Under the Influence: Marijuana Legalization, Crime, and Father Well-Being in Fragile Families

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Abstract

This paper studies the effect of state medical marijuana legalization on a broad set of family well-being outcomes using data from the Fragile Families and Child Wellbeing Study (FFCWS), a longitudinal cohort of largely unmarried, economically disadvantaged parents. Exploiting staggered state-level adoption of medical marijuana laws between 2001 and 2018, I estimate both a two-way fixed effects (TWFE) event-study specification and the state-clustered Callaway and Sant'Anna group-time average treatment effect estimator across eight outcome domains: mental health, substance use, criminal justice contact, labor market activity, relationship and family structure, intimate partner violence, parenting behavior, and housing and food insecurity. The significant father-side effects include a reduced likelihood of repartnering after separation, increased criminal justice contact, worsened mental health, and greater receipt of welfare. A mechanism check shows the mental health effect does not attenuate when criminal justice contact is added as a control, suggesting a channel operating independently of legal exposure and not through decriminalization. A heterogeneity check shows these effects are concentrated among fathers who had not used marijuana prior to legalization, consistent with the effects operating through legalization-induced changes in behavior, not simply pre-existing use. This pattern is evidence that medical marijuana legalization affects family well-being through a channel specific to fathers' own exposure, not a generic household-level or state-level confound.

JEL Codes: I12, I18, J12, K42, J13

Keywords: marijuana legalization, fragile families, father well-being, criminal justice, mental health, staggered difference-in-differences

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1 Introduction

Between 2001 and 2018, thirty-two U.S. states and the District of Columbia legalized medical marijuana, and six of those states went on to legalize recreational use within the same window. This wave of state-level policy change is among the largest peacetime shifts in drug policy in American history, and it arrived precisely as a generation of children in economically fragile households were growing up. Its net economic consequences remain contested: estimates of the benefits and costs of legalization vary widely depending on which margins are weighed (Brown, Cohen, and Felix, 2023). Whether legalization changed the wellbeing of the parents raising those children, and if so how, is a question with direct relevance to family policy but limited direct evidence, because most of the existing literature on marijuana legalization studies population-level outcomes (crime rates, treatment admissions, traffic fatalities), not the individual parents inside a single, consistently observed panel of families.

This paper uses the Fragile Families and Child Wellbeing Study (FFCWS), a longitudinal birth-cohort study of parents in twenty large U.S. cities, most of whom were unmarried at the time of the focal child's birth, to ask a direct question: did state medical marijuana legalization change the wellbeing of the mothers and fathers in these families, and did it change it differently for the two parents? Fathers are a particularly apt lens for this question. Roughly half of fathers in a fragile family report at least one of physical abuse, a substance use disorder, or a period of incarceration (Waller and Swisher, 2006), and a body of work built around this same sample has argued that fathers' own trajectories, not just mothers' or children's, need to be studied directly, not folded into a household-level average (Carlson and McLanahan, 2010). I construct a set of forty-one outcomes spanning eight domains: mental health, substance use, criminal justice contact, labor market activity, relationship and family structure, intimate partner violence, parenting behavior, and housing and food insecurity; and estimate the effect of each parent's state medical marijuana legalization status using both a standard two-way fixed effects (TWFE) event-study design and the Callaway and Sant'Anna (2021) staggered-adoption estimator, which corrects for the negative-weighting problems that afflict TWFE under staggered treatment timing (Callaway and Sant'Anna, 2021).

The central finding is an asymmetry that survives every specification test I apply. Of fifteen father-side outcomes tested, five are significant under the state-clustered Callaway-Sant'Anna (CS) estimator: fathers become less likely to repartner after a relationship ends, more likely to have criminal justice contact, more likely to show signs of depression or another mental health condition, and more likely to receive welfare. Of twenty-six mother-side and child-side outcomes tested under the identical specification and the identical sample construction, not one is significant. The pattern is not subtle: whatever medical marijuana legalization is doing to the parents in fragile families, it

is doing it almost exclusively to fathers.

The repartnering result indicates that fathers in states that legalize medical marijuana become 3.4 percentage points less likely to have repartnered with a new partner following a relationship dissolution (state-clustered ATT $= -0.034$, $p = 0.022$), an effect that survives a fully dynamic Callaway-Sant'Anna event-time decomposition with a flat pre-trend and a post-treatment effect that grows monotonically from roughly -0.02 at the year of legalization to -0.12 eight to ten years out. It is not the only father-side finding, and I do not treat it in isolation. A heterogeneity check shows the repartnering effect, along with the mental health, criminal justice, and welfare effects, is concentrated almost entirely among fathers who had not used marijuana before legalization, and a mechanism check shows that the mental health effect does not attenuate when criminal justice contact is added as a statistical control, which points toward a channel that operates on father wellbeing directly and not through the legal system.

These are not effects that matter only for the father himself. A body of econometric work on parental behavior has established that what happens to a parent does not stay with the parent: a parent's problem drinking predicts worse labor market outcomes for that parent's own adult children (Balsa, 2008), a supply-driven increase in parental methamphetamine use raises the probability a child enters foster care (Cunningham and Finlay, 2013), and maternal depression and substance use jointly depress child human capital accumulation (Frank and Meara, 2009). Fathers' repartnering, mental health, and criminal justice trajectories are, in this same sense, not endpoints; they are plausible inputs into the household environment surrounding the children in this paper's own mother- and child-side outcome set, even though I do not find that legalization moves those child-side outcomes directly. Documenting what happens to fathers is therefore not a narrower question than documenting what happens to families: it is very often the same question, asked at the point where the underlying behavior actually originates.

This paper differs from the existing literature on marijuana legalization in four ways. First, nearly all of the existing economics literature on marijuana policy studies administrative or population-representative survey data at the state or metropolitan level (Anderson and Rees, 2023; Dave et al., 2022; Morris et al., 2014); this paper instead follows a single panel of parents over as many as six survey waves, which lets me hold the household fixed and ask whether a specific parent's own trajectory bends around the date their state legalized. Second, the existing literature on marijuana and family or relationship outcomes is thin and its results point in different directions depending on the outcome and sample (Baggio et al., 2024; Baldwin, 2024); I bring a set of forty-one outcomes to bear on the same sample under the same design, which lets the mother- and child-side null results function as an internal specificity check on the father-side findings, instead of leaving the reader to wonder whether the significant results are the product of chance in an unreported wider search. Third, I use the sample's own asymmetry as an identification device: because mothers and fathers

in the same households face the same state policy environment at the same time, a treatment effect that shows up for one parent and not the other is difficult to explain as a confound correlated with legalization timing at the state or household level, and easier to explain as something operating through the treated parent's own behavior. Finally, the data source itself is underused for a question of this kind. The applied economics literature on how a parent's own behavior propagates through the household draws overwhelmingly on general-population panels such as the NLSY, PSID, and SIPP. FFCWS is comparatively rare in this specific literature despite being, by design, better suited to it: it oversamples non-marital births specifically to capture the economically fragile households in which father instability of the kind studied here is most concentrated, and unlike surveys built around a single household respondent, it interviews mothers and fathers separately and follows each of them across the same set of waves, which is what makes it possible to ask whether a shared policy shock affects the two parents in the same household differently.

The paper proceeds as follows. Section 2 reviews the relevant literature on marijuana legalization and situates the paper's contribution. Section 3 lays out the institutional background of state medical marijuana legalization. Section 4 describes the FFCWS data, the sample construction, and how the legalization policy variable is merged onto the panel. Section 5 lays out the empirical strategy, including the TWFE event-study specification and the Callaway-Sant'Anna estimator. Section 6 presents the main results by domain. Sections 7 and 8 report a robustness check; and mechanism, heterogeneity and specificity check. Section 9 discusses the findings, and Section 10 concludes.

2 Related Literature and Contribution

2.1 Related Literature

The economics literature on medical marijuana legalization and crime has generally found no evidence that legalization increases crime, and some evidence that it modestly reduces certain categories of violent crime. Using state panel data from 1990 to 2006, Morris et al. (2014) find no crime-exacerbating effect of medical marijuana laws on any FBI Part I offense category, and some evidence of a reduction in homicide and assault rates. This literature, however, is built almost entirely on aggregate crime statistics: it can speak to whether a state's overall crime rate moves after legalization, but it cannot speak to whether the parents in a specific type of household (the low-income, largely unmarried households that make up the Fragile Families sample) experience more or less of their own criminal justice contact. The finding in this paper that fathers' own criminal justice contact rises after legalization is not necessarily in tension with a falling aggregate crime rate: a state can see less crime in the aggregate while a specific, already-vulnerable subpopulation

experiences more legal exposure, for instance through enforcement of the still-substantial regulatory apparatus that surrounds "legal" marijuana (licensing, possession limits, public consumption rules, workplace policies) or through second-order effects of increased use.

The literature on marijuana legalization and mental health raises a related concern from a different direction, and it is considerably more mixed than the crime literature. Borbely et al. (2022) find that marijuana legalization increases marijuana use but has no detectable average effect on mental health, with effects that vary substantially by age: elderly individuals with pre-existing chronic conditions appear to benefit from medical marijuana legalization, while recreational legalization is associated with worse mental health among adults under 35. Ortega (2023) finds that recreational marijuana legalization is followed by a decline in mental health treatment admissions, which the author interprets cautiously, since a decline in treatment admissions is consistent with either genuinely improved mental health or with a substitution away from formal treatment toward self-management with a now-legal substance. None of this literature isolates fathers in low-income, non-marital-birth households, and none of it can distinguish a direct effect of legalization on mental health from an effect that runs through the criminal justice system, a distinction this paper addresses directly with a mechanism check in Section 8.1. This gap is notable given how developed the corresponding literature is for other substances: the intergenerational transmission of parental drinking is well documented and, more importantly, heterogeneous, concentrated disproportionately among the heaviest-drinking parents instead of spread uniformly across the distribution (Schmidt and Tauchmann, 2011), and parental smoking measurably shapes children's cognitive and non-cognitive skill formation (Srivastava and Trinh, 2021). No comparable body of within-family econometric evidence yet exists for marijuana specifically, which is the gap this paper's design is built to start filling.

The labor market literature raises the same problem again, and it is less settled than it first appears. Dave et al. (2022) find little evidence that recreational marijuana legalization harms labor market outcomes for most working-age adults, with some evidence of employment gains concentrated in the agricultural sector as a new licit market opens. Using firm-level data instead of individual-level data, Pan (2025) reaches the opposite conclusion at the establishment level: recreational legalization reduces employment, investment, and sales growth at firms with high-skilled labor forces, a strong union presence, and greater local dispensary density, an effect the author attributes to legalization introducing labor-related frictions instead of opening a new market. The two studies are not strictly incompatible. Aggregate employment among working-age adults can hold roughly steady even as its composition shifts away from higher-skill firms, but the contrast is a useful reminder that "neutral to positive" is an average across a labor market literature that does not agree on sign once the unit of analysis changes. This paper's finding that fathers' welfare receipt rises after legalization, absent a corresponding change in employment or hours, is a different empirical object still: it speaks to

economic strain within a specific low-income population, not to aggregate or firm-level labor demand more broadly.

A closely related strand of the literature, on marijuana and family or relationship stability, is older and considerably thinner still. Kaestner (1997) finds that marijuana and cocaine use are both associated with reduced marital stability using a sample of young adults, though this work predates the modern legalization wave and cannot speak to a policy shock. More recent work on marijuana and intimate partner violence is directly conflicting: Baggio et al. (2024) find that recreational marijuana legalization is associated with a roughly 20 percent increase in intimate partner violence incidents in National Incident-Based Reporting System data, with poor mental health and binge drinking as candidate mediators, while Baldwin (2024), using a difference-in-differences design over 2013–2019, finds a substantial decrease in intimate partner violence following recreational legalization. This paper's own intimate partner violence results are uniformly null across all four IPV outcomes tested (Section 6.5), which does not resolve the conflict in the broader literature but does suggest that whatever mechanism drives IPV effects elsewhere, it is not detectable in this particular sample of largely non-marital-birth, economically disadvantaged households.

Across all three strands (crime, mental health, and labor markets and family structure) the same limitation recurs: none of this literature is built to isolate a specific, economically fragile population of parents, and none of it can hold the household fixed and compare a mother and a father facing the identical policy shock at the identical time. That gap is what the rest of this paper is designed to close.

2.2 Contribution

This paper's contribution is not a new causal channel from the literature above; it is a research design that lets a single, consistently measured sample of parents answer a question the literature has not directly posed: within the same households, facing the same state policy environment at the same time, does medical marijuana legalization affect mothers and fathers differently? The paper's design, following Callaway and Sant'Anna (2021) for the staggered-adoption problem and structured after the reduced-form policy-shock approach used by Flores, Gayle, and Hincapié (2023) in their study of parental leave policy, treats the set of mother- and child-side null results not as a limitation but as evidence supporting the paper's central claim: that the effect is genuinely father-specific and not a confound riding along with legalization timing.

The choice of data source is itself part of the contribution. The applied economics literature on how a parent's own behavior propagates through the household – Balsa (2008)'s finding that a parent's problem drinking predicts worse labor market outcomes for that parent's own adult children, for one example, uses the NLSY79 – has been built almost entirely on general-population

panels, chief among them the NLSY, PSID, and SIPP. FFCWS is comparatively rare in this specific literature, which is a missed opportunity, since it is arguably better suited to this kind of question than the panels more commonly used. It was designed from the outset to oversample non-marital births, so the economically fragile, largely unmarried households in which father instability of the kind studied here is most concentrated are not a thin subsample but the bulk of the data; and unlike surveys built around a single household respondent, FFCWS interviews mothers and fathers separately and follows both across the same set of waves, which is precisely what makes the within-household, cross-parent comparison at the heart of this paper's design possible in the first place.

The breadth of the outcome set is the third piece of the contribution. Forty-one outcomes across eight domains (mental health, substance use, criminal justice contact, labor market activity, relationship and family structure, intimate partner violence, parenting behavior, and housing and food insecurity) is deliberately more than a single-outcome paper would need. Estimating all forty-one under an identical specification and on an identical sample means the twenty-six null mother- and child-side results are not a separate robustness exercise; they are produced by the same procedure, on the same households, in the same regression framework as the five significant father-side results, which is what lets the null results function as an internal specificity check.

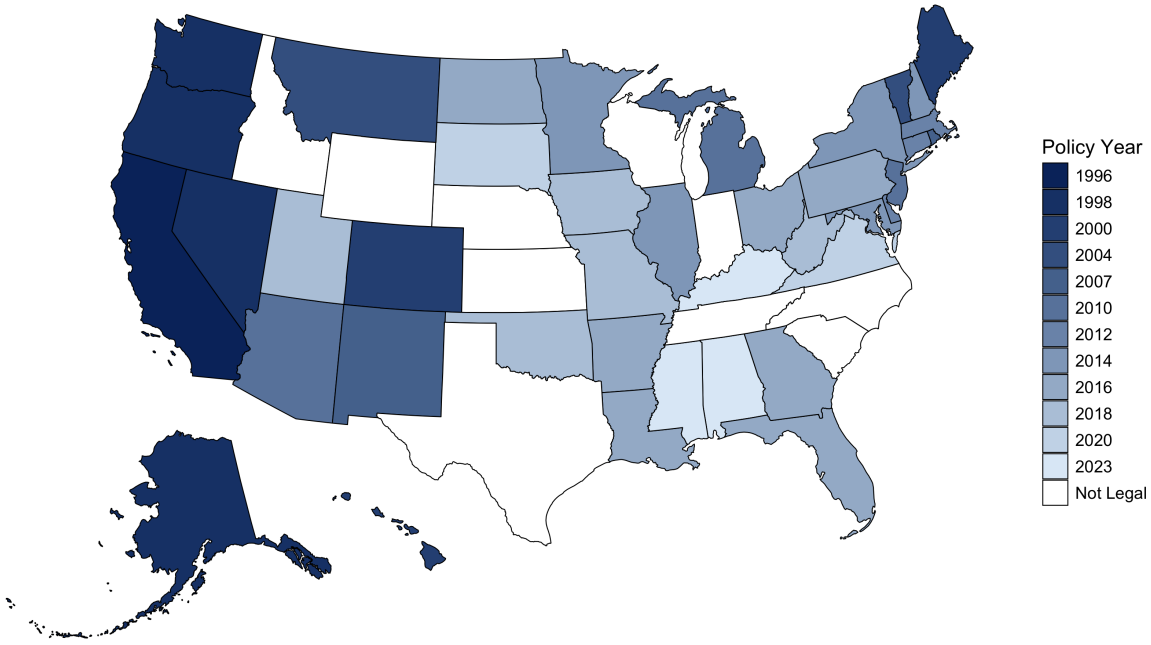
3 Institutional Background: Medical Marijuana Legalization

Medical marijuana legalization in the United States has proceeded state by state, not through a single federal action, producing exactly the kind of staggered policy variation that this paper's empirical strategy is built to exploit. Between 2001 and 2018, the years spanned by the FFCWS panel used in this paper, thirty-two states and the District of Columbia enacted a medical marijuana law, with adoption dates spread across nearly the entire window instead of clustered around a small number of years.

Figure 1 maps each state's year of medical marijuana legalization. The underlying policy file begins in 1998, so any state that legalized before that year would otherwise be miscoded as a 1998 adopter; cross-checking against the historical record confirms that California is the only state in the sample whose true legalization date, November 1996 (Proposition 215), predates the data window, and the figure applies that single correction. Alaska, Oregon, and Washington legalized via the same November 1998 ballot cycle as the data file's start date and are coded correctly as 1998; Nevada is correctly coded as 2000.

