	20 Knots	25 Knots
<i>A. Fathers earning above 25th percentile (age-adjusted)</i>					
Test statistic	229.313 [0.023]	185.954 [0.652]	146.458 [1.000]	127.468 [1.000]	121.881 [1.000]
<i>B. Fathers continuous antidepressant medication use</i>					
Test statistic	230.110 [0.021]	175.039 [1.000]	149.755 [1.000]	108.679 [1.000]	102.271 [1.000]
<i>C. Fathers continuous anti-anxiety medication use</i>					
Test statistic	223.182 [0.046]	203.038 [0.192]	145.985 [1.000]	116.708 [1.000]	71.585 [1.000]
<i>D. Mothers earning above 25th percentile (age-adjusted)</i>					
Test statistic	194.349 [0.538]	175.108 [1.000]	131.453 [1.000]	119.043 [1.000]	91.815 [1.000]
<i>E. Mothers continuous antidepressant medication use</i>					
Test statistic	175.585 [1.000]	136.683 [1.000]	128.979 [1.000]	103.295 [1.000]	77.078 [1.000]
<i>F. Mothers continuous anti-anxiety medication use</i>					
Test statistic	180.760 [1.000]	154.463 [1.000]	139.915 [1.000]	101.455 [1.000]	95.274 [1.000]
<i>G. Children's average test scores</i>					
Test statistic	237.188 [0.009]	235.940 [0.005]	214.900 [0.033]	183.645 [0.387]	145.133 [1.000]
<i>H. School value added in third grade</i>					
Test statistic	4005.189 [0.000]	1090.418 [0.000]	126.298 [1.000]	218.940 [0.010]	131.147 [1.000]
<i>I. Children continuous ADHD medication use</i>					
Test statistic	163.634 [1.000]	136.770 [1.000]	126.092 [1.000]	91.684 [1.000]	52.104 [1.000]
d.f.	183	178	173	168	163

Notes: This table displays the Frandsen, Lefgren, and Leslie (2023) joint test for strict monotonicity and exclusion. Parents' outcomes are measured three years after the ruling and children's outcomes are measured for the youngest child in the case. The test implies that outcomes averaged at the judge level should be a continuous function of the judge-level propensity, with bounded slope. The table shows the test statistic and p-value (in square brackets) associated with a separate test for each main outcome, and degrees of freedom. Failure to reject the null hypothesis means that the exclusion restriction and monotonicity holds.

Table A5: Joint Custody and Fathers' Earnings for Full Sample

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
<i>Panel A: Earning above the 25th percentile (age-adjusted)</i>					
Joint custody	0.105 (0.064)	0.172*** (0.065)	0.180*** (0.067)	0.170** (0.066)	0.126* (0.069)
Control complier mean	0.544 (0.054)	0.484 (0.055)	0.455 (0.056)	0.478 (0.055)	0.453 (0.058)
Dep. Mean	0.535	0.534	0.533	0.543	0.544
Observations	19,957	19,384	18,005	16,579	15,155
<i>Panel B: Earning above the 50th percentile (age-adjusted)</i>					
Joint custody	0.111* (0.060)	0.137** (0.061)	0.118* (0.063)	0.072 (0.062)	0.072 (0.066)
Control complier mean	0.231 (0.048)	0.195 (0.049)	0.254 (0.049)	0.307 (0.049)	0.285 (0.052)
Dep. Mean	0.286	0.289	0.292	0.297	0.303
Observations	19,957	19,384	18,005	16,579	15,155
<i>Panel C: Earning above the 75th percentile (age-adjusted)</i>					
Joint custody	0.088* (0.046)	0.058 (0.046)	0.031 (0.048)	0.046 (0.047)	0.021 (0.049)
Control complier mean	0.085 (0.034)	0.091 (0.035)	0.131 (0.035)	0.159 (0.035)	0.149 (0.037)
Dep. Mean	0.126	0.125	0.125	0.128	0.131
Observations	19,957	19,384	18,005	16,579	15,155

Notes: This table shows the earnings results for the full sample of fathers, including all years of available earnings data. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level.*** p<.01, ** p<.05, * p<.1.