Figure 2 shows the same variation from a different angle, plotting the share of states in each U.S. Census region with an active medical marijuana law in each year, following Flores, Gayle,

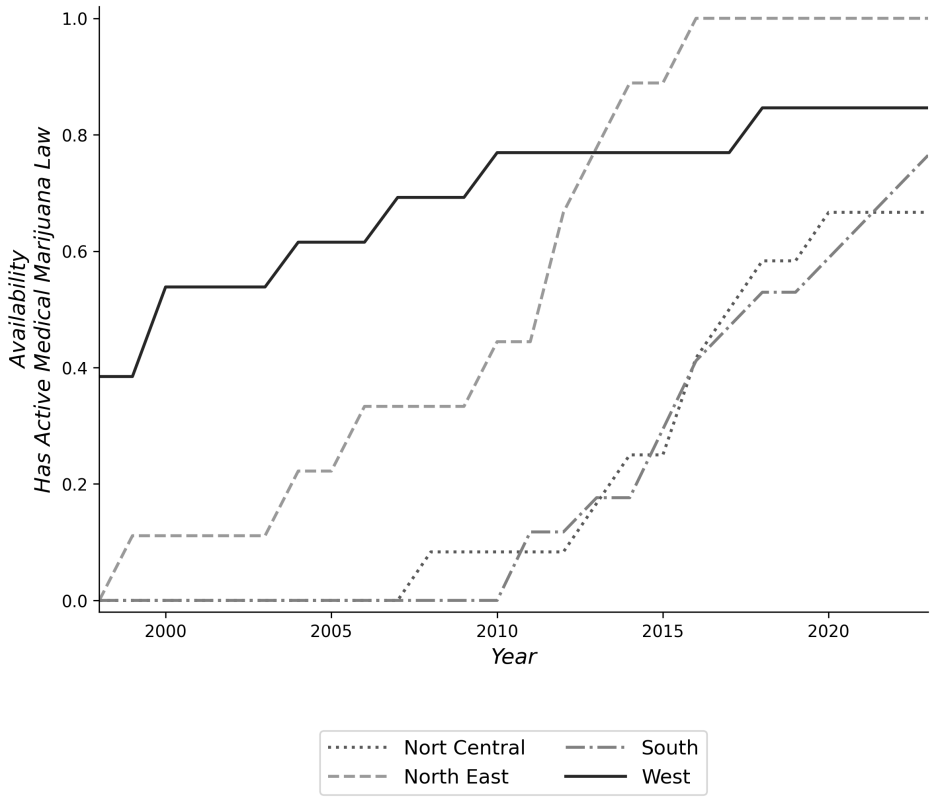
Figure 1: **Year of State Medical Marijuana Legalization**



and Hincapié (2023). The regional pattern makes the staggered-adoption logic underlying the identification strategy concrete: the West is already roughly 40 percent legalized at the start of the sample window and reaches 85 percent by 2018, the Northeast follows with a steep rise concentrated in the early-to-mid 2010s and reaches full legalization by 2016, while the Midwest and South lag both in level and timing throughout, only beginning a sustained rise after 2012. Every state in every region eventually contributes some pre- and some post-treatment observations to the analysis panel described in Section 4, but the timing of the switch, which is what the Callaway-Sant'Anna estimator exploits, differs sharply and systematically by region.

The binary "active law" distinction used throughout this paper is a considerably coarser measure of a state's underlying policy regime than the historical record supports, and it is worth being explicit about what that coarsening does and does not threaten. Pacula and Smart (2017) organize the evolution of state medical marijuana laws into three distinct waves, each triggered by a specific political event and not a smooth continuum: a ballot era (1996–2000) in which the first seven states, beginning with California's Proposition 215, passed vague laws with no patient registries and ambiguous treatment of dispensaries, against a backdrop of explicit federal opposition to the new policies; an early legislative era (2000–2009), beginning with Hawaii's S.B. 862, in which states added patient registries and cultivation limits but real dispensary access remained slow to develop

Figure 2: **Availability of Medical Marijuana Legalization by U.S. Census Region**



Notes: The figure shows the unweighted share of states in each region with an active medical marijuana law in each year, using the standard four-region U.S. Census Bureau classification (the District of Columbia is grouped with the South, its official Census division). *Nort Central*: Illinois, Indiana, Iowa, Kansas, Michigan, Minnesota, Missouri, Nebraska, North Dakota, Ohio, South Dakota, Wisconsin. *North East*: Connecticut, Maine, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, Vermont. *South*: Alabama, Arkansas, Delaware, District of Columbia, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia. *West*: Alaska, Arizona, California, Colorado, Hawaii, Idaho, Montana, Nevada, New Mexico, Oregon, Utah, Washington, Wyoming. Unlike the population-weighted regional averages in Flores, Gayle, and Hincapié (2023), this figure gives each state equal weight within its region, since the treatment variable here is a simple binary indicator for an active state law, not a share of the working-age population covered.

under continued federal uncertainty; and a late legislative era (2009–present), triggered by the U.S. Department of Justice's October 2009 memorandum instructing federal prosecutors not to pursue individuals in clear compliance with state medical marijuana law, which precipitated a rapid buildout of state-licensed dispensary systems and formal patient registries. Because the FFCWS panel used in this paper spans 2001 through 2018, it covers the tail of the early legislative era and the entirety of the late legislative era: a father whose state legalized in, say, 2003 and one whose state legalized in 2013 are coded identically as "treated" in this paper's specification despite entering meaningfully different regulatory environments, one with limited practical access to a supply channel and one with an established, state-licensed dispensary infrastructure. The

family and year fixed effects used throughout this paper do not require treatment to be measured at this level of granularity to remain unbiased for the average effect of legal status as coded, but the distinction matters for interpreting magnitudes: if the true "dose" of legal access grew over the sample period as dispensary infrastructure matured, pooling early- and late-era cohorts under a single indicator should, if anything, shrink the effects reported below instead of inflating them, since the Callaway-Sant'Anna estimator's cohort-specific effects are ultimately averaged into a single overall estimate.

This coarsening is also less costly than it might first appear, because access under even the earliest, vaguest laws in practice extended well beyond a narrow clinical population. In California, the first and most permissive ballot-era state, the most common conditions reported by patients seeking a physician's recommendation were difficult-to-verify complaints such as chronic pain, anxiety, and insomnia, not the terminal or severely debilitating illnesses invoked to justify Proposition 215's passage (Kilmer and MacCoun, 2017). Critics have long characterized the medical marijuana movement as a "Trojan horse" for broader legalization on precisely these grounds (Kilmer and MacCoun, 2017); for the purposes of this paper, that same permissiveness is a reason to expect that "active medical marijuana law" captures meaningfully increased legal access for a general low-income population like the one studied here, not a narrow benefit available only to a small number of severely ill patients.

4 Data

4.1 Data Source

This paper uses the Future of Families and Child Wellbeing Study (FFCWS), formerly the Fragile Families and Child Wellbeing Study, a longitudinal birth-cohort study administered by the Office of Population Research at Princeton University (McLanahan et al., 2024). The FFCWS follows a multistage sample: U.S. cities with a 1998 population over 200,000 were sampled first, then hospitals within cities, and finally births within hospitals, with selection probabilities proportional to city population to preserve representativeness.[1] The design deliberately oversampled non-marital births at a ratio of roughly three to one, producing a baseline sample with a substantially higher share of Black, Hispanic, and low-income families than the general U.S. population. Table 1 compares the FFCWS baseline sample to the 1998 U.S. population of women giving birth.

Both biological parents were interviewed at the child's birth and at five follow-up waves, corresponding to child ages of approximately 1, 3, 5, 9, and 15, along with the primary caregiver when

[1] Cities were shortlisted using the intensity of three dimensions – welfare generosity, child support enforcement, and labor market conditions – and the specific cities within each stratum were then selected at random. Appendix Figure A1 shows the twenty cities in the survey and the share of the sample's 4,898 births drawn from each.

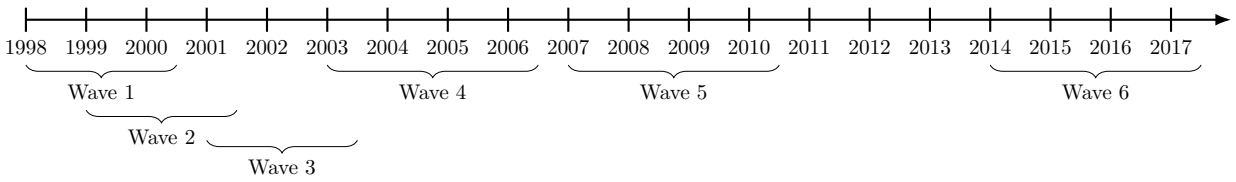
Table 1: **FFCWS Sample Compared to the 1998 U.S. Population**

		FFCWS Data	**1998 U.S. Population**	
		Women in Baseline Sample	Single Women Giving Birth	All Women
Race	White non-Hispanic	21%	40%	60%
	Black non-Hispanic	48%	32%	15%
	Hispanic	27%	24%	19%
	Other	4%	4%	6%
Unmarried		76%	100%	44%
US-Born		83%	83%	88%
Education	Less than High School	35%		22%
	High School	30%		33%
	Some College	24%		22%
	College or Graduate	11%		23%

Notes: This table compares the demographic characteristics of mothers in the FFCWS baseline sample to two reference groups from the 1998 U.S. population: single women who recently gave birth, and all women of childbearing age. The FFCWS surveyed families at the time of a child's birth in large U.S. cities (population over 200,000) between 1998 and 2000, with a deliberate oversample of unmarried parents, so the baseline sample is disproportionately unmarried, minority, and less educated relative to the general U.S. population, though broadly comparable to the national population of single women giving birth.

not a parent, the child, and, at later waves, the child's teacher, with in-home assessments conducted alongside several waves. A restricted-use geographic file records each parent's state of residence at every wave. Figure 3 shows the calendar years spanned by fieldwork for each wave; because interviews for a given wave were not completed within a single calendar year, the six waves' fieldwork periods overlap and together span nearly two decades. That overlap is what makes the sample useful for the present paper: the panel spans exactly the years of the modern medical marijuana legalization wave, letting me observe the same parent, in the same household, before and after their state of residence changes its marijuana law.

Figure 3: **FFCWS Fieldwork Timeline by Wave**



4.2 Sample Construction

I build the mother analysis panel using a three-criterion sample selection protocol; Table 2 summarizes the resulting sample sizes at each stage. The first criterion requires that a mother be present in

all six waves: of the 4,897 mothers observed at baseline, 2,530 remain through wave 6. The second criterion requires that the interviewed child be within roughly three months of the wave's target age. For example, 57 to 71 months for the wave 4 (age 5) interview, since fieldwork for a wave was often conducted over several calendar years and some children were consequently interviewed well past their target age; conditional on the first criterion, 1,809 mothers have a focal-age child at every wave. The third criterion requires non-missing values on the child outcome variables under study; conditional on the first two criteria, this leaves 1,618 mothers, three of whom have missing race data and are dropped, for a final analytic sample of 1,615 mothers. Table 3 shows that the distribution of mother's race and child's sex is essentially unchanged across waves conditional on satisfying all three criteria, which is consistent with attrition that is close to random within this restricted sample. The larger attrition rates at waves 5 and 6 likely reflect the longer span of time between those survey waves. The resulting mother panel contains 29,060–29,070 person-year observations on the same 1,615 mothers across 38 states. Fathers linked to these same 1,615 families are included in the father panel whenever they have a genuine interview record and non-missing outcome values; applying this restriction yields a father panel of 15,290–17,311 person-year observations on 924–964 fathers across 36 states, with the exact count varying slightly by outcome and reaching a maximum of 964 fathers pooled across the full outcome set.[2]

Table 2: **Progression of the Number of Mothers in the Sample**

Wave	**1**	**2**	**3**	**4**	**5**	**6**
Child's Age	**Birth**	**Year 1**	**Year 3**	**Year 5**	**Year 9**	**Year 15**
N_1 (mother in wave)	4897	4364	4009	3675	2986	2530
Attrition		533	355	334	689	456
Attrition rate (%)		10.9	8.1	8.3	18.7	15.3
N_2 (focal-age child)	4897	4249	3868	3541	2679	1809
Attrition		648	381	327	862	870
Attrition rate (%)		13.2	8.9	8.5	24.3	32.5
N_3 (non-missing outcome)	2822	2601	2466	2358	2152	1615
Attrition		221	135	108	206	537
Attrition rate (%)		7.8	5.2	4.4	8.7	25.0

Notes: N_1 restricts to mothers with a genuine interview record at each wave up to and including the wave shown. N_2 additionally restricts to mothers whose child was within the wave's target age window at the time of interview. N_3 additionally restricts to non-missing values on the child outcome variables under study; the wave-6 N_3 count of 1,615 already nets out three mothers with missing race data, dropped from the 1,618 mothers who otherwise satisfied all three criteria

[2]Because the exact set of non-missing fathers differs slightly across outcomes, Appendix Section A2 re-estimates the full father and mother/child outcome sets on the intersection of families with non-missing data on *every* outcome in each set (881 fathers; 1,444 mothers/children), as a check that the headline results are not an artifact of this outcome-specific sample composition.

Table 3: **Demographics Distribution over the Waves**

Wave	**1**	**2**	**3**	**4**	**5**	**6**
Child's Age	**Birth**	**Year 1**	**Year 3**	**Year 5**	**Year 9**	**Year 15**
N_3	**2822**	**2601**	**2466**	**2358**	**2152**	**1615**
Mother's Race						
White	20%	21%	22%	22%	22%	23%
Black	52%	51%	51%	51%	51%	51%
Hispanic	25%	25%	24%	24%	24%	23%
Other	3%	3%	3%	3%	3%	3%
Child's Sex						
Male	51%	51%	51%	51%	52%	51%
Female	49%	49%	49%	49%	48%	49%

Every father in the analysis is drawn from within the same 1,615-family mother sample described above, so the father-side results in this paper describe a subset of the same households studied on the mother side, not an independently constructed sample. The smaller father panel relative to the mother panel reflects a well-documented feature of the FFCWS design: father-specific measures, particularly mental health and substance use items, are only collected when the father is the interviewed parent or completes a partner-report module, so a meaningful share of the 1,615 families contribute no usable father-side data at all.

At baseline, mothers average 26.5 years of age (SD 6.0); 50.3 percent of mothers are Black, 23.3 percent are white, 23.0 percent are Hispanic, and 3.3 percent identify with another race or ethnicity, using the standard FFCWS race and ethnicity coding. Consistent with the sample's oversample of non-marital births, only 31.1 percent of mothers are married to the child's father at baseline. Average household size is 4.6 for the mother panel and 4.1 for the father panel. Every specification in this paper controls for household size, which varies within family over time and is a standard proxy for a family's economic circumstances and caregiving burden.

Table 4 reports baseline demographic and labor market characteristics for the mother and father samples splitting each sample into families ever observed living in a medical-marijuana-legal state at some point during the panel (*Ever-Legal*) and families never observed under one (*Never-Legal*). The Ever-Legal mothers and fathers are more likely to be Black and less likely to be white than their Never-Legal counterparts, reflecting that early- and ever-legalizing states in the sample include large, racially diverse states such as California, Illinois, Michigan, and New York. Ever-Legal fathers also report a meaningfully lower earnings bracket at baseline than Never-Legal fathers (5.42 versus 6.23 on the nine-point bracket scale), while the corresponding gap for mothers is negligible (4.07 versus 4.02). Neither pattern threatens identification, since every specification in this paper relies on within-family variation over time via family fixed effects, not a cross-sectional comparison between

Ever-Legal and Never-Legal families; the table is included to give the reader a transparent view of how the two groups differ at the point they enter the panel, not as a balance test the identification strategy depends on.

Table 4: **Descriptive Statistics of Mothers and Fathers at Baseline**

	Mothers			Fathers		
	Overall	Never-Legal	Ever-Legal	Overall	Never-Legal	Ever-Legal
Observations	1615	710	905	964	254	710
Black	0.50	0.44	0.55	0.48	0.44	0.49
White	0.23	0.30	0.18	0.26	0.36	0.23
Hispanic	0.23	0.24	0.22	0.23	0.19	0.24
Other Race	0.03	0.02	0.04	0.04	0.01	0.05
Less than High School	0.25	0.25	0.25	0.25	0.24	0.25
High School	0.30	0.31	0.28	0.30	0.25	0.33
Some College	0.32	0.32	0.32	0.28	0.33	0.26
College or Graduate	0.14	0.12	0.16	0.16	0.19	0.16
Married	0.31	0.32	0.30	–	–	–
Age	26.53	26.08	26.88	–	–	–
	(6.03)	(5.96)	(6.06)			
Household Size	4.44	4.32	4.54	3.40	3.17	3.45
	(1.56)	(1.45)	(1.64)	(1.80)	(1.39)	(1.88)
Employed	0.59	0.60	0.58	0.82	0.82	0.82
Hours Worked (Weekly)	35.79	35.74	35.83	44.55	44.43	44.60
	(11.71)	(11.68)	(11.74)	(11.37)	(12.57)	(10.91)
Earnings Bracket (1-9)	4.05	4.02	4.07	5.61	6.23	5.42
	(2.37)	(2.32)	(2.42)	(2.57)	(2.69)	(2.52)
Receiving Welfare	0.24	0.23	0.26	0.02	0.01	0.03

Notes: All characteristics measured at each family's first observed survey wave. *Ever-Legal* families are ever observed living in a state with an active medical marijuana law at some point during the panel (2001–2018); *Never-Legal* families are not. Standard deviations in parentheses for continuous measures. *Earnings Bracket* is the ordinal code of the bracketed earnings variable (1 = under \$5,000, 9 = \$60,000 or more); it is not a dollar-denominated mean. *Married* and *Age* are not available for fathers in this FFCWS extract, since marital status is a mother-reported household measure (matching its treatment as a mother-side outcome throughout the paper) and father's own age at baseline was not captured in the father-report modules used to build this panel.

4.3 The Marijuana Legalization Policy Variable

The treatment variable is an indicator for whether a parent's state of residence has an active medical marijuana law in effect in a given survey year, merged onto the panel from the state-year policy file described in Section 3. I use medical legalization as the primary treatment throughout the paper. I also constructed a recreational-legalization indicator during the data-development stage of this project; in the estimation sample, recreational legalization status is perfectly nested within

medical legalization status (every recreationally-legal state-year is also a medically-legal state-year), and only six states in the sample – California, Colorado, the District of Columbia, Massachusetts, Michigan, and Washington – ever adopt recreational legalization within the 2001–2018 window. Given this near-collinearity and the limited number of recreational-legal states, the paper's primary specification uses medical legalization only; Section 7.2 reports what happens when recreational status is used in its place, which is qualitatively similar for two of the paper's five significant father-side findings, though the state-clustered CS estimator itself could not be reliably estimated on this sparser treatment.

Legalization timing varies substantially across the sample's states, which is what identifies the staggered-adoption event-study design in Section 5. Roughly 56 percent of mothers and 74 percent of fathers in the analysis sample are ever observed living in a state with an active medical marijuana law during the panel window, with first-treatment years ranging across nearly the full span of the sample period.

4.4 Outcome Measures

I organize the outcome measures into eight domains: mental health (father and mother depression and broader mental health caseness, and three child mental-health and physical-health measures), substance use (father, mother, and child marijuana and other substance use), crime and justice (father and mother criminal justice contact, defined as any conviction or jail/incarceration spell), labor market activity (employment, hours worked, earnings bracket, and welfare receipt for both parents), relationship and family structure (marital status, repartnering, and the child's living arrangement with the biological father), intimate partner violence (mother-reported IPV from the biological father or a new partner, and father-reported IPV from the biological mother), parenting behavior (harsh parenting, parenting stress, and spanking), and housing and food insecurity (food insecurity for adults and children, and housing instability). Table 5 lists the full father-side outcome set and Table 9 lists the full mother- and child-side outcome set.

4.5 A Word of Caution

Some features of the data warrant caution in interpreting the results that follow. The father-specific outcomes are collected on a smaller and selected sample, since fathers who are estranged from the household at a given wave are less likely to complete a father-report module; to the extent that estrangement is itself correlated with the outcomes under study, the father-side estimates should be interpreted as applying to fathers who remain reachable by the survey, which is a meaningfully different population from all biological fathers in the original birth cohort. Nor can this paper's design entirely rule out selective migration (families that relocate in response to, or for reasons

correlated with, a state's marijuana policy could bias the treatment-timing comparison) though the sharp asymmetry between father- and mother-side results, discussed at length in Section 9, is itself difficult to reconcile with a migration story, since migration would presumably be a household-level decision affecting both parents similarly. Also, this paper studies a policy that was still actively changing throughout the sample window: several states in the panel legalized, amended their regulatory regime, or moved on to recreational legalization within the eighteen years observed here, and the broader methodological literature has cautioned that policy still in motion is harder to learn from cleanly than policy that has settled into a stable post-adoption regime (Pacula and Sevigny, 2014). The Callaway-Sant'Anna design used throughout this paper is a partial answer to that concern, since it estimates cohort-specific effects instead of pooling all adoption timings into a single coefficient, but it does not eliminate it entirely.

5 Empirical Strategy

5.1 Two-Way Fixed Effects Event Study

The baseline specification is a two-way fixed effects (TWFE) regression of outcome Y for individual i in state s and year t on an indicator for the individual's state having an active medical marijuana law, controlling for household size and absorbing family and year fixed effects:

$$Y_{ist} = \alpha + \beta \, \text{MedLegal}_{st} + \theta \, X_{ist} + \mu_i + \lambda_t + \varepsilon_{ist} \tag{1}$$

where X_{ist} is a vector of time-varying controls (household size in every specification reported); μ_i is a family fixed effect; and λ_t is a survey-year fixed effect. Because μ_i is a family-level fixed effect observed across all waves in the panel, it mechanically absorbs any time-invariant family characteristic, including race and ethnicity; a family- and year-fixed-effects design of this kind cannot separately identify the effect of a time-invariant covariate such as race, which is why race does not appear as a separate control in equation (1). Standard errors are clustered by state throughout, since the treatment varies at the state-year level (Bertrand, Duflo, and Mullainathan, 2004).

For the event-study version of equation (1), I replace the pooled treatment indicator with a set of binned event-time dummies, indexed by years relative to the individual's state's medical legalization date, binned at ± 5 years and with the dummy for $t = -1$ omitted as the reference period:

$$Y_{ist} = \alpha + \sum_{\substack{k=-5 \\ k \neq -1}}^{5} \beta_k \, \mathbb{1}[\text{EventTime}_{ist} = k] + \theta \, X_{ist} + \mu_i + \lambda_t + \varepsilon_{ist} \tag{2}$$

The β_k coefficients trace out the outcome's trajectory relative to the family's own legalization date and are the basis for the event-study figures reported alongside each domain's results in Section 6.

5.2 The Staggered-Adoption Problem and the Callaway-Sant'Anna Estimator

Equation (1) is a standard specification, but it is well known that TWFE estimates of a single pooled treatment coefficient under staggered treatment timing can be badly biased, because the implicit comparisons made by the TWFE estimator include "forbidden comparisons" of already-treated units against other already-treated units, with weights that can be negative when treatment effects are heterogeneous across cohorts or over time (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020). Because states in this sample legalize medical marijuana at different times across nearly the full eighteen-year panel window, this is a first-order concern, not a theoretical curiosity.

I therefore treat the Callaway and Sant'Anna (2021) group-time average treatment effect estimator as the paper's primary specification, with the TWFE results in equation (1) reported alongside it as a point of comparison, not as the leading estimate. Let G_i denote individual i's *group*: the calendar year in which their state's medical marijuana law first takes effect, with $C_i = 1$ denoting the never-treated comparison group (families whose state never legalizes medical marijuana within the sample window). Let $Y_{it}(g)$ denote the potential outcome at time t if first treated in year g, and $Y_{it}(0)$ the potential outcome if never treated. The building block of the CS estimator is the group-time average treatment effect,

$$ATT(g,t) \equiv \mathbb{E}\big[Y_{it}(g) - Y_{it}(0) \,\big|\, G_i = g\big], \qquad t \geq g, \tag{3}$$

the average effect of treatment on the cohort of units first treated in year g, measured at each subsequent calendar year t. Under a parallel-trends assumption holding between each treated group and the never-treated comparison group, and no anticipation of treatment prior to g, $ATT(g,t)$ is identified using only never-treated units as the comparison group, which avoids the forbidden already-treated-versus-already-treated comparisons that generate the TWFE bias. I estimate $ATT(g,t)$ with the regression-adjustment estimator,

$$ATT^{reg}(g,t) = \mathbb{E}\big[Y_t - Y_{g-1} \,\big|\, G_g = 1\big] - \mathbb{E}\big[m_{g,t}(X) \,\big|\, G_g = 1\big], \tag{4}$$

where $m_{g,t}(X) = \mathbb{E}[Y_t - Y_{g-1} \mid X, C = 1]$ is fit by an outcome regression of the change in Y since period $g-1$ on X (household size, as in equations (1) and (2)), estimated among the never-treated comparison units alone.

I aggregate the cohort-time effects $ATT(g,t)$ in two ways. The paper's baseline estimate for each outcome is the "simple" aggregation of Callaway and Sant'Anna (2021), which first averages each group's effect over all post-treatment periods it is observed in, then averages across groups weighted by each group's share of the treated population:

$$\theta_S^O = \sum_g \theta(g)\, P(G = g \mid G \leq T), \qquad \theta(g) = \frac{1}{T - g + 1} \sum_{t=g}^{T} ATT(g,t). \tag{5}$$

Separately, for the small number of outcomes significant under the overall aggregation in equation (5), I construct a dynamic, event-time-indexed aggregation, reported in Section 7.1, that instead averages across groups within a common relative event time $e = t - g$:

$$\theta_D(e) = \sum_g \mathbf{1}\{g + e \leq T\}\, ATT(g,\, g+e)\, P(G = g \mid G + e \leq T). \tag{6}$$

Equation (6) is the CS-estimator analog of the binned event-time coefficients β_k in equation (2), and is what produces the dynamic decomposition figures in Section 7.1.

6 Results

6.1 The Father-Side Outcomes

Table 5 reports the full fifteen-outcome father-side set: the TWFE coefficient on medical legalization from equation (1), and the overall average treatment effect from the state-clustered Callaway-Sant'Anna estimator, for every father-side outcome in every domain. Five outcomes are significant at the 10 percent level under the CS estimator: father repartnering, father depression, father any-mental-health, father criminal justice contact, and father welfare receipt. Two further outcomes (father hours worked and father earnings) are significant under TWFE alone but lose significance under the state-clustered CS estimator, the paper's preferred specification; the corresponding pattern is considerably more pronounced on the mother and child side, where it is discussed alongside the full mother- and child-side outcome set in Section 8.3.

Before turning to these regression-adjusted estimates, it is worth seeing what a completely naive comparison would show. Appendix Table A1 reports the raw, unconditional difference in each outcome's mean between person-years with an active state medical marijuana law and person-years without one; no family fixed effects, no year fixed effects, no adjustment for staggered timing, just a two-sample t-test on the pooled panel. Twenty of the forty-one outcomes are significant at the 10 percent level in this naive comparison, on both the mother and father sides and scattered across

every domain, including several – mother married, mother marijuana use, child obesity, father housing instability – that are not part of any credible causal story in this setting and do not survive either the TWFE or the CS specification in Table 5 or Table 9. This pattern is precisely what motivates the paper's empirical strategy: a raw comparison of legal versus not-legal state-years is confounded by every other characteristic that differs across those state-years, and family fixed effects, together with the Callaway-Sant'Anna correction for staggered adoption, are what separate the five outcomes that survive from the many that do not.

Table 5: **Father Outcome Set: TWFE and Callaway-Sant'Anna Estimates**

Outcome	N	Fam.	TWFE Coef. (SE)	CS ATT (SE)
Health & Mental Health				
Father Depression	15930	959	0.0163 (0.0116)	0.0230* (0.0120)
Father Any Mental Health	15930	959	0.0061 (0.0105)	0.0162* (0.0092)
Substance Use				
Father Used Other Substance	15930	959	0.0021 (0.0113)	0.0004 (0.0036)
Father Used Marijuana	15930	959	0.0045 (0.0039)	0.0019 (0.0031)
Crime & Justice				
Father Convicted/Jailed	15930	959	0.0091 (0.0100)	0.0228* (0.0125)
Labor Market				
Father Hours Worked	15916	958	-0.9930* (0.4972)	-1.8494 (1.6286)
Father Earnings	14672	914	-0.0334** (0.0131)	0.0222 (0.0302)
Father Employed	15930	959	0.0144 (0.0087)	0.0175 (0.0239)
Father on Welfare	15930	959	0.0030 (0.0026)	0.0059* (0.0031)
Relationship & Family Structure				
Father Repartnered	15930	959	-0.0151 (0.0123)	-0.0336** (0.0146)
Intimate Partner Violence				
Father Reports IPV (Bio-Mom)	15930	959	0.0087 (0.0063)	0.0136 (0.0084)
Parenting Behavior				
Father Harsh Parenting	15930	959	-0.0056 (0.0125)	-0.0134 (0.0762)
Father Parenting Stress	15623	952	0.0107 (0.0149)	0.0285 (0.0226)
Father Spanked Child (Past Month)	15292	924	-0.0010 (0.0033)	0.0044 (0.1089)
Housing & Food Insecurity				
Father Housing Instability	15927	959	0.0042 (0.0075)	0.0076 (0.0247)

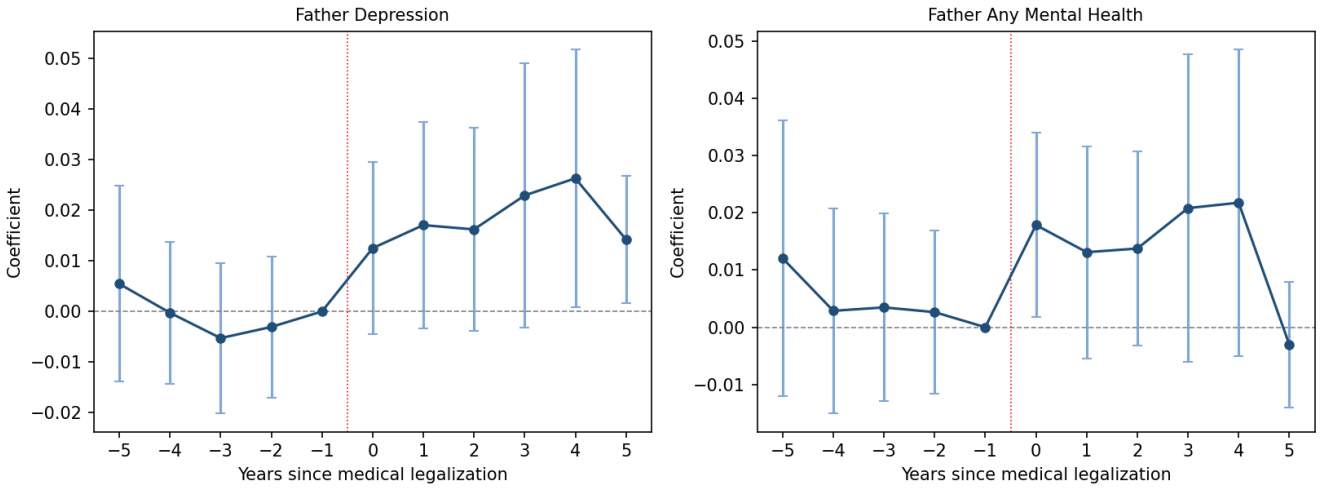
Notes: Sample is the final restricted, imputed father analysis panel (2001–2018), 924–959 fathers depending on the outcome. TWFE column: family and year fixed effects, household size control, standard errors clustered by state. CS column: Callaway and Sant'Anna (2021) estimator, never-treated comparison group, modal state cluster, 999-iteration bootstrap. Both use medical legalization status only. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. The corresponding mother- and child-side outcomes are reported separately in Table 9.

6.2 Health and Mental Health

Two outcomes drive the health and mental health domain's contribution to the paper's headline result. Father depression (a conservative CIDI-based caseness measure) rises by 2.3 percentage points under medical legalization (CS ATT = 0.023, $p = 0.054$), and a broader father any-mental-health measure (combining conservative depression caseness with a liberal depression-or-anxiety measure) rises by 1.6 percentage points (CS ATT = 0.016, $p = 0.079$). Neither is significant under plain TWFE ($p = 0.170$ and $p = 0.561$, respectively), which is consistent with the pattern documented throughout the paper of the CS estimator detecting effects that a pooled TWFE coefficient, averaged over a staggered and heterogeneous adoption path, does not.

Figure 4 plots the TWFE event-study paths for both retained father-side health and mental health outcomes. The corresponding mother- and child-side measures in this domain (mother depression, mother any-mental-health, child any-mental-health, child hospitalized overnight, and child obesity) are addressed as part of the mother- and child-side specificity check in Section 8.3, with the event-study paths reported in Appendix Figure A2.

Figure 4: **Event Studies for Father outcomes: Health and Mental Health domain.**



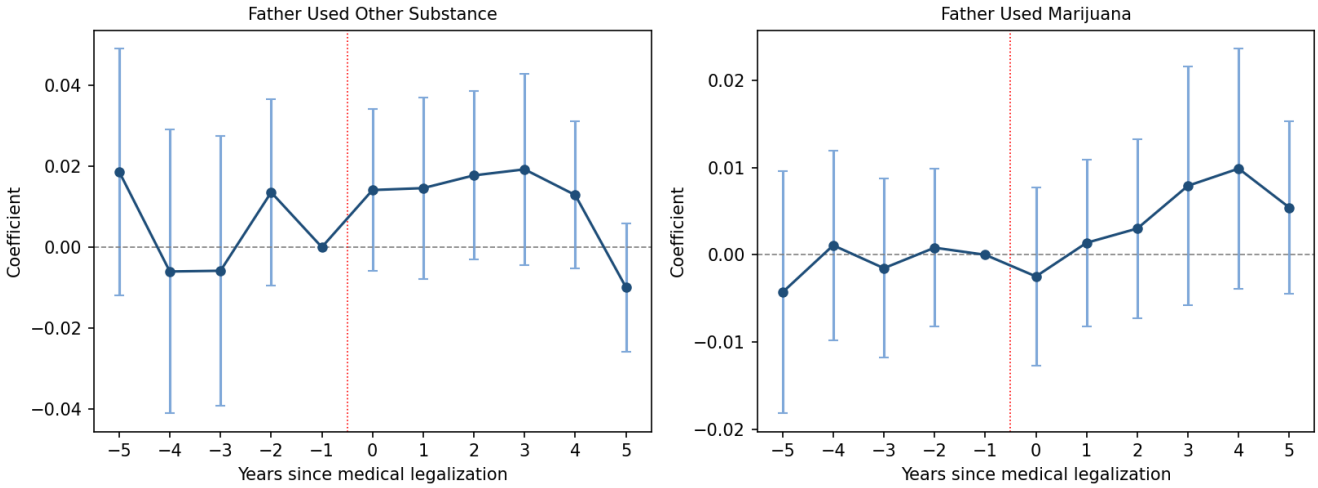
Notes: Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

6.3 Substance Use and Crime

Neither retained father-side substance-use outcome is significant under the state-clustered CS estimator: father marijuana use and father use of other substances (heavy alcohol, tobacco, or other drug use) are both null throughout. Substantively, this null pattern is consistent with a hypothesis raised early in this project's development: families in the Fragile Families sample are drawn from

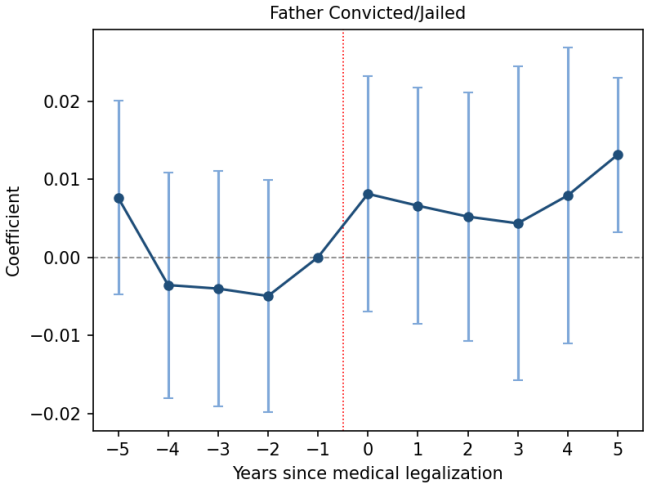
a population already characterized by elevated rates of substance use at baseline, so a change in the legal status of one specific substance may do relatively little to move average use rates within a population that already uses at high baseline rates regardless of legal status. Figure 5 plots the event-study paths; the corresponding mother- and child-side substance-use outcomes are discussed in Section 8.3 and plotted in Appendix Figure A3.