Table A6: Joint Custody and Mothers' Earnings Full Sample

	(1) Year 1	(2) Year 2	(3) Year 3	(4) Year 4	(5) Year 5
<i>Panel A: Earning above the 25th percentile (age-adjusted)</i>					
Joint custody	-0.071 (0.066)	-0.091 (0.068)	-0.019 (0.068)	0.073 (0.067)	-0.013 (0.070)
Control complier mean	0.617 (0.055)	0.650 (0.056)	0.612 (0.056)	0.532 (0.055)	0.595 (0.057)
Dep. Mean	0.599	0.602	0.610	0.613	0.621
Observations	20,114	19,636	18,297	16,942	15,579
<i>Panel B: Earning above the 50th percentile (age-adjusted)</i>					
Joint custody	-0.058 (0.065)	-0.084 (0.066)	-0.019 (0.066)	0.039 (0.066)	-0.028 (0.069)
Control complier mean	0.369 (0.053)	0.398 (0.054)	0.355 (0.054)	0.313 (0.053)	0.336 (0.056)
Dep. Mean	0.321	0.327	0.334	0.343	0.353
Observations	20,114	19,636	18,297	16,942	15,579
<i>Panel C: Earning above the 75th percentile (age-adjusted)</i>					
Joint custody	-0.026 (0.050)	-0.017 (0.052)	0.045 (0.051)	0.012 (0.051)	-0.015 (0.054)
Control complier mean	0.196 (0.041)	0.175 (0.041)	0.129 (0.040)	0.137 (0.040)	0.167 (0.043)
Dep. Mean	0.133	0.138	0.139	0.148	0.151
Observations	20,114	19,636	18,297	16,942	15,579

Notes: This table shows the earnings results for the full sample of mothers, including all years of available earnings data. All estimates control for variables listed in table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level.*** p<.01, ** p<.05, * p<.1.

Table A7: Joint Custody and Children's Test Scores and School Quality, by Gender

	(1) Average Score	(2) Swedish Score	(3) Math Score
<i>Panel A: Test scores by subject (first test taken)</i>			
Girl	0.684*** (0.227)	0.758*** (0.229)	0.487** (0.227)
Boy	0.141 (0.235)	-0.233 (0.240)	0.463** (0.235)
Dep. Mean	-0.301	-0.256	-0.313
Observations	10,814	10,814	10,814
	(1) Grade 3	(2) Grade 6	(3) Grade 9
<i>Panel B: School value added</i>			
Girl	0.237*** (0.088)	0.099 (0.076)	-0.027 (0.061)
Boy	0.227** (0.089)	0.084 (0.082)	-0.029 (0.069)
Dep. Mean	-0.016	-0.019	0.002
Observations	5,261	4,882	4,324

Notes: The table presents IV estimates of joint custody on children's education outcomes. Test scores are from the first national standardized test taken after the custody dispute, administered in grades 3, 6, and 9. Average score is the mean of Swedish and math scores. Only children with both scores are included. Scores are standardized (mean 0, SD 1) on the full population of test takers. School value added is constructed by regressing each student's average scores on child gender, age, migration status, parental migration status, parental education, and parental earnings, separately by grade and test year, using the full Swedish student population with available scores. Residuals from this regression are used to calculate a leave-out mean, approximating a school value-added measure. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

Table A8: Joint Custody and Children's Test Scores and School Quality for Full Sample

	(1) Average Score	(2) Swedish Score	(3) Math Score
<i>Panel A: Test scores by subject (first test taken)</i>			
Joint custody	0.388** (0.190)	0.320* (0.193)	0.378** (0.191)
Control complier mean	-0.546 (0.152)	-0.465 (0.153)	-0.543 (0.153)
Dep. Mean	-0.288	-0.244	-0.303
Observations	12,814	12,814	12,814
	(1) Grade 3	(2) Grade 6	(3) Grade 9
<i>Panel B: School value added</i>			
Joint custody	0.231*** (0.086)	0.079 (0.075)	0.039 (0.052)
Control complier mean	-0.124 (0.067)	-0.129 (0.058)	-0.017 (0.043)
Dep. Mean	-0.015	-0.021	-0.005
Observations	5,370	5,062	6,425

Notes: The table presents IV estimates of joint custody on children's education outcomes. Test scores are from the first national standardized test taken after the custody dispute, administered in grades 3, 6, and 9. Average score is the mean of Swedish and math scores. Only children with both scores are included. Scores are standardized (mean 0, SD 1) on the full population of test takers. School value added is constructed by regressing each student's average scores on child gender, age, migration status, parental migration status, parental education, and parental earnings, separately by grade and test year, using the full Swedish student population with available scores. Residuals from this regression are used to calculate a leave-out mean, approximating a school value-added measure. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