Figure 5: **Event Studies for Father outcomes: Substance Use domain.**



Notes: Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

Figure 6: **Event Studies for Father outcomes: Crime and Justice domain**



Notes: Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

Father criminal justice contact, which is a combined measure of any conviction or jail/incarceration spell, rises by 2.3 percentage points under the state-clustered CS estimator (ATT = 0.023, p =

0.069), while the corresponding TWFE coefficient is small and insignificant ($p = 0.371$). Figure 6 plots the event-study path; as discussed in Section 7.1, the dynamic Callaway-Sant'Anna decomposition for father criminal justice contact shows a pattern of gradually accumulating, same-signed effects instead of any single sharply significant relative period, and a cluster of near-identical estimates at the far right of the event window that is a known artifact of sparse cohort-time cells, not a genuine late-horizon reversal. The corresponding mother-side outcome, mother criminal justice contact, is addressed in Section 8.3 and plotted in Appendix Figure A4.

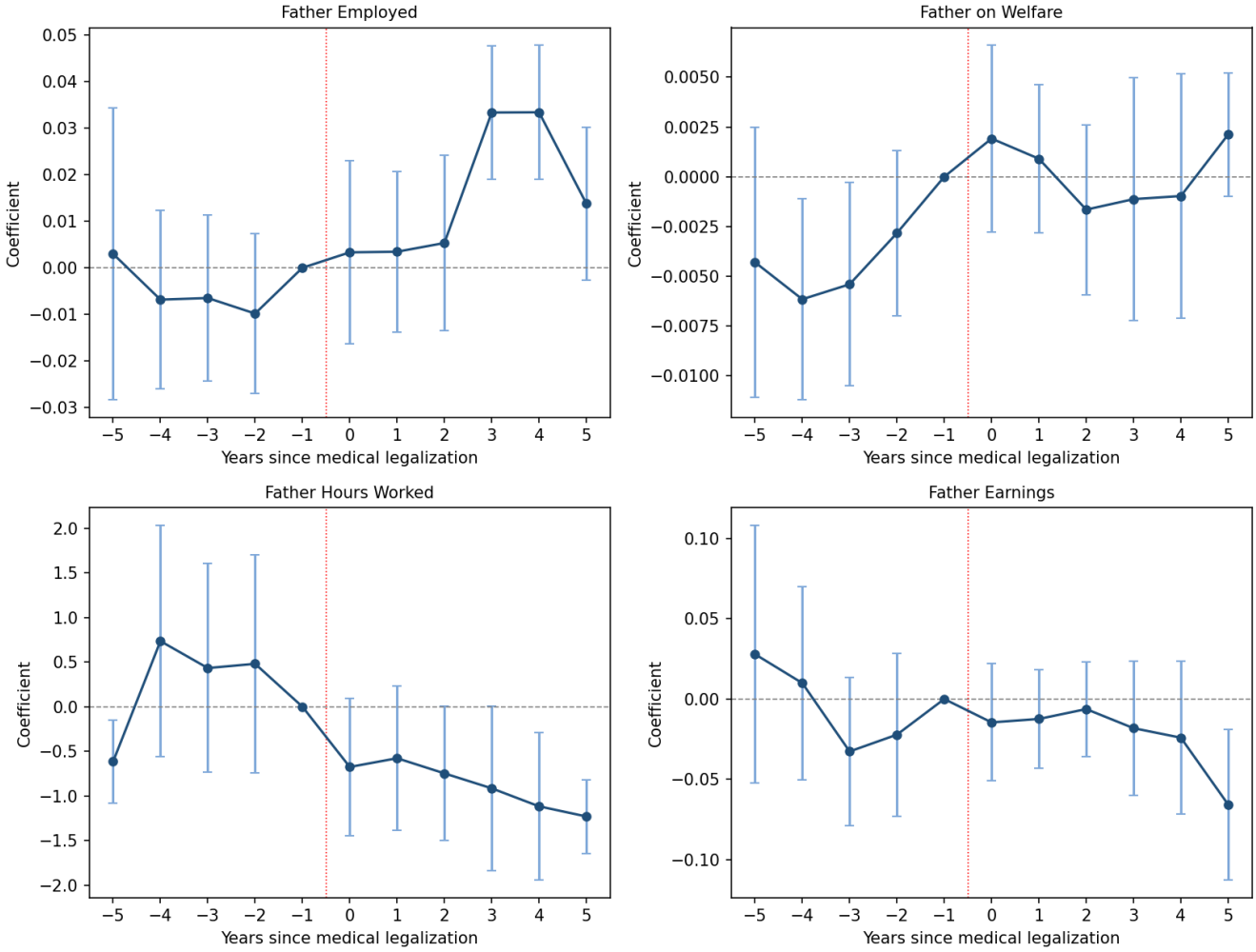
6.4 Labor Market

Father welfare receipt rises by 0.6 percentage points under the state-clustered estimator (CS ATT = 0.006, $p = 0.060$), the fifth and final outcome that clears the paper's 10 percent significance threshold under CS. This occurs without a corresponding significant change in father employment, hours worked, or earnings bracket, which is worth noting explicitly: the welfare result does not appear to be mechanically driven by a father working less or earning less, at least not by an amount the other labor-market measures here can detect. Figure 7 plots all four retained father-side labor market outcomes. Mother welfare receipt, mother hours worked, and mother earnings bracket all show TWFE-only signals that do not survive the CS check, reinforcing the domain's overall father-specific pattern; these mother-side outcomes are discussed in Section 8.3 and plotted in Appendix Figure A5.

6.5 Relationship and Family Structure, and Intimate Partner Violence

Father repartnering is the paper's single largest and most robust finding. Fathers whose state legalizes medical marijuana are 3.4 percentage points less likely to have repartnered with a new partner following a relationship dissolution with the child's mother (CS ATT = -0.034, $p = 0.022$), the only outcome in the entire forty-one-outcome set significant at the conventional 5 percent threshold under the state-clustered estimator. The corresponding TWFE coefficient is smaller and insignificant (coefficient = -0.015, $p = 0.230$), again consistent with the broader pattern of the CS estimator surfacing effects that a pooled TWFE specification, run across a staggered adoption path with heterogeneous cohort sizes, does not. Section 7.1 presents the dynamic Callaway-Sant'Anna decomposition of this effect specifically, which is the most credible single event-study path in the paper. Father-reported IPV from the biological mother also describes the father's relationship with the child's mother. IPV is not significant under CS, though it is the closest of the paper's null father-side results ($p = 0.104$). Figure 8 plots both event-study paths side by side. Mother repartnering, mother marital status, the child's living arrangement with the biological father, and the mother-reported IPV outcomes are discussed as part of the mother- and child-side specificity

Figure 7: **Event Studies for Father outcomes: Labor Market domain.**



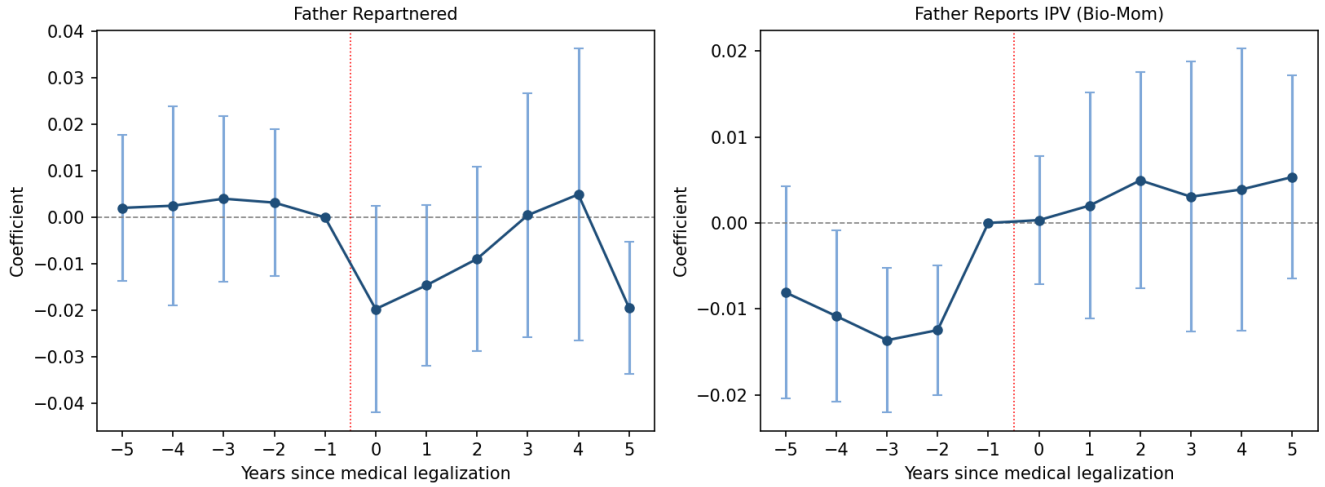
Notes: Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

check in Section 8.3 and plotted in Appendix Figures A6 and A7.

6.6 Parenting Behavior, and Housing and Food Insecurity

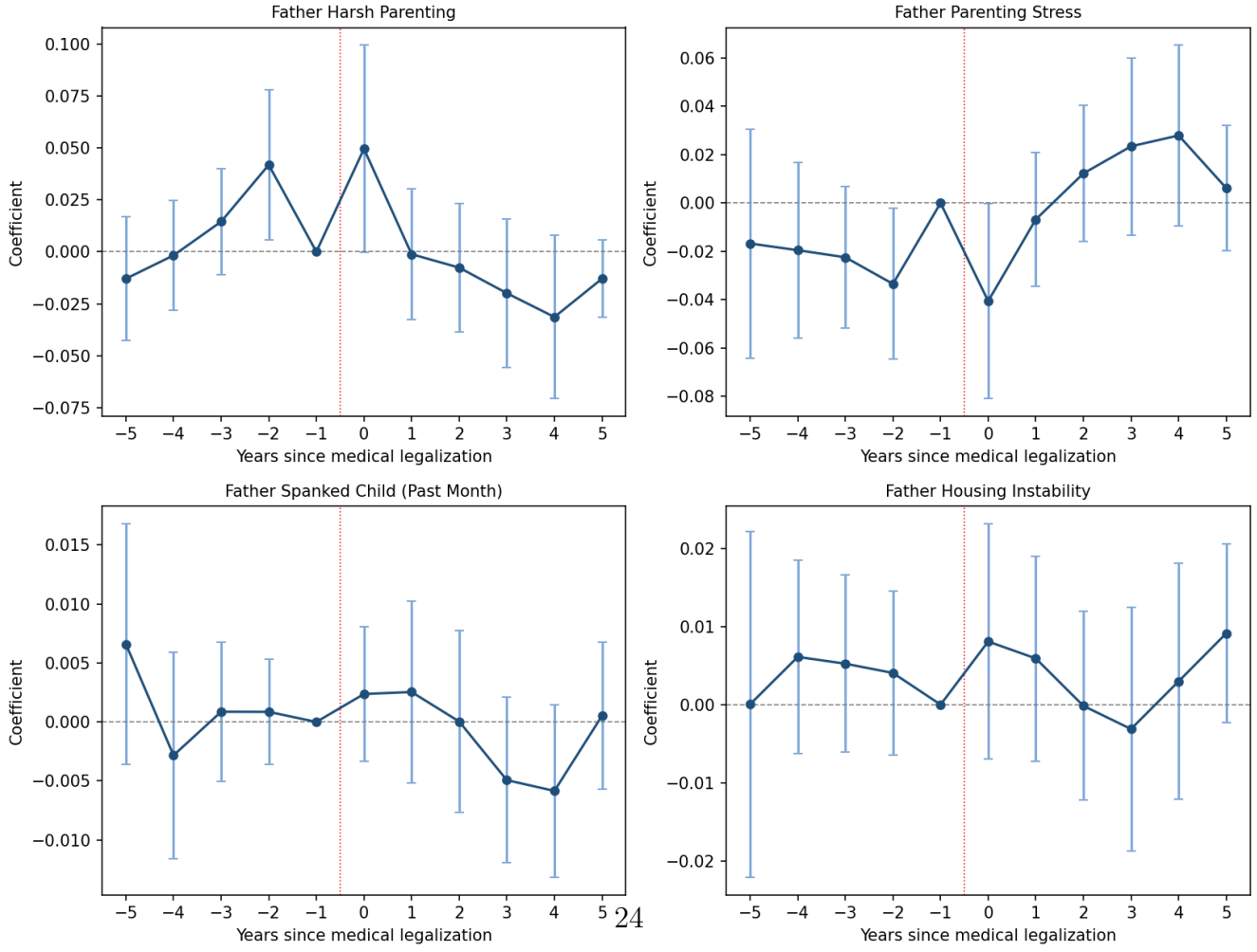
The remaining two domains, parenting behavior and housing and food insecurity, are uniformly null for fathers under the state-clustered estimator. None of the three retained father-side parenting-behavior outcomes (harsh parenting, parenting stress, and spanking) are significant under CS. Father housing instability is also not significant under CS. Figure 9 plots the event-study paths for all four father outcomes across these two domains. The corresponding mother- and child-side outcomes in these domains are discussed in Section 8.3 and plotted in Appendix Figures A8 and A9.

Figure 8: **Event Studies for Father outcomes: Relationship/Family Structure and Intimate Partner Violence domains.**



Notes: Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

Figure 9: **Event Studies for Father outcomes: Parenting Behavior and Housing/Food insecurity domains.**



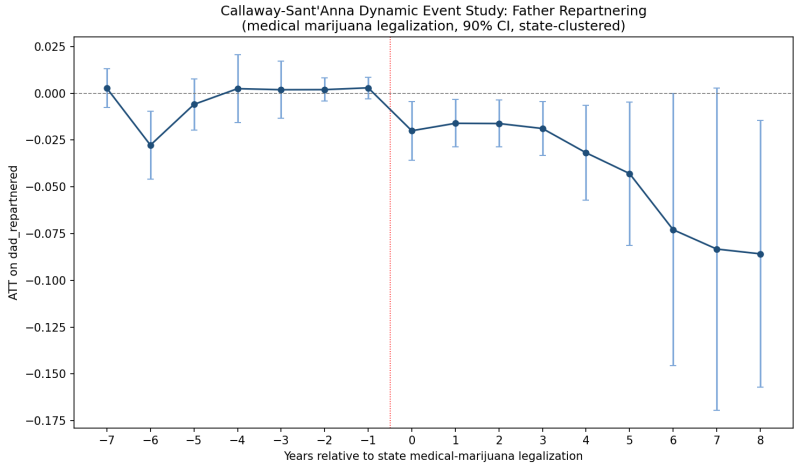
Notes: Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

7 Robustness Check

7.1 Dynamic Event-Study Confirmation

The event-study figures in Section 6 are built from the TWFE binned-dummy specification in equation (2), not from the Callaway-Sant'Anna estimator's own group-time decomposition. The distinction matters: the CS point estimates reported throughout the paper are not subject to the TWFE staggered-adoption bias discussed in Section 5.2, but the accompanying pre-trend visualizations, as plotted, technically are. To confirm that the paper's central results are not an artifact of this mismatch, I re-estimate the dynamic event-time path directly from the CS estimator's own group-time average treatment effects, aggregated by relative event time instead of collapsed to a single overall effect, for all five outcomes significant under the CS overall aggregation.

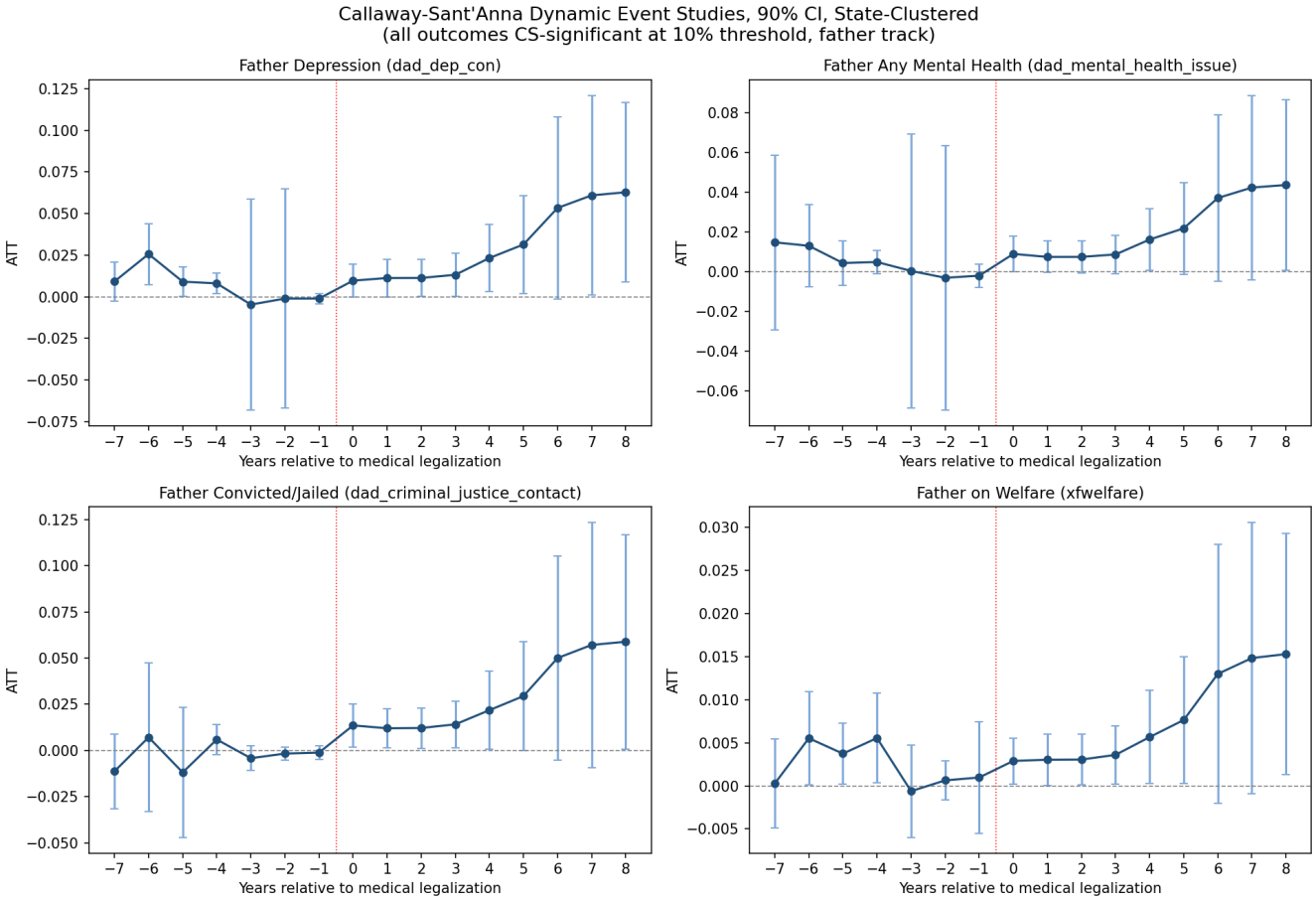
Figure 10: **Callaway-Sant'Anna dynamic event-time decomposition: father repartnering.**



Notes: 90 percent confidence intervals, state-clustered.

Figure 10 plots this dynamic decomposition for father repartnering, the paper's anchor finding. The pattern is close to a textbook credible event study: of the pre-treatment relative periods shown, none show a systematic trend away from zero, and the one or two periods that cross the 90 percent simultaneous confidence band do so in isolation, consistent with the false-discovery rate one would expect from testing this many relative periods at a 10 percent threshold by chance, and not with a genuine pre-trend. The post-treatment path, by contrast, is monotonic: the estimated effect grows from roughly -0.02 at the year of legalization to -0.12 by eight to ten years after legalization, with the later periods individually crossing the 90 percent confidence band. Relative periods beyond ten years post-treatment are excluded from the figure; in the raw estimator output, these periods return identical point estimates and standard errors repeated across three consecutive relative-time

Figure 11: **Callaway-Sant'Anna dynamic event-time decomposition: remaining four CS-significant outcomes.**



Notes: 90 percent confidence intervals, state-clustered.

bins, a signature of a cohort-time cell with too few observations to be separately identified, not a genuine effect, and are dropped instead of reported.

Figure 11 repeats the exercise for the remaining four CS-significant outcomes. Father welfare receipt shows a similarly clean pattern, with individual relative periods at eight to ten years post-treatment crossing the confidence band. Father depression, father any-mental-health, and father criminal justice contact show a more gradual signature: a small, consistently same-signed, monotonically growing effect across the post-treatment window, but no single relative period that is individually significant on its own. I read this as weaker, but not contradictory, evidence: the overall CS aggregation for these three outcomes is significant because it is picking up the cumulative weight of a persistent small drift across many post-treatment years, not a single large, sharply estimated jump. The pattern is real and directionally consistent, but a reader should weight it as suggestive, not as strong as the father repartnering and father welfare results.

7.2 Recreational Legalization

Section 4.3 notes that recreational marijuana legalization is perfectly nested within medical legalization in this sample, every recreationally-legal state-year is also medically-legal, and that only six states (California, Colorado, the District of Columbia, Massachusetts, Michigan, and Washington) ever adopt recreational legalization within the 2001–2018 window, which is why the paper's primary specification uses medical legalization throughout. This section reports what happens when recreational legalization is used as the treatment instead, on the same samples, controls, fixed effects, and state-clustered standard errors as the headline results (Tables 5 and 9).

The state-clustered Callaway-Sant'Anna estimator could not be reliably estimated for the recreational treatment. Recreational adoption timing in this sample collapses to four calendar cohorts, two of which are close to unusable on their own (three fathers first observed recreational-legal in 2012, one in 2014) and two of workable size (130 fathers first observed in 2016, 68 in 2018). With this few distinct adoption cohorts, the bootstrap-based aggregation this paper uses for every other CS estimate returns a degenerate zero point estimate with an undefined standard error, for every outcome, regardless of aggregation type (simple, cohort, or event) or estimation method (regression-adjustment or doubly robust): confirmed to be a property of the recreational-adoption timing, since the identical pipeline reproduces Table 5 exactly when applied to medical legalization on the same underlying data. Given this, the table below reports TWFE estimates only.

Table 6: **Father Outcome Set, Recreational Legalization: TWFE Estimates**

Outcome	N	Fam.	TWFE Coef. (SE)
Health & Mental Health			
Father Depression	15930	959	-0.0017 (0.0072)
Father Any Mental Health	15930	959	-0.0055 (0.0091)
Substance Use			
Father Used Other Substance	15930	959	-0.0103* (0.0055)
Father Used Marijuana	15930	959	-0.0108 (0.0069)
Crime & Justice			
Father Convicted/Jailed	15930	959	0.0125 (0.0075)
Labor Market			
Father Hours Worked	15916	958	-0.5644 (0.3724)
Father Earnings	14672	914	-0.0291 (0.0248)
Father Employed	15930	959	-0.0044 (0.0092)
Father on Welfare	15930	959	0.0061** (0.0022)
Relationship & Family Structure			
Father Repartnered	15930	959	-0.0157** (0.0071)
Intimate Partner Violence			
Father Reports IPV (Bio-Mom)	15930	959	0.0067 (0.0062)

(continued)

Outcome	N	Fam.	TWFE Coef. (SE)
Parenting Behavior			
Father Harsh Parenting	15930	959	-0.0035 (0.0172)
Father Parenting Stress	15623	952	0.0026 (0.0174)
Father Spanked Child (Past Month)	15292	924	0.0021 (0.0042)
Housing & Food Insecurity			
Father Housing Instability	15927	959	-0.0024 (0.0073)

Notes: Treatment is recreational (rather than medical) marijuana legalization; same sample, controls, fixed effects, and state-clustered standard errors as Table 5. Callaway-Sant'Anna estimates are not reported; see Section 7.2 for why. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Two of the five father outcomes significant under medical legalization in Table 5 replicate directionally and are themselves significant under this TWFE recreational specification. Father welfare receipt is nearly identical in magnitude (coefficient = 0.0061, $p = 0.010$, compared with a medical CS ATT of 0.0059), and father repartnering points the same direction at roughly half the medical magnitude (coefficient = -0.0157, $p = 0.035$, compared with a medical CS ATT of -0.0336). Father criminal justice contact points the same direction but falls just short of the 10 percent threshold (coefficient = 0.0125, $p = 0.102$). Father depression and father any-mental-health are not significant here ($p = 0.811$ and $p = 0.554$). The corresponding mother- and child-side outcome set under recreational legalization is reported in Appendix Table A4 and discussed alongside the paper's mother- and child-side specificity check in Section 8.3.

This check is consistent with, but cannot independently confirm, the paper's medical-legalization findings: two of the five significant father-side results replicate closely under an alternative, more restrictive definition of legal access, and a third points the same direction without clearing the threshold. That is precisely the limitation anticipated in Section 4.3, which is why the paper's primary specification relies on medical, not recreational, legalization throughout.

8 Mechanism and Heterogeneity

8.1 Mechanism: Direct Effect or Decriminalization Channel?

A natural question about the father mental health result is whether it operates through reduced legal exposure: fathers experience less criminal justice contact once marijuana possession is decriminalized in practice, and this in turn improves mental health, or through some more direct channel independent of the legal system. I test this by re-estimating the father any-mental-health

specification with father criminal justice contact added as a statistical control, and comparing the coefficient on medical legalization with and without this control. Substantial attenuation of the legalization coefficient once criminal justice contact is added would point toward decriminalization as the operative channel; the absence of attenuation would point toward a more direct effect.

Table 7 reports the result under both the TWFE and CS specifications. Under TWFE, the coefficient attenuates by only 2.1 percent (from 0.0061 to 0.0060) when the criminal justice control is added, and the criminal justice contact variable itself is not significantly associated with mental health in this regression ($p = 0.661$). Under the CS specification, the ATT does not attenuate at all; if anything, it rises slightly (from 0.0162 to 0.0169). Neither result is consistent with a decriminalization channel; the pattern instead points to legalization affecting father mental health directly – plausibly through channels such as changed consumption patterns, changed social or family circumstances, or the direct pharmacological or psychological effects of increased access – and not through the criminal justice system, which means the mental health and criminal justice contact results in this paper should be read as two separate findings, not as one finding double-counted as two.

Table 7: **Mechanism Check: Father Any Mental Health, With and Without Criminal Justice Contact Control**

	Coefficient/ATT	(SE)
TWFE specification		
Without CJ contact control	0.0061	(0.0105)
With CJ contact control	0.0060	(0.0103)
Callaway-Sant'Anna specification		
Without CJ contact control	0.0162	(0.0092)
With CJ contact control	0.0169	(0.0094)

Notes: TWFE attenuation: 2.1%. CS attenuation: −4.5% (ATT rises slightly). Neither specification shows the substantial attenuation that would indicate a decriminalization channel. $N = 15{,}930$ father-years, 959 fathers, both specifications.

8.2 Heterogeneity by Pre-Legalization Marijuana Use

If the father-side effects documented in this paper are driven by legalization-induced changes in fathers' own marijuana use, the effects should be concentrated among fathers who were not already using marijuana before their state legalized, since a father who was already using has, by construction, less scope for a legalization-induced change in use. If instead the effects are driven by something correlated with legalization but unrelated to individual use, for instance a state-level

confounder, then there is no obvious reason the effect should differ by a father's own pre-legalization use.

I test this by splitting the sample on each father's marijuana-use status in his first observed survey wave (a pre-determined characteristic, not a post-treatment outcome, since it is measured before any exposure to legalization for the large majority of the sample) and re-estimating the CS overall ATT separately within each subgroup, for all five CS-significant outcomes. Table 8 reports the results. Among the 885 fathers who report no marijuana use at baseline, all five outcomes move in the same direction as the pooled estimate, and three remain significant at the 10 percent level on their own (father repartnering, $p = 0.046$; father any-mental-health, $p = 0.078$; father welfare, $p = 0.037$), with father depression close behind ($p = 0.084$). Among the 74 fathers who report marijuana use at baseline, point estimates are smaller in magnitude and, for three of the five outcomes, reverse sign, though this subgroup is small enough that a null result there is difficult to distinguish from a genuinely small or zero effect.

Table 8: **Heterogeneity by Baseline (Pre-Legalization) Father Marijuana Use**

	No Baseline Use ($N = 885$)		Baseline Use ($N = 74$)	
Outcome	CS ATT (SE)	p	CS ATT (SE)	p
Father Repartnered	-0.0345** (0.0173)	0.046	-0.0273 (0.0283)	0.334
Father Depression	0.0250* (0.0145)	0.084	-0.0117 (0.0668)	0.861
Father Any Mental Health	0.0210* (0.0120)	0.078	-0.0964 (0.0948)	0.309
Father Convicted/Jailed	0.0257 (0.0203)	0.205	0.0130 (0.0146)	0.374
Father on Welfare	0.0092** (0.0044)	0.037	-0.0373 (0.0363)	0.304

Notes: Baseline marijuana use measured in each father's first observed survey wave. State-clustered Callaway-Sant'Anna estimator, 90% CI (10% significance threshold). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The substantive reading is that these effects are not concentrated among fathers who would have used marijuana regardless of its legal status; they are concentrated among fathers whose relationship to the substance plausibly changed because of the policy change itself. That matches what one would expect if legalization is operating through an extensive-margin channel: inducing new use, or a change in the circumstances surrounding use, among fathers who were not already using, and not through a channel correlated with legalization timing but independent of individual behavior.

8.3 Specificity Check: Mothers and Children

The results above showing significant father-side effects clustered in five outcomes, and null results everywhere else, raises an obvious question: is this a genuine, father-specific effect of legalization,

or is it what one would expect from testing forty-one outcomes and reporting whichever happen to cross a significance threshold? I address this question directly by treating the mother- and child-side outcomes as a built-in specificity check. If medical marijuana legalization were operating as a generic household-level or state-level confound; correlated, for instance, with a state's broader economic trajectory or with some unobserved shift in state political or social conditions around the same time; there is no obvious reason that confound would be selective to fathers. Mothers and children live in the same households, under the same state policy environment, observed over the same panel window, with the same family and year fixed effects and the same estimator applied to their outcomes. If the father-side results were an artifact of a broader confound, the mother- and child-side outcomes should pick up at least some of it.

Of twenty-one mother-side outcomes and five child-side outcomes, twenty-six in total, spanning every domain in the outcome set, not one is significant under the state-clustered CS estimator at the 10 percent level. The closest is mother housing instability at $p = 0.191$, followed by mother reports of IPV from a new partner at $p = 0.231$ and child obesity at $p = 0.261$; none is close to any conventional significance threshold. The result is genuinely a null set across an outcome set exactly as broad as the one that produced five significant findings on the father side. Table 9 reports the full mother- and child-side outcome set in Table 5. The domain-by-domain event-study paths underlying these mother- and child-side estimates are reported in Appendix Figures A2 through A9.

Table 9: **Mother- and Child-Side Outcome Set: TWFE and Callaway-Sant'Anna Estimates**

Outcome	N	Fam.	TWFE Coef. (SE)	CS ATT (SE)
Health & Mental Health				
Mother Depression	29060	1615	-0.0118 (0.0099)	-0.0118 (0.0176)
Mother Any Mental Health	29060	1615	-0.0037 (0.0071)	0.0041 (0.0172)
Child Any Mental Health	29060	1615	-0.0073 (0.0102)	0.0117 (0.0264)
Child Hospitalized Overnight	29060	1615	-0.0123* (0.0062)	-0.0181 (0.0226)
Child Obesity	21276	1613	-0.0273** (0.0130)	-0.0319 (0.0284)
Substance Use				
Child Used Other Substance	16607	1615	-0.0112 (0.0181)	-0.0184 (0.0258)
Child Used Marijuana	16588	1615	-0.0042 (0.0192)	-0.0033 (0.0801)
Mother Used Other Substance	29060	1615	0.0106 (0.0147)	0.0271 (0.0311)
Mother Used Marijuana	29060	1615	0.0109** (0.0051)	0.0111 (0.0157)
Crime & Justice				
Mother Convicted/Jailed	29060	1615	-0.0086* (0.0051)	-0.0091 (0.0183)
Labor Market				
Mother Earnings	23436	1518	-0.1727** (0.0825)	-0.1640 (0.2089)
Mother Employed	29060	1615	-0.0085 (0.0163)	-0.0207 (0.2854)
Mother Hours Worked	28756	1610	-0.8557** (0.3487)	-0.8653 (1.6538)
Mother on Welfare	29060	1615	0.0262** (0.0128)	0.0242 (0.0299)
Relationship & Family Structure				
Mother Married	29060	1615	0.0237*** (0.0087)	0.0215 (0.0215)

(continued)

Outcome	N	Fam.	TWFE Coef. (SE)	CS ATT (SE)
Mother Repartnered	29060	1615	-0.0163 (0.0154)	-0.0019 (0.0444)
Child Living with Biological Father	29060	1615	0.0403*** (0.0105)	0.0334 (0.0487)
Intimate Partner Violence				
Mother Reports IPV (New Partner)	10192	714	0.0026 (0.0043)	0.0041 (0.0035)
Mother Reports IPV (Biological Father)	28975	1611	0.0029 (0.0045)	-0.0070 (0.0117)
Mother Reports IPV (Bio-Dad/New Partner)	29060	1615	0.0045 (0.0041)	-0.0049 (0.0138)
Parenting Behavior				
Mother Harsh Parenting	29060	1615	-0.0230 (0.0198)	-0.0003 (0.1301)
Mother Parenting Stress	29056	1615	0.0008 (0.0242)	-0.0134 (0.0861)
Mother Spanked Child (Past Month)	29040	1614	-0.0130** (0.0064)	-0.0005 (0.0559)
Housing & Food Insecurity				
Mother Housing Instability	29060	1615	-0.0011 (0.0090)	0.0239 (0.0183)
Mother Food Insecure (Adult)	24992	1545	0.0000 (0.0047)	0.0007 (0.0017)
Mother Food Insecure (Child)	24992	1545	-0.0001 (0.0019)	0.0011 (0.0009)

Notes: Sample is the final restricted, imputed mother analysis panel (2001–2018), 714–1615 mothers/children depending on the outcome (Mother Reports IPV (New Partner) is asked only of mothers who report a new partner, a structurally smaller conditional population rather than a missingness problem). TWFE column: family and year fixed effects, household size control, standard errors clustered by state. CS column: Callaway and Sant'Anna (2021) estimator, never-treated comparison group, modal state cluster, 999-iteration bootstrap. Both use medical legalization status only. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. None of the 26 outcomes below is significant under CS at the 10% level; see Table 5 for the corresponding father-side outcomes.