Table A9: OLS Results

	Earn >25th		Antidepressant		Anti-anxiety	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Complier Re-weighted	OLS	Complier Re-weighted	OLS	Complier Re-weighted
<i>Panel A: Fathers</i>						
Joint custody	0.082*** (0.009)	0.085*** (0.014)	-0.006 (0.005)	-0.005 (0.009)	-0.012*** (0.005)	-0.004 (0.009)
Dep. Mean	0.552	0.481	0.054	0.087	0.061	0.099
Observations	14,830	14,830	14,830	14,830	14,830	14,830
<i>Panel B: Mothers</i>						
Joint custody	0.031*** (0.009)	0.032** (0.013)	0.014** (0.006)	0.019* (0.011)	0.005 (0.006)	0.017 (0.010)
Dep. Mean	0.630	0.606	0.103	0.136	0.092	0.116
Observations	15,076	15,076	15,076	15,076	15,076	15,076
	Test Scores		School Value Added		ADHD	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Complier Re-weighted	OLS	Complier Re-weighted	OLS	Complier Re-weighted
<i>Panel C: Children</i>						
Joint custody	0.003 (0.213)	0.012 (0.242)	0.006 (0.008)	0.008 (0.012)	-0.003 (0.003)	-0.003 (0.005)
Dep. Mean	-0.301	-0.328	-0.016	-0.014	0.023	0.028
Observations	10,814	10,814	5,261	5,261	15,271	15,271

Notes: The table displays results from OLS regressions. Earnings outcomes are measured as earning above the 25th earnings percentile three years after the custody rulings. Health outcomes indicate that continuously using antidepressants or anti-anxiety medication three years after the ruling. Test scores are measured as the average score on the first national standardized test the child takes after the custody ruling. All estimations include court-by-year fixed effects. Standard errors are given in parentheses and clustered on the case level. *** p<.01, ** p<.05, * p<.1.

Table A10: Complier Means and Estimation Sample Means

	Complier mean	Sample mean
Plaintiff is female	0.677	0.683
Child is a girl	0.480	0.485
Child's age	7.888	7.930
Father's age	40.611	40.616
Mother's age	36.532	36.622
Child foreign born	0.079	0.084
Father foreign born	0.453	0.475
Mother foreign born	0.426	0.434
Parents cohabiting	0.356	0.348
Number of children in case	1.906	1.961
Father positive earnings	0.905	0.892
Father log earnings	9.159	8.950
Mother positive earnings	0.903	0.895
Mother log earnings	8.703	8.618
Father education level: Compulsory	0.230	0.250
Father education level: High School	0.477	0.463
Father education level: College	0.251	0.240
Mother education level: Compulsory	0.227	0.233
Mother education level: High school	0.435	0.443
Mother education level: College	0.294	0.285
Child mental health medication	0.020	0.022
Father mental health medication	0.156	0.166
Mother mental health medication	0.216	0.228
Father ever convicted	0.188	0.222
Mother ever convicted	0.038	0.048

Notes: Column 1 reports estimated complier characteristics, while column 2 reports summary statistics for the baseline estimation sample (Appendix Table A1) for comparison. Complier characteristics are estimated using the procedure described in Amaral et al. (2023, Appendix B). The method decomposes the sample into never-takers, always-takers, and compliers and recovers complier means from the estimated shares of each compliance type under the LATE framework, following Marbach and Hangartner (2020) and Dahl, Kostøl, and Mogstad (2014).

Table A11: Results Mother Sole Custody

	(1) Earnings >25th	(2) Antidepressant Medication	(3) Anti-anxiety Medication
<i>Panel A: Fathers</i>			
Mother sole custody	-0.321** (0.139)	0.197*** (0.072)	0.122* (0.067)
Control complier mean	0.793 (0.102)	0.006 (0.044)	-0.002 (0.045)
Dep. Mean	0.535	0.057	0.062
Observations	14,830	14,830	14,830
<i>Panel B: Mothers</i>			
Mother sole custody	-0.166 (0.137)	0.017 (0.085)	0.027 (0.083)
Control complier mean	0.809 (0.103)	0.086 (0.064)	-0.002 (0.065)
Dep. Mean	0.618	0.100	0.092
Observations	15,076	15,076	15,076
	(1) Average Test Scores	(2) School Value Added	(3) ADHD Medication
<i>Panel C: Children</i>			
Mother sole custody	-0.707* (0.364)	-0.299** (0.133)	-0.009 (0.037)
Control complier mean	0.226 (0.262)	0.299 (0.133)	0.009 (0.037)
Dep. Mean	-0.301	-0.016	0.023
Observations	10,814	5,261	15,271