The gap between TWFE and CS is considerably wider on the mother and child side than it is for fathers. Ten of the twenty-six outcomes are significant under plain TWFE alone: mother convicted/jailed, mother marijuana use, mother earnings, mother hours worked, mother on welfare, mother married, mother spanked child, child living with the biological father, child hospitalized overnight, and child obesity. Every one of these loses significance under the state-clustered CS estimator. This pattern is strong evidence that the father-side effects are not an artifact of the outcome set's breadth. Whatever medical marijuana legalization is doing in this sample, it is doing it through a channel specific to fathers' own circumstances, not through a mechanism that would be expected to leave a mark on the household as a whole.

As a further check, Appendix Table A4 repeats this mother- and child-side outcome set under recreational, not medical, legalization (Section 7.2). Six of the twenty-six outcomes are nominally significant under TWFE, but the state-clustered CS estimator that filters out the ten similarly scattered TWFE-only mother- and child-side findings under medical legalization above could not be reliably estimated for the sparser recreational treatment, so that filter is not available here. Given how aggressively CS eliminates TWFE-only significance elsewhere in this outcome set, these six coefficients are best read as a further illustration of TWFE's tendency to overstate significance under staggered timing, not as evidence against the specificity argument above.

9 Discussion

The results in this paper fit together into a single account of what medical marijuana legalization is doing in the fathers. First, father repartnering falls, father mental health worsens, father criminal justice contact rises, and father welfare receipt rises, while the identical set of outcomes shows nothing at all for mothers and children in the same households. Next, the mental health effect does not run through the criminal justice channel: adding father criminal justice contact as a control does not attenuate the mental health coefficient under either specification, which rules out the most obvious confounding story linking two of the five significant outcomes to each other. Finally, the effects are concentrated among fathers who had not used marijuana before their state legalized, which is the pattern one would expect if legalization is inducing new use, or a change in the circumstances surrounding use, among a specific subset of fathers, and not some broader confound correlated with legalization timing.

The most obvious reading is that state medical marijuana legalization changes some fathers' relationship to the substance: plausibly inducing use among fathers who would not otherwise have used, or changing the social and legal context surrounding use for those who might have used regardless; and that this change carries consequences for the father's own mental health, his exposure to the criminal justice system, his economic circumstances, and, ultimately, his likelihood of forming a new partnership after a relationship with the child's mother ends. The direction of the mental health result runs counter to a simple self-medication story, in which increased legal access to marijuana would be expected to improve, not worsen, measured mental health; it is more consistent with an account in which increased use itself, or the social and family disruption associated with a change in use, is the more relevant channel.

The father repartnering result deserves particular attention because it is both the largest single effect in the paper and the outcome for which the paper's identification is cleanest: unlike welfare receipt or criminal justice contact, which could in principle be affected by state-level economic or enforcement conditions correlated with legalization timing, a father's likelihood of repartnering is a highly individual, within-household outcome that is hard to attribute to anything other than something happening to that specific father. Because mother repartnering shows no corresponding effect, the result is difficult to explain as a shock to the relationship or the household as a unit; it looks instead like a shock to the father's own capacity or inclination to form a new partnership, which is consistent with the paper's broader finding that his mental health and legal circumstances are also deteriorating over the same post-legalization window.

This paper's findings should be read as a complement to the mixed literature on marijuana legalization and family or relationship outcomes reviewed in Section 2, not as an attempt to settle it. The population studied here, largely unmarried, economically disadvantaged parents in the Fragile

Families cohort, is not representative of the general population studied in state-level administrative data, and the paper's contribution is precisely that it isolates a specific, vulnerable subpopulation for whom the effects of legalization may differ from the population average. The near-total absence of a detectable effect on mothers and children in the same households is, if anything, the paper's most important finding for family policy: it suggests that whatever risk medical marijuana legalization poses to family wellbeing in this population, that risk is not evenly distributed across household members, and policy or clinical attention aimed at mitigating it should be aimed specifically at fathers, not at the family unit as a whole.

10 Conclusion

This paper uses twenty years of panel data on a cohort of largely unmarried, economically disadvantaged parents to ask whether state medical marijuana legalization affected parents' wellbeing, and whether it affected mothers and fathers differently. Across a set of forty-one outcomes spanning mental health, substance use, criminal justice contact, labor market activity, relationship and family structure, intimate partner violence, parenting behavior, and housing and food insecurity, the answer is a sharp asymmetry: five of fifteen father-side outcomes are significant under a state-clustered staggered-adoption estimator, and zero of twenty-six mother- and child-side outcomes are. Fathers become less likely to repartner after a relationship ends, more likely to have criminal justice contact, more likely to show signs of poor mental health, and more likely to receive welfare. A mechanism check rules out decriminalization as the channel linking the mental health and criminal justice results, and a heterogeneity check shows the effects are concentrated among fathers who had not used marijuana before legalization, consistent with an extensive-margin behavioral channel and not a broader confound.

The father repartnering result is this paper's central and most robust contribution: a 3.4 percentage point decline in the likelihood of repartnering after a relationship dissolution, confirmed under a fully dynamic event-time decomposition with a flat pre-trend and a monotonically growing post-treatment effect. Read alongside the paper's other father-side findings and its uniformly null mother- and child-side outcome set, the evidence points toward a specific, policy-relevant conclusion: medical marijuana legalization appears to carry real costs for a subset of fathers in economically fragile households, operating through a channel tied to the fathers' own circumstances, not to the household as a whole, and future work on the family consequences of drug policy should treat parents as distinct subjects of study, not interchangeable ones.

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Appendix

A1 Difference in Means

Table A1: Unconditional Difference in Outcome Means, Never-Legal vs. Ever/Currently-Legal Person-Years

Outcome	Mean (Not Legal)	Mean (Legal)	Difference
Health & Mental Health			
Mother Depression	0.1156	0.1115	-0.0041
Father Depression	0.0966	0.0961	-0.0005
Mother Any Mental Health	0.1875	0.1901	0.0025
Father Any Mental Health	0.1503	0.1485	-0.0019
Child Any Mental Health	0.0769	0.0812	0.0043
Child Hospitalized Overnight	0.0303	0.0256	-0.0047**
Child Obesity	0.3926	0.4280	0.0354***
Substance Use			
Child Used Other Substance	0.0780	0.0975	0.0195***
Child Used Marijuana	0.0547	0.1094	0.0547***
Father Used Other Substance	0.5572	0.5579	0.0007
Mother Used Other Substance	0.3777	0.3595	-0.0183***
Mother Used Marijuana	0.0277	0.0385	0.0108***
Father Used Marijuana	0.1030	0.1121	0.0090
Crime & Justice			
Mother Convicted/Jailed	0.0622	0.0604	-0.0018
Father Convicted/Jailed	0.3875	0.3845	-0.0029
Labor Market			
Mother Earnings	4.1392	4.6158	0.4765***
Father Hours Worked	44.4649	43.2396	-1.2253***
Father Earnings	5.7979	5.8663	0.0684
Father Employed	0.7967	0.7947	-0.0020
Mother Employed	0.6543	0.6615	0.0072
Mother Hours Worked	36.6190	36.6866	0.0676
Mother on Welfare	0.1624	0.1565	-0.0059
Father on Welfare	0.0164	0.0113	-0.0051***
Relationship & Family Structure			
Mother Married	0.3023	0.3332	0.0309***
Mother Repartnered	0.1711	0.1889	0.0177***
Father Repartnered	0.0621	0.0221	-0.0401***
Child Living with Biological Father	0.4248	0.4215	-0.0033
Intimate Partner Violence			
Mother Reports IPV (New Partner)	0.0139	0.0168	0.0030
Mother Reports IPV (Biological Father)	0.1022	0.1068	0.0046
Mother Reports IPV (Bio-Dad/New Partner)	0.1054	0.1097	0.0044
Father Reports IPV (Bio-Mom)	0.1198	0.1261	0.0064
Parenting Behavior			
Mother Harsh Parenting	0.8451	0.8325	-0.0126**
Father Harsh Parenting	0.5632	0.7118	0.1486***
Mother Parenting Stress	1.9811	1.9975	0.0165*
Father Parenting Stress	1.9368	1.9095	-0.0273**
Mother Spanked Child (Past Month)	0.5122	0.4545	-0.0577***

(continued)

Outcome	Mean (Not Legal)	Mean (Legal)	Difference
Father Spanked Child (Past Month)	0.3610	0.3358	-0.0251***
Housing & Food Insecurity			
Mother Housing Instability	0.0595	0.0665	0.0071**
Mother Food Insecure (Adult)	0.1867	0.1898	0.0032
Mother Food Insecure (Child)	0.0685	0.0734	0.0049
Father Housing Instability	0.0618	0.0722	0.0105**

Notes: Pooled person-year comparison across the full estimation panel (2001–2018); no family, year, or state fixed effects, and no adjustment for staggered treatment timing. *Not Legal* and *Legal* refer to the parent's state-year medical marijuana legalization status in that specific survey year, not a family-level ever/never classification. Two-sample Welch t-test. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. This table is descriptive only; see Table 5 (father outcomes) and Table 9 (mother- and child-side outcomes) for the paper's family-fixed-effects TWFE and Callaway-Sant'Anna estimates.

A2 Common-Sample Robustness Check

The father panel underlying Table 5 ranges from 924 to 959 fathers depending on the outcome, and the mother- and child-side panel underlying Table 9 ranges from 714 to 1,615 mothers and children, because each outcome's regression sample is restricted to families with a non-missing value on that specific outcome (Section 4.2). A natural concern is whether the paper's headline pattern; five significant father outcomes, zero significant mother- and child-side outcomes; is sensitive to this outcome-specific sample composition, instead of reflecting a stable pattern within a single, fixed set of families. I address this directly by re-estimating both outcome sets on a common sample: the intersection of families with non-missing data on every outcome in the relevant set.

For fathers, intersecting the non-missing samples across all fifteen outcomes in Table 5 leaves 881 fathers, roughly 92 percent of the maximum 959-father sample used in the headline results. For the mother- and child-side outcome set, I treat Mother Reports IPV (New Partner) as the same structural special case documented in Table 9's notes: it is asked only of mothers who report a new partner, a smaller conditional population, not an ordinary missing-data problem; and intersect the remaining twenty-five outcomes, leaving 1,444 mothers and children, 89 percent of the maximum 1,615-family sample; Mother Reports IPV (New Partner) is then estimated on this same 1,444-family panel, where it retains its usual structural restriction to 648 mothers who report a new partner. Table A2 and Table A3 report the TWFE and state-clustered Callaway-Sant'Anna estimates for both outcome sets, re-estimated on these fixed samples, in the same format as Tables 5 and 9.

Table A2: Father Outcome Set, Common Sample: TWFE and Callaway-Sant'Anna Estimates

Outcome	N	Fam.	TWFE Coef. (SE)	CS ATT (SE)
Health & Mental Health				
Father Depression	14639	881	0.0139 (0.0120)	0.0200* (0.0108)
Father Any Mental Health	14639	881	0.0025 (0.0116)	0.0120 (0.0089)
Substance Use				
Father Used Other Substance	14639	881	0.0030 (0.0121)	0.0060 (0.0050)
Father Used Marijuana	14639	881	0.0031 (0.0041)	0.0008 (0.0034)
Crime & Justice				
Father Convicted/Jailed	14639	881	0.0090 (0.0099)	0.0233* (0.0127)
Labor Market				
Father Hours Worked	14639	881	-1.0152* (0.5191)	-1.8716 (1.6179)
Father Earnings	14159	881	-0.0373*** (0.0134)	0.0143 (0.0304)
Father Employed	14639	881	0.0114 (0.0097)	0.0177 (0.0240)
Father on Welfare	14639	881	0.0039 (0.0028)	0.0064* (0.0035)
Relationship & Family Structure				
Father Repartnered	14639	881	-0.0190 (0.0133)	-0.0448** (0.0183)
Intimate Partner Violence				
Father Reports IPV (Bio-Mom)	14639	881	0.0089* (0.0052)	0.0113 (0.0076)
Parenting Behavior				
Father Harsh Parenting	14639	881	-0.0053 (0.0148)	-0.0214 (0.0786)
Father Parenting Stress	14582	881	0.0065 (0.0137)	0.0291 (0.0228)
Father Spanked Child (Past Month)	14578	881	-0.0002 (0.0034)	0.0041 (0.1092)
Housing & Food Insecurity				
Father Housing Instability	14636	881	0.0054 (0.0061)	0.0080 (0.0241)

Notes: Sample is restricted to the 881 fathers with non-missing data on *all fifteen* father outcomes (the intersection of the per-outcome samples underlying Table 5), so every row below is estimated on the identical set of families. TWFE column: family and year fixed effects, household size control, standard errors clustered by state. CS column: Callaway and Sant'Anna (2021) estimator, never-treated comparison group, modal state cluster, 999-iteration bootstrap. Both use medical legalization status only. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. See Section A2 for discussion.

Table A3: Mother- and Child-Side Outcome Set, Common Sample: TWFE and Callaway-Sant'Anna Estimates

Outcome	N	Fam.	TWFE Coef. (SE)	CS ATT (SE)
Health & Mental Health				
Mother Depression	25982	1444	-0.0109 (0.0100)	-0.0135 (0.0200)
Mother Any Mental Health	25982	1444	-0.0016 (0.0078)	0.0019 (0.0189)
Child Any Mental Health	25982	1444	-0.0029 (0.0111)	0.0123 (0.0319)
Child Hospitalized Overnight	25982	1444	-0.0082 (0.0060)	-0.0161 (0.0231)
Child Obesity	19217	1444	-0.0218 (0.0142)	-0.0252 (0.0296)
Substance Use				
Child Used Other Substance	14856	1444	-0.0145 (0.0202)	-0.0254 (0.0279)
Child Used Marijuana	14837	1444	-0.0085 (0.0176)	-0.0025 (0.1070)
Mother Used Other Substance	25982	1444	0.0103 (0.0152)	0.0311 (0.0317)
Mother Used Marijuana	25982	1444	0.0120** (0.0051)	0.0100 (0.0176)
Crime & Justice				

(continued)

Outcome	N	Fam.	TWFE Coef. (SE)	CS ATT (SE)
Mother Convicted/Jailed	25982	1444	-0.0093 (0.0060)	-0.0071 (0.0196)
Labor Market				
Mother Earnings	22305	1444	-0.1845** (0.0818)	-0.1748 (0.2289)
Mother Employed	25982	1444	-0.0105 (0.0188)	-0.0179 (0.2893)
Mother Hours Worked	25829	1444	-0.8329** (0.3679)	-0.7612 (1.6543)
Mother on Welfare	25982	1444	0.0332** (0.0134)	0.0286 (0.0317)
Relationship & Family Structure				
Mother Married	25982	1444	0.0185** (0.0078)	0.0187 (0.0250)
Mother Repartnered	25982	1444	-0.0206 (0.0150)	-0.0039 (0.0490)
Child Living with Biological Father	25982	1444	0.0329** (0.0130)	0.0299 (0.0508)
Intimate Partner Violence				
Mother Reports IPV (Biological Father)	25971	1444	0.0051 (0.0052)	-0.0068 (0.0113)
Mother Reports IPV (Bio-Dad/New Partner)	25982	1444	0.0070 (0.0044)	-0.0046 (0.0135)
Mother Reports IPV (New Partner)	9224	648	0.0031 (0.0048)	0.0047 (0.0042)
Parenting Behavior				
Mother Harsh Parenting	25982	1444	-0.0193 (0.0200)	0.0049 (0.1271)
Mother Parenting Stress	25979	1444	0.0090 (0.0271)	0.0003 (0.0998)
Mother Spanked Child (Past Month)	25980	1444	-0.0095* (0.0056)	-0.0015 (0.0566)
Housing & Food Insecurity				
Mother Housing Instability	25982	1444	-0.0023 (0.0084)	0.0245 (0.0202)
Mother Food Insecure (Adult)	23350	1444	-0.0004 (0.0048)	0.0012 (0.0020)
Mother Food Insecure (Child)	23350	1444	-0.0020 (0.0019)	0.0012 (0.0015)

Notes: Sample is restricted to the 1,444 mothers/children with non-missing data on all mother- and child-side outcomes *except* Mother Reports IPV (New Partner), which retains its structural skip-pattern restriction (asked only of mothers who report a new partner) even within this common sample. TWFE and CS specifications otherwise identical to Table 9. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. See Section A2 for discussion.

Four of the five father outcomes significant in the headline results remain significant at the 10 percent level on the common sample. Father repartnering is, if anything, more sharply estimated (CS ATT $= -0.045$, $p = 0.014$, compared with -0.034, $p = 0.022$ in the headline results); father criminal justice contact (0.023, $p = 0.066$, compared with 0.023, $p = 0.069$) and father welfare receipt (0.006, $p = 0.063$, compared with 0.006, $p = 0.060$) are both nearly identical to their headline estimates; and father depression is modestly attenuated but remains significant (0.020, $p = 0.065$, compared with 0.023, $p = 0.054$). Only father any-mental-health, the weakest of the five headline results, and already the closest to the paper's 10 percent threshold ($p = 0.079$), loses significance on the common sample (0.012, $p = 0.175$); the point estimate is smaller but retains the same sign, consistent with reduced statistical power from an 8 percent smaller sample, not with a qualitatively different result.