Notes: The table provides IV results for the alternative treatment where mother receives sole custody. Earnings outcomes are measured as earning above the 25th earnings percentile three years after the custody rulings. Health outcomes indicates that continuously using antidepressants or anti-anxiety medication three years after the ruling. Test scores are measured as the average score on the first national standardized test the child takes after the custody ruling. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

Table A12: Results Joint Legal Custody

	(1) Earnings >25th	(2) Antidepressant Medication	(3) Anti-anxiety Medication
<i>Panel A: Fathers</i>			
Joint legal custody	0.330*** (0.099)	-0.141*** (0.051)	-0.084* (0.048)
Control complier mean	0.413 (0.079)	0.177 (0.042)	0.106 (0.039)
Dep. Mean	0.518	0.057	0.063
Observations	12,719	12,719	12,719
<i>Panel B: Mothers</i>			
Joint legal custody	0.009 (0.095)	-0.026 (0.062)	-0.027 (0.061)
Control complier mean	0.586 (0.077)	0.132 (0.049)	0.100 (0.049)
Dep. Mean	0.609	0.101	0.095
Observations	12,948	12,948	12,948
	(1) Average Test Scores	(2) School Value Added	(3) ADHD Medication
<i>Panel C: Children</i>			
Joint legal custody	0.661** (0.279)	0.278** (0.115)	0.027 (0.026)
Control complier mean	-0.781 (0.219)	-0.278 (0.115)	-0.027 (0.026)
Dep. Mean	-0.328	-0.018	0.024
Observations	9,197	4,408	13,123

Notes: The table provides IV results for the alternative treatment where joint custody is defined as joint legal custody, disregarding cases that only involves residence. Earnings outcomes are measured as earning above the 25th earnings percentile three years after the custody rulings. Health outcomes indicates that continuously using antidepressants or anti-anxiety medication three years after the ruling. Test scores are measured as the average score on the first national standardized test the child takes after the custody ruling. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

Table A13: Testing the Judge Case Restriction

	Earn >25th		Antidepressant		Anti-anxiety	
	(1)	(2)	(3)	(4)	(5)	(6)
Cases:	45	55	45	55	45	55

Panel A: Fathers

Joint Custody	0.191** (0.078)	0.279*** (0.087)	-0.101** (0.040)	-0.116*** (0.044)	-0.057 (0.038)	-0.064 (0.042)
Dep. Mean	0.553	0.552	0.054	0.056	0.062	0.062
Observations	16,369	12,640	16,369	12,640	16,369	12,640

Panel B: Mothers

Joint Custody	0.014 (0.078)	0.002 (0.086)	0.022 (0.050)	0.011 (0.055)	0.011 (0.049)	0.029 (0.055)
Dep. Mean	0.630	0.630	0.105	0.102	0.093	0.093
Observations	16,640	12,846	16,640	12,846	16,640	12,846

	Test Scores		School Value Added		ADHD	
	(1)	(2)	(3)	(4)	(5)	(6)
Cases:	45	55	45	55	45	55

Panel C: Children

Joint Custody	0.310 (0.213)	0.449* (0.242)	0.178** (0.079)	0.239*** (0.092)	0.015 (0.021)	0.006 (0.024)
Dep. Mean	-0.304	-0.299	-0.017	-0.016	0.023	0.023
Observations	11,939	9,223	5,759	4,477	16,854	13,020

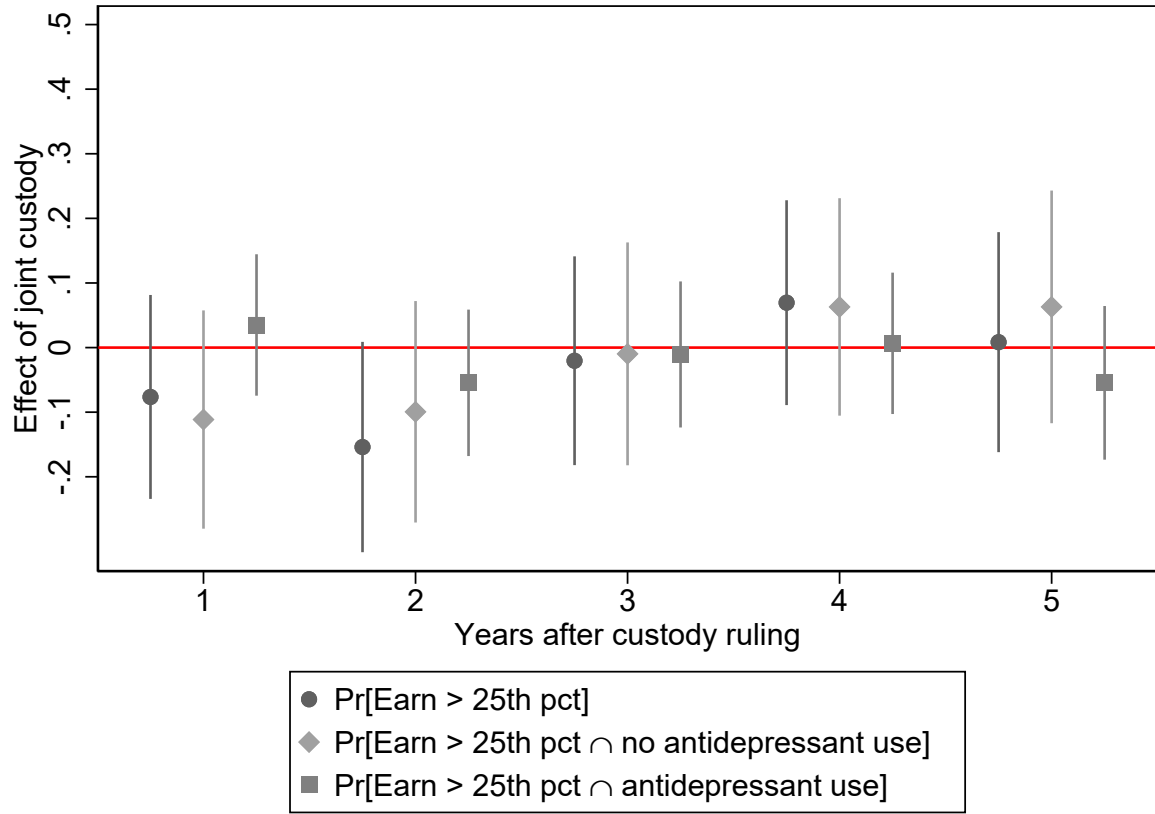
Notes: The table displays results using different judge case restriction. Case restrictions deviates from the baseline 50 cases by +/- 5 cases. Earnings outcomes are measured as earning above the 25th earnings percentile three years after the custody rulings. Health outcomes indicates that continuously using antidepressants or anti-anxiety medication three years after the ruling. Test scores are measured as the average score on the first national standardized test the child takes after the custody ruling. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

Table A14: Testing the Exclusion Restriction

	Earn >25th		Antidepressant		Anti-anxiety	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Fathers</i>						
Joint Custody	0.257*** (0.084)	0.276*** (0.092)	-0.120*** (0.043)	-0.129*** (0.049)	-0.057 (0.041)	-0.066 (0.048)
Mediation		0.045 (0.263)		-0.121 (0.142)		-0.214 (0.148)
Investigation		-0.047 (0.161)		0.032 (0.087)		0.111 (0.092)
Dep. Mean	0.552	0.535	0.054	0.057	0.061	0.062
Observations	14,830	14,830	14,830	14,830	14,830	14,830
<i>Panel B: Mothers</i>						
Joint Custody	-0.024 (0.082)	-0.077 (0.101)	0.020 (0.053)	0.070 (0.067)	0.009 (0.052)	0.060 (0.057)
Mediation		0.456 (0.289)		-0.301 (0.190)		-0.075 (0.168)
Investigation		-0.322* (0.177)		0.173 (0.117)		0.074 (0.101)
Control for med/inv-prop	Yes	No	Yes	No	Yes	No
Instrument for med/inv	No	Yes	No	Yes	No	Yes
Dep. Mean	0.630	0.618	0.103	0.100	0.092	0.092
Observations	15,076	15,076	15,076	15,076	15,076	15,076
	Test Scores		School Value Added		ADHD	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel C: Children</i>						
Joint Custody	0.469** (0.227)	0.428* (0.238)	0.236*** (0.089)	0.242*** (0.094)	0.019 (0.022)	0.018 (0.022)
Mediation		0.196 (0.662)		-0.049 (0.186)		-0.019 (0.067)
Investigation		-0.255 (0.410)		0.034 (0.119)		-0.006 (0.041)
Control for med/inv-prop	Yes	No	Yes	No	Yes	No
Instrument for med/inv	No	Yes	No	Yes	No	Yes
Dep. Mean	-0.301	-0.301	-0.016	-0.016	0.023	0.023
Observations	10,814	10,814	5,261	5,261	15,271	15,271