On the mother- and child-side, the null pattern is unchanged: none of the twenty-six outcomes is significant under the state-clustered CS estimator on the 1,444-family common sample, exactly as in the headline results.

I read this as confirming that the paper's central asymmetry is not an artifact of outcome-specific sample composition. Four of the five father-side findings, including the anchor father-repartnering result, hold on an identical, fixed set of 881 fathers, and the mother- and child-side outcome set remains uniformly null on its own fixed 1,444-family sample. The one result that does not survive, father any-mental-health, was already the weakest and least precisely estimated of the five in the headline specification, and its loss of significance here is consistent with a smaller sample, not with a change in the sign or approximate magnitude of the underlying estimate.

A3 Recreational Legalization: Mother- and Child-Side Results

Table A4 reports the mother- and child-side outcome set under recreational, not medical, legalization, on the same samples, controls, fixed effects, and state-clustered standard errors as the father-side recreational-legalization table in Section 7.2. As discussed there, the state-clustered Callaway-Sant'Anna estimator could not be reliably estimated for the recreational treatment, so only TWFE estimates are reported. This table is discussed alongside the paper's mother- and child-side specificity check in Section 8.3.

Table A4: Mother- and Child-Side Outcome Set, Recreational Legalization: TWFE Estimates

Outcome	*N*	Fam.	TWFE Coef. (SE)
Health & Mental Health			
Mother Depression	29060	1615	0.0022 (0.0093)
Mother Any Mental Health	29060	1615	0.0185* (0.0098)
Child Any Mental Health	29060	1615	0.0065 (0.0057)
Child Hospitalized Overnight	29060	1615	-0.0206*** (0.0053)
Child Obesity	21276	1613	-0.0012 (0.0168)
Substance Use			
Child Used Other Substance	16607	1615	0.0697 (0.0421)
Child Used Marijuana	16588	1615	0.0382* (0.0197)
Mother Used Other Substance	29060	1615	0.0209 (0.0153)
Mother Used Marijuana	29060	1615	-0.0029 (0.0066)
Crime & Justice			
Mother Convicted/Jailed	29060	1615	-0.0056 (0.0058)
Labor Market			
Mother Earnings	23436	1518	-0.1719 (0.1141)
Mother Employed	29060	1615	-0.0153 (0.0220)
Mother Hours Worked	28756	1610	-0.5598 (0.6760)
Mother on Welfare	29060	1615	0.0079 (0.0273)
Relationship & Family Structure			
Mother Married	29060	1615	-0.0298 (0.0195)
Mother Repartnered	29060	1615	-0.0051 (0.0160)
Child Living with Biological Father	29060	1615	-0.0197 (0.0349)
Intimate Partner Violence			
Mother Reports IPV (Biological Father)	28975	1611	-0.0015 (0.0037)
Mother Reports IPV (Bio-Dad/New Partner)	29060	1615	-0.0020 (0.0035)
Mother Reports IPV (New Partner)	10192	714	0.0070* (0.0039)

(continued)

Outcome	N	Fam.	TWFE Coef. (SE)
Parenting Behavior			
Mother Harsh Parenting	29060	1615	-0.0354 (0.0248)
Mother Parenting Stress	29056	1615	0.0141 (0.0362)
Mother Spanked Child (Past Month)	29040	1614	-0.0123* (0.0071)
Housing & Food Insecurity			
Mother Housing Instability	29060	1615	0.0016 (0.0134)
Mother Food Insecure (Adult)	24992	1545	-0.0015 (0.0035)
Mother Food Insecure (Child)	24992	1545	-0.0072** (0.0033)

Notes: Treatment is recreational (rather than medical) marijuana legalization; same sample, controls, fixed effects, and state-clustered standard errors as Table 9. Callaway-Sant'Anna estimates are not reported; see Section 7.2 for why. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A4 Plots and Figures

A4.1 Figures

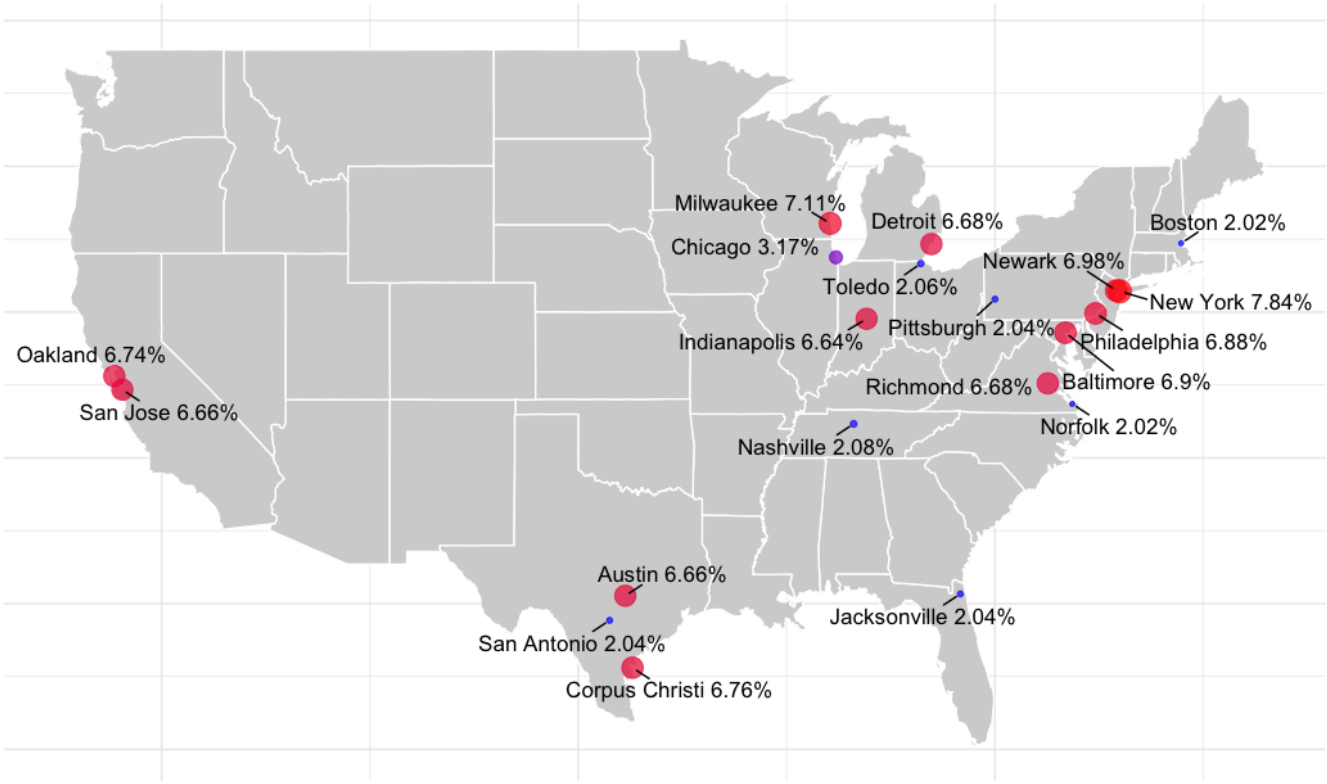

Figure A1: FFCWS Survey Cities and Sample Share by City

A4.2 Additional Event Study Plots

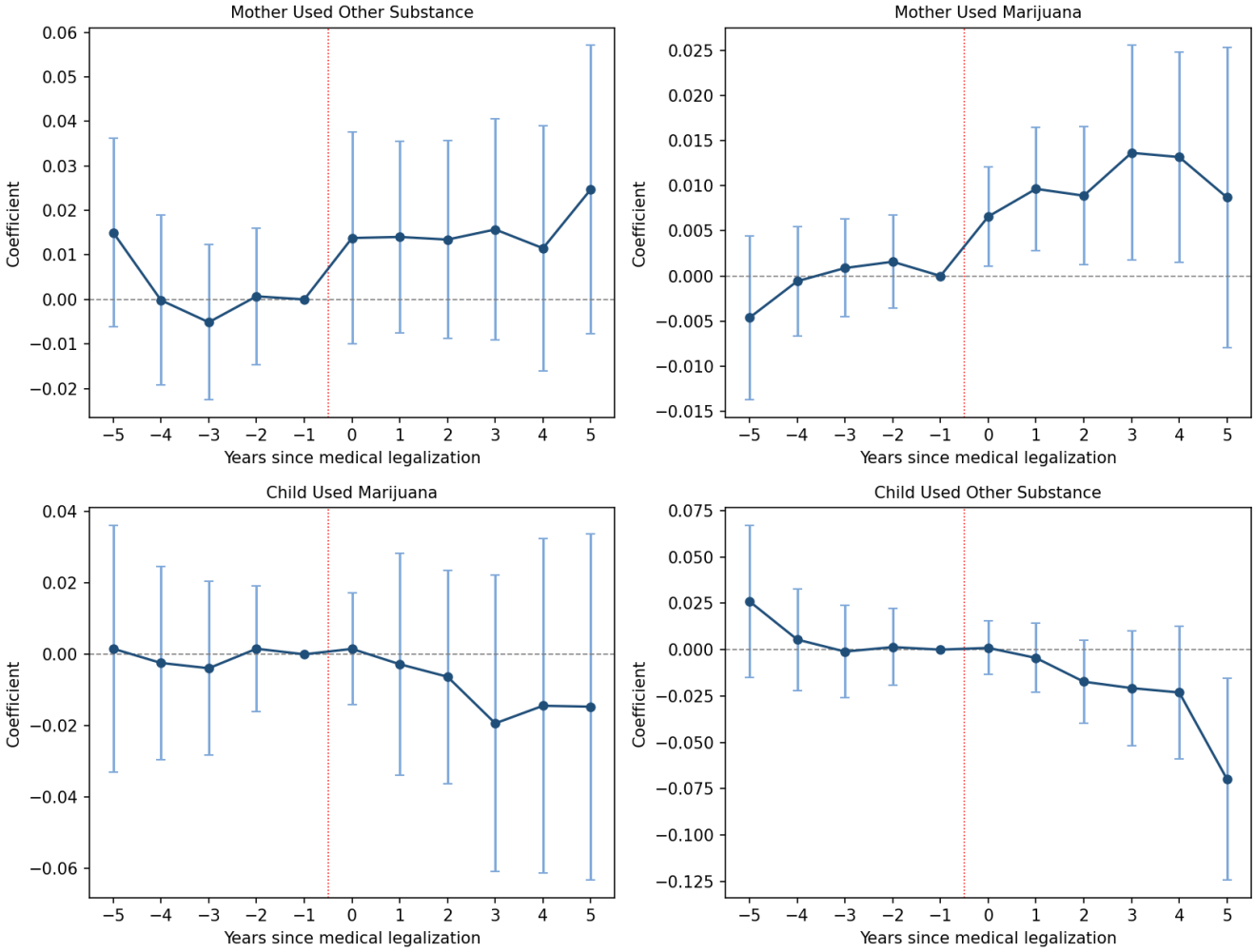

Figure A3: Event studies, substance use domain, mother- and child-side outcomes. Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

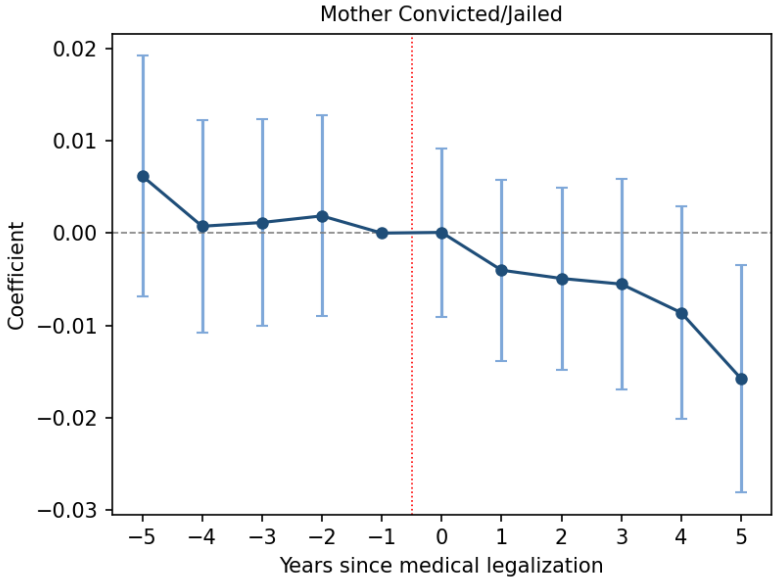

Figure A4: Event study, crime and justice domain, mother-side outcome. Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

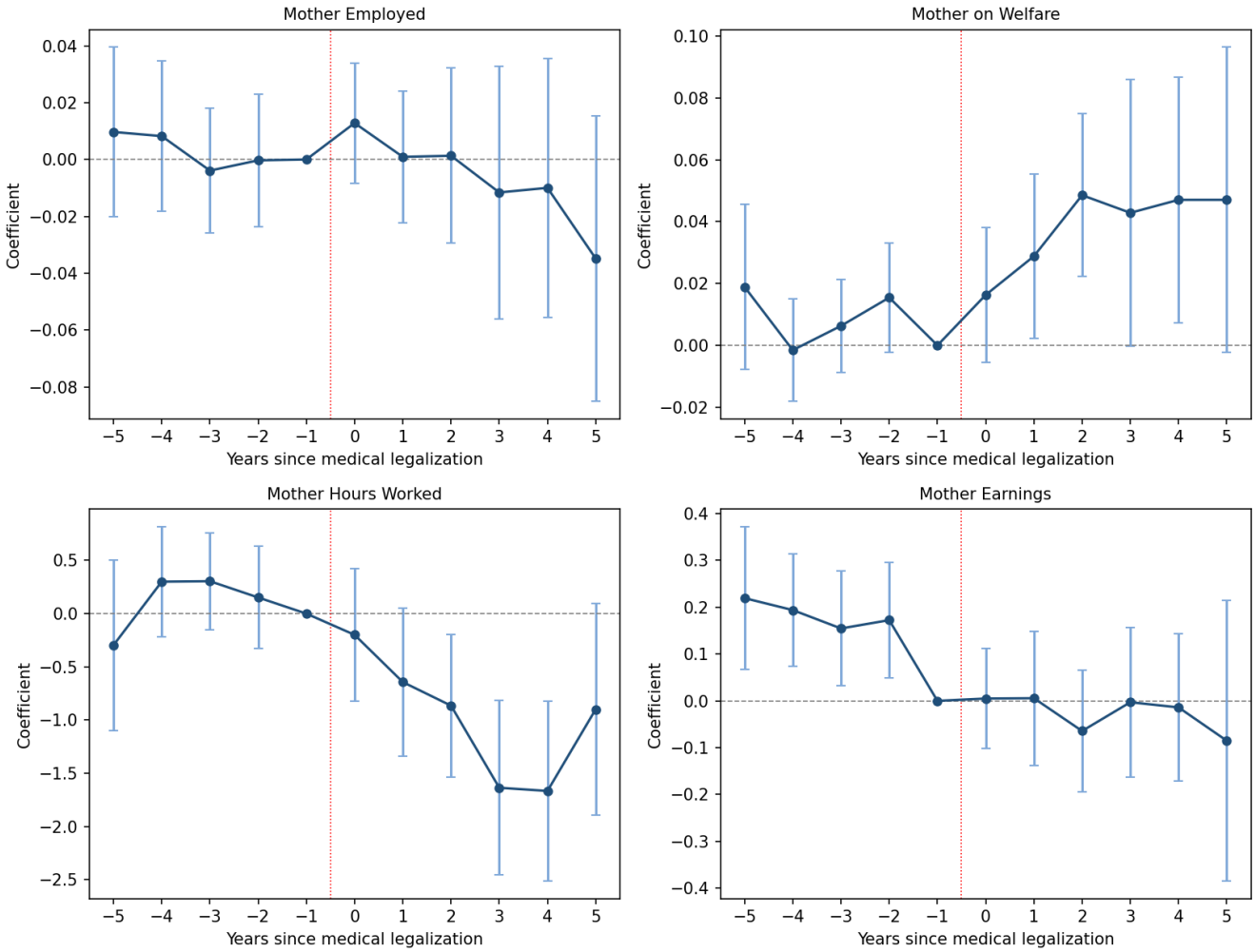

Figure A5: Event studies, labor market domain, mother-side outcomes. Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