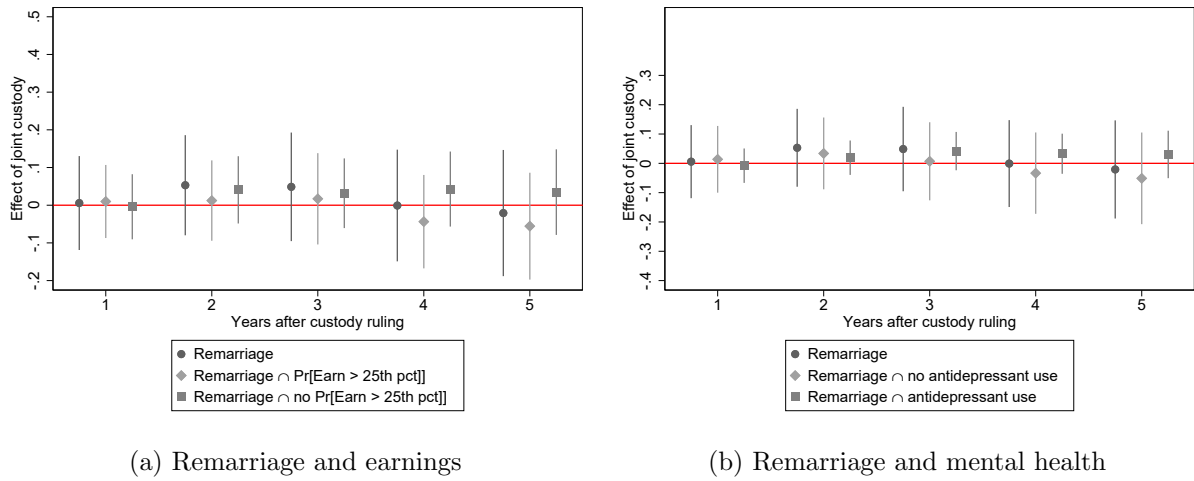
Notes: The table displays estimations including additional decisions judges make in the court processes. Mediation refers to ordering mediation talks between parents and a third party. Investigation refers to ordering a formal investigation by the social welfare office into the family. The first column adds in propensities as controls. The second column adds in instruments for the additional endogenous variables. These are made analogous to how we construct the main instrument. Earnings outcomes are measured as earning above the 25th earnings percentile three years after the custody rulings. Health outcomes indicates that continuously using antidepressants or anti-anxiety medication three years after the ruling. Test scores are measured as the average score on the first national standardized test the child takes after the custody ruling. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are given in parentheses and clustered at the case level. *** p<.01, ** p<.05, * p<.1.

Figure A1: Decomposing the Effect of Joint Custody on Mothers' Earnings and Mental Health



Notes: The figure reports IV estimates of the effect of joint custody on mothers' earnings and joint earnings–mental health outcomes one to five years after the custody ruling. For each year, the first estimate shows the effect on earning above the 25th percentile of the earnings distribution. The remaining estimates show the effects on earning above the 25th percentile while not using antidepressants and while using antidepressants in the same year, respectively. The latter two outcomes partition the probability of earning above the 25th percentile. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are clustered at the case level. Error bars denote 95% confidence interval.

Figure A2: Decomposing the Effect of Joint Custody on Mothers Remarrying, Earnings and Mental Health



Notes: The figure reports IV estimates of the effect of joint custody on mothers' remarriage and decompositions of this outcome. In each panel, the first estimate shows the effect on remarriage. Panel A partitions remarriage according to whether mothers earn above or below the 25th percentile of the earnings distribution. Panel B partitions remarriage according to whether mothers do or do not use antidepressants in the same year. All estimates control for variables listed in Table 1 and court-by-year fixed effects. Standard errors are clustered at the case level. Error bars denote 95% confidence interval.