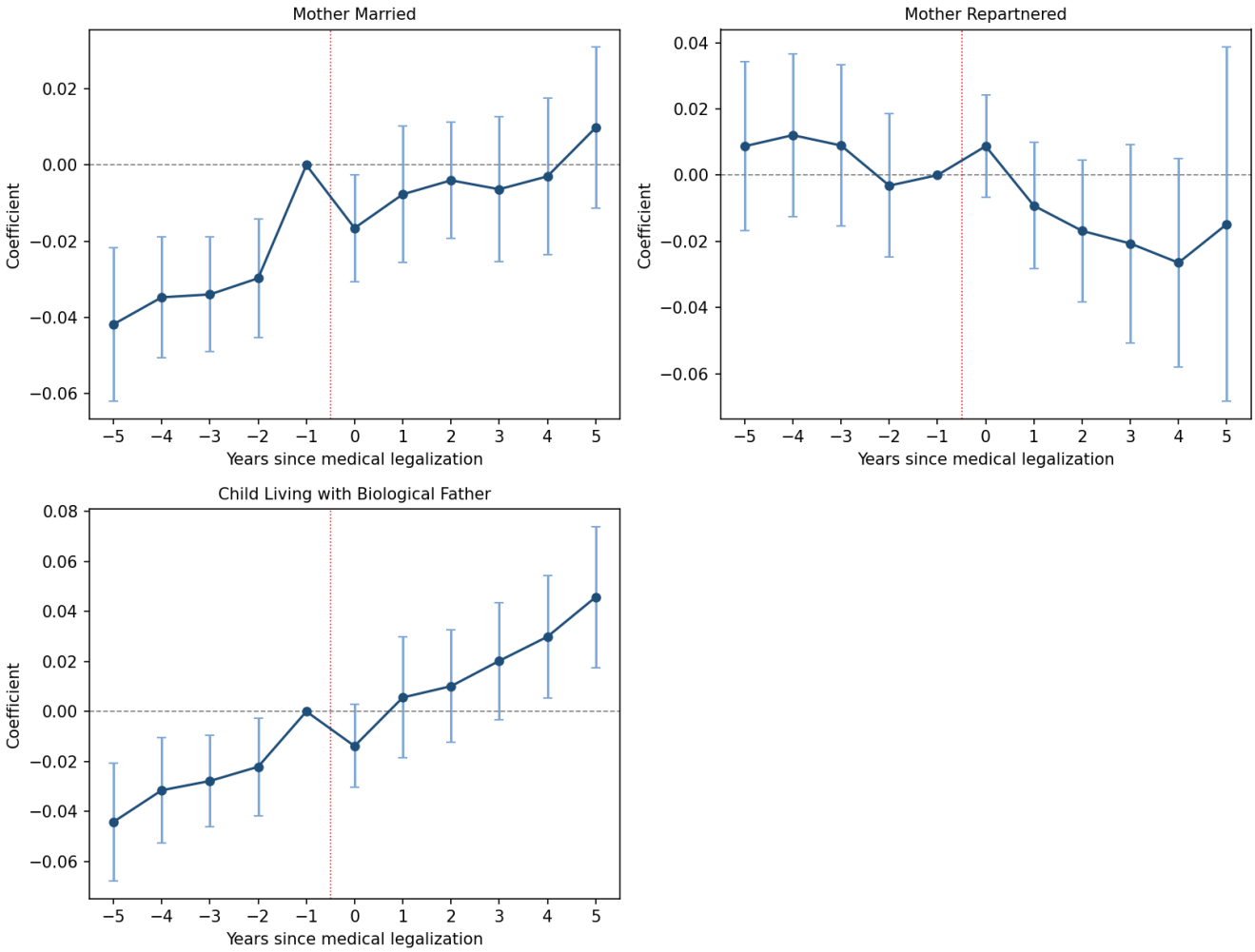

Figure A6: Event studies, relationship and family structure domain, mother- and child-side outcomes. Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

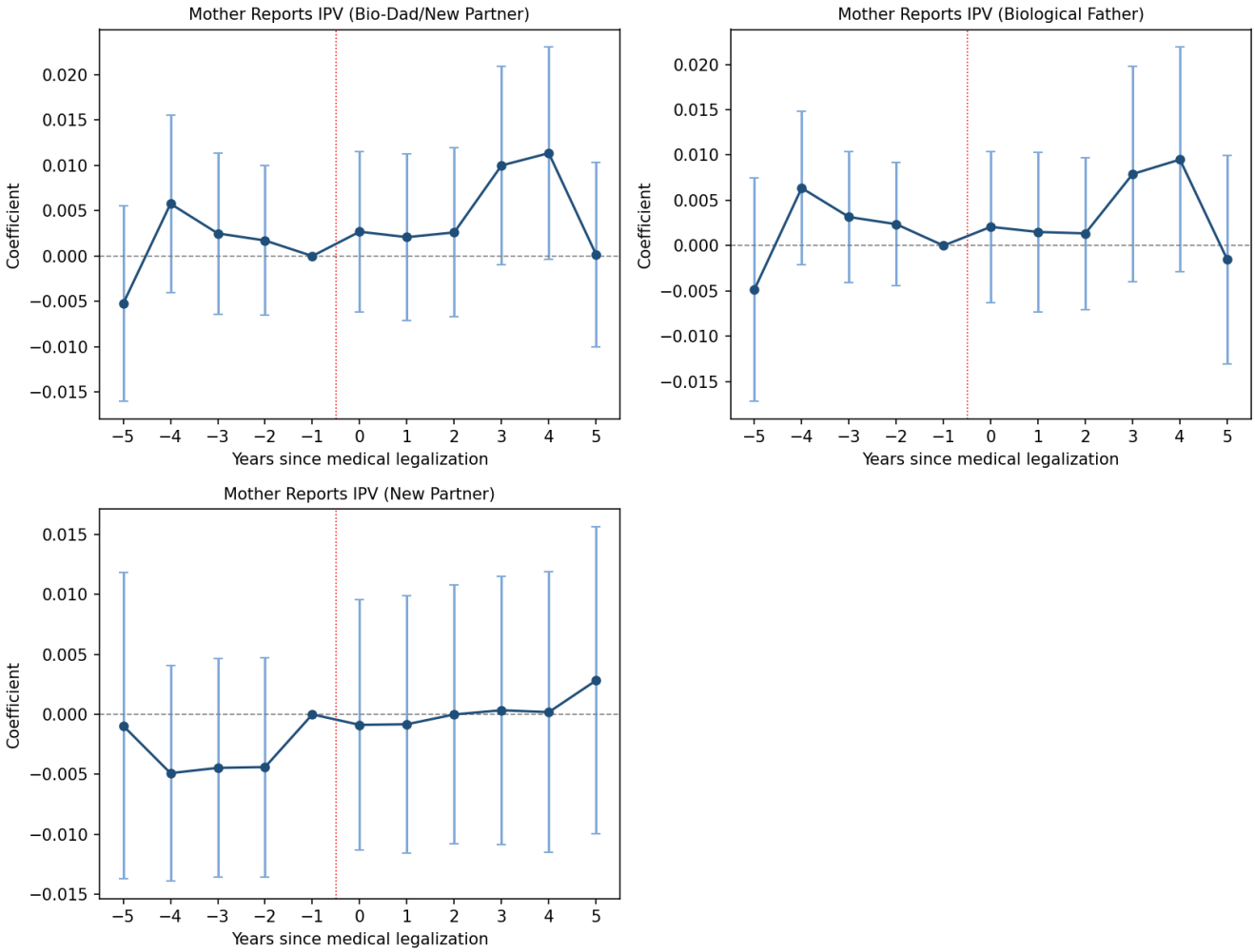

Figure A7: Event studies, intimate partner violence domain, mother-side outcomes. Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

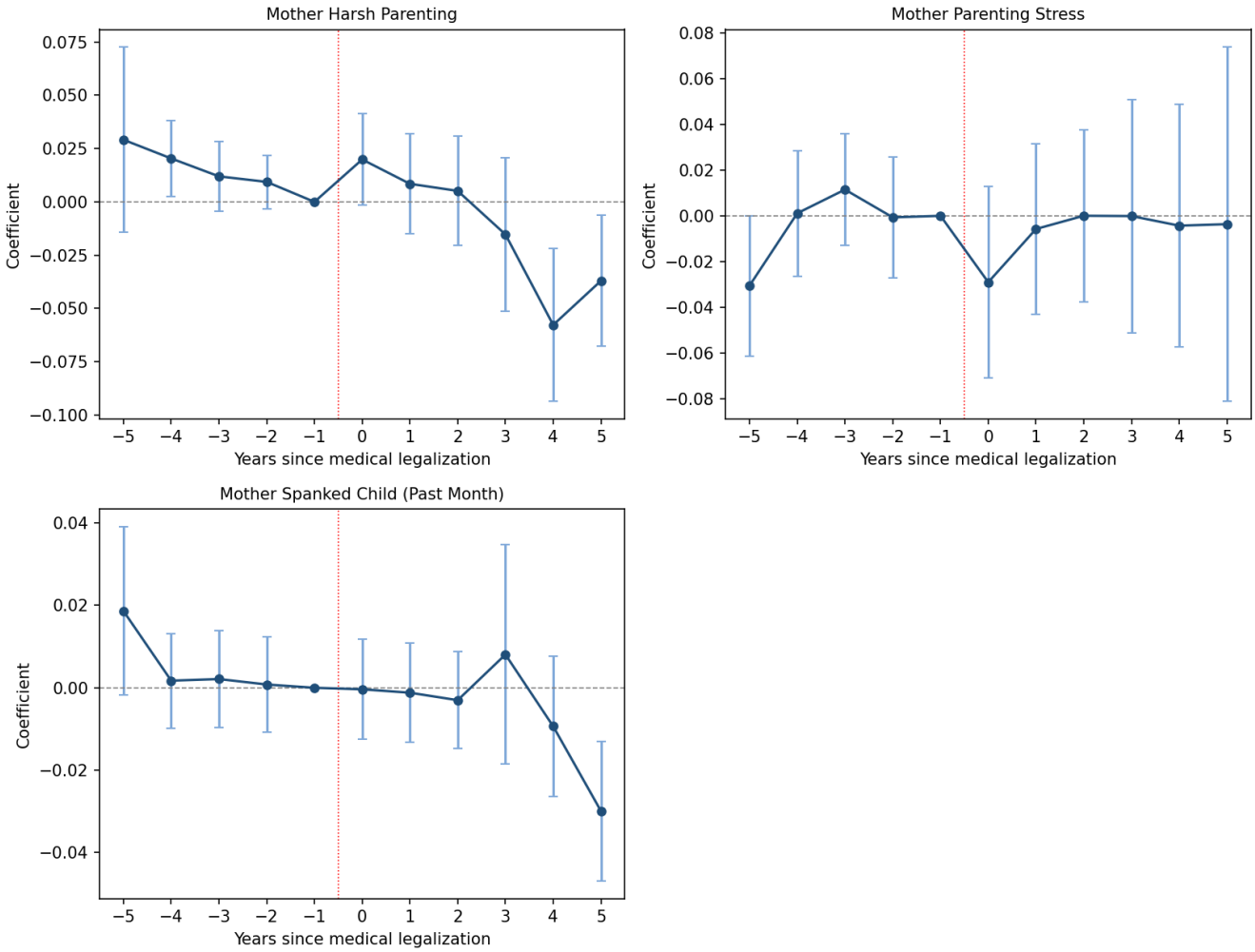

Figure A8: Event studies, parenting behavior domain, mother-side outcomes. Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

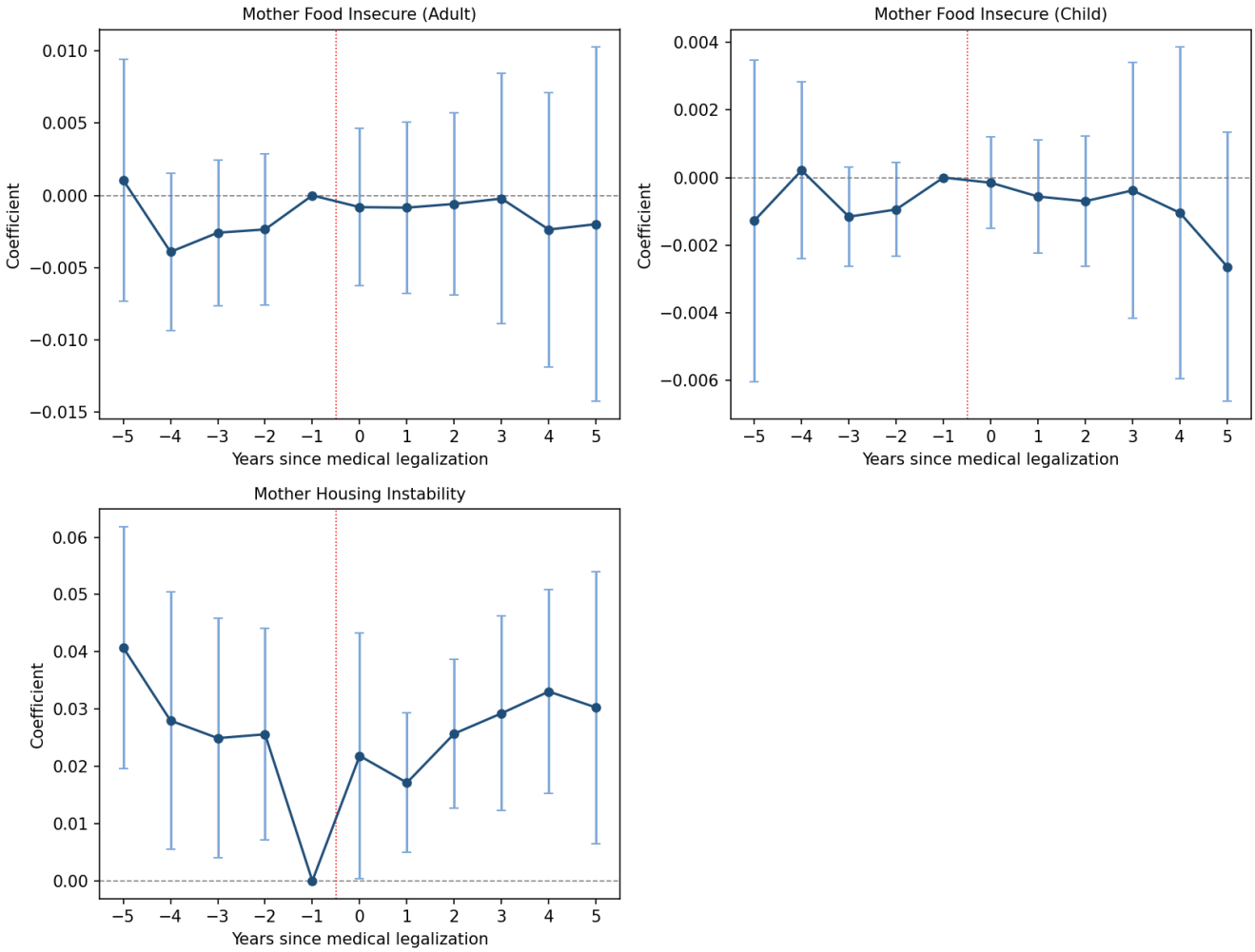

Figure A9: Event studies, housing and food insecurity domain, mother-side outcomes. Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.

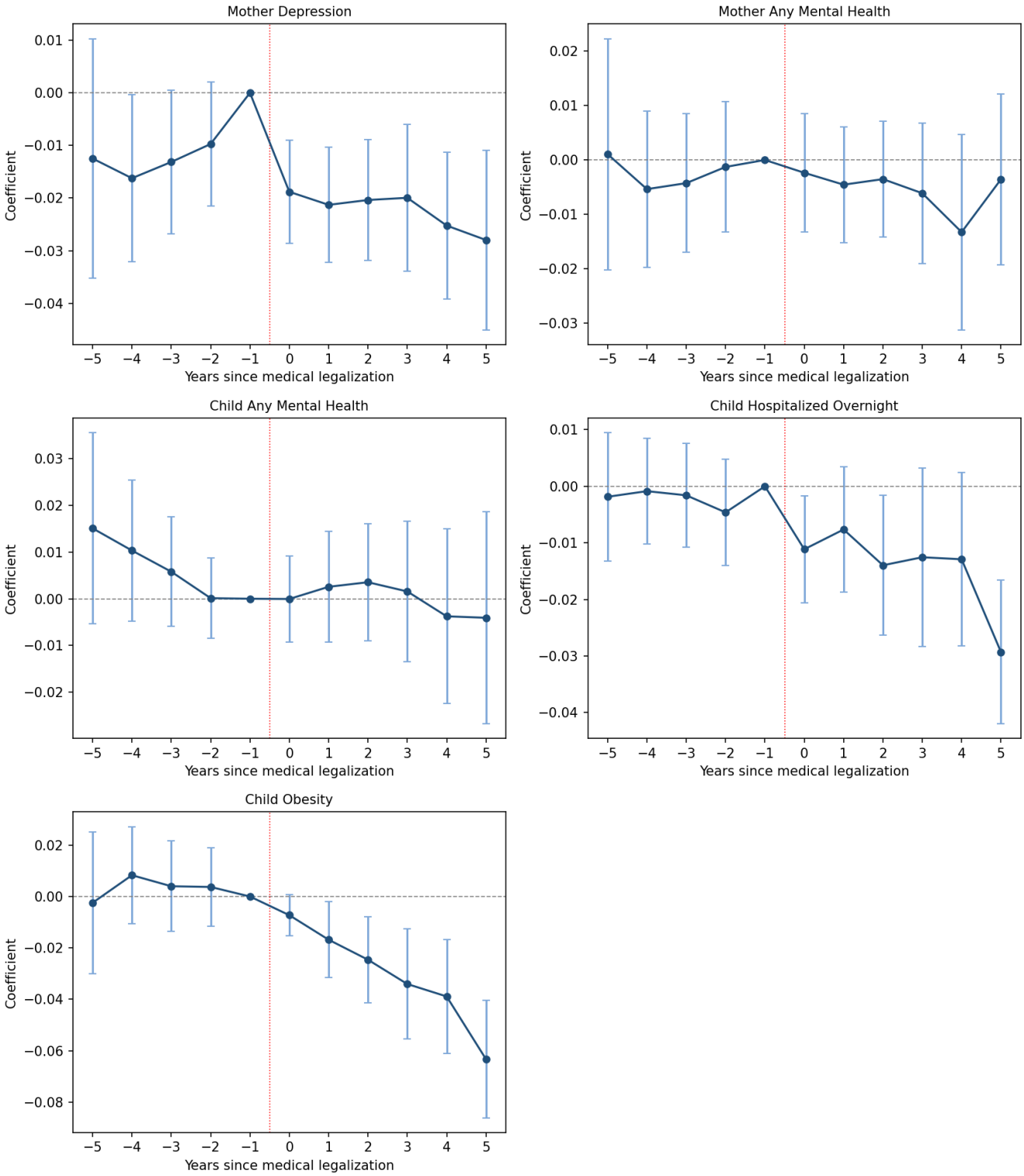

Figure A2: Event studies, health and mental health domain, mother- and child-side outcomes. Years relative to state medical marijuana legalization on the horizontal axis; year −1 normalized to zero. Bars are 90 percent confidence intervals, standard errors clustered by state.