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Recreational marijuana legalization and admission to the foster-care system

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Abstract

We estimate the effects of legalized recreational marijuana on entry into the foster-care system. Exploiting state-level variation in legalization and its timing, we estimate that legalization decreases foster-care placements by at least 10%, with larger effects in years after legalization, and for admissions for reasons of parental drug and alcohol abuse, physical abuse, neglect, and parental incarceration. Our findings imply that legalization may have important consequences for child welfare, and that substitution toward marijuana from other substances can be an important part of how legalization affects admissions.

KEYWORDS
drug policy, foster care, marijuana

JEL CLASSIFICATION
I30, J13, K42

1 | INTRODUCTION

Just before Colorado and Washington legalized marijuana for recreational use, the New York Times reported on struggles between marijuana-using parents and the child welfare system (Secret, 2011):

> Hundreds of New Yorkers who have been caught with small amounts of marijuana, or who have simply admitted to using it, have become ensnared in civil child neglect cases in recent years, though they did not face even the least of criminal charges, according to city records and defense lawyers. A small number of parents in these cases have even lost custody of their children.
>
> New York City's child welfare agency said that it was pursuing these cases for appropriate reasons, and that marijuana use by parents could often hint at other serious problems in the way they cared for their children.

By 2018, marijuana was legal for recreational use in 12 states. These legalizations represent a radical departure from long-standing policy toward the drug, and have attracted commensurate attention from researchers. While much of the

Abbreviations: AFCARS, Adoption and Foster Care Analysis Reporting System; CPS, Child Protective Services; MML, Medical Marijuana Legalization; RML, Recreational Marijuana Legalization.

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literature on legalization has centered on its effects on broader socioeconomic outcomes such as housing prices and crime (see, e.g., Brinkman & Mok-Lamme, 2019; Cheng et al., 2018; Dragone et al., 2019), less is known about the direct welfare consequences of legalization policies for marijuana users and those close to them. Less still is known about the effects of legalization on child welfare.

In this paper, we study the effects of Recreational Marijuana Legalization (RML) on the entry of children into the foster-care system. As the preceding quotation alludes, there are two primary channels through which RML may affect entry into the foster care system. First, the legalization of recreational marijuana may change the policies of state welfare agencies, as well as the attitudes of their agents, toward marijuana use, reducing the number of children removed from their parents and placed in foster care because of concerns about parental marijuana use itself. Second, legal parental access to marijuana may affect the substantive wellbeing of children in their homes, and consequently their likelihood of being placed in foster care. For example, marijuana use may engender neglectful behavior among parents, raising welfare concerns that result in removal from the home and placement in foster care. Alternatively, there is some evidence that the use of marijuana decreases violent and other physically abusive behavior (see Ostrowsky, 2011, e.g.,), which may decrease foster-care placements due to physical abuse. Legalization may also affect child welfare by causing parents to substitute toward marijuana from alcohol or harder drugs (Anderson et al., 2013; Chu, 2015; Dragone et al., 2019), or by acting as a complement to, or gateway drug that results in, other forms of substance abuse (DeSimone, 1998; Secades-Villa et al., 2015). Our primary dataset, which distinguishes between routes of entry into the foster-care system, allows us to partially disentangle these competing effects.

Although the implications of marijuana use for users is a subject of ongoing debate (see, e.g., Carliner et al., 2017), the literature provides some guidance about the public health consequences of marijuana legalization. Anderson et al. (2013) find that traffic fatalities decrease in response to the legalization of medical marijuana, an effect they attribute to substitution away from alcohol and toward marijuana. Hansen et al. (2020) note that marijuana-related traffic fatalities increase in RML states after legalization, though they also find a similar increase in non-RML states. Several studies have also assessed the effects of marijuana legalization on child welfare. Wang et al. (2018) find that marijuana-related emergency room visits among adolescents increased in Colorado following RML. Dai (2019) documents that youth use of marijuana has increased along with a concomitant decline in the use of alcohol and cigarettes. Anderson et al. (2015) and Anderson et al. (2019), on the other hand, find that youth marijuana use does not increase following medical marijuana legalization (MML), and actually decreases in response to RML. Similarly, Dills, Goffard and Miron (2017) find no substantial effect of marijuana laws (decriminalizing possession, and legalization for medical and recreational use) on youth use of marijuana or other drugs.

Several studies have examined factors that affect child welfare, including those that may result in foster-care placement. Paxson and Waldfogel (2002) find that parental employment and the local poverty rate are related to the maltreatment of children, and that local welfare benefits affect the likelihood of foster-care placement. In contrast, Lindo et al. (2013) do not find systematic evidence of a relationship between local labor-market conditions and child maltreatment. Much of this literature emphasizes the relationship between substance abuse, maltreatment, and foster care. Markowitz and Grossman (1998, 2000) present evidence that excise taxes on alcohol lead to reductions in reports of violence toward children. Cunningham and Finlay (2012) present evidence that methamphetamine use is associated with increased foster-care entries, and can help explain the increase in entries during the turn of the century. Freisthler, Gruenald and Wolf (2015) find an association between marijuana use and certain types of neglect and physical abuse, and report that parents convicted of charges related to substance abuse are more likely to be involved with child protective services.

There is also a nascent literature on the effects of marijuana legalization on child welfare. Vijay (undated) presents some evidence that reports of child maltreatment increase subsequent to passage of MML legislation, but raises the question of whether this finding represents an increase in maltreatment itself or an increase in the reporting of maltreatment. Rashid and Waddell (2018) perform a similar analysis of the effects of both MML and RML on child maltreatment, finding that MML reduces physical abuse and that RML reduces both neglect and physical abuse. Importantly, they also estimate the effects of these legalization regimes on "substantiated" maltreatment reports, which are those that Child Protective Services (CPS) investigates and deems consistent with states' legal definition of maltreatment.

Our paper adds to this literature in several ways. First, previous work (Doyle & Aizer, 2018) has shown that the relationship between reports of child maltreatment and entries into the foster-care system is weak. Thus, our paper provides evidence on the effects of RML on a previously unstudied dimension of child welfare. Second, as we discuss below, the evidentiary and legal standards for placement into foster care are much more stringent than those for substantiated reporting of maltreatment by CPS.[1] Because avoiding the removal of children from their homes is an important objective of CPS caseworkers (DePanfilis, 2018), the foster-care option is only considered in cases when maltreatment at home is so serious that the child's wellbeing cannot be guaranteed there. Furthermore, when evidence of maltreatment calls for removal, CPS involves the court system in most cases, which makes the final decisions on whether a child should be removed, where the child should be placed, and the termination of parental rights (DePanfilis, 2018). For these reasons, our study is well suited to identify substantive effects of RML on child welfare. Third, our data disaggregate foster-care entries by the reason for removal from the home (e.g., parental drug use, parental alcohol abuse, physical abuse, neglect, and child drug abuse, among others), allowing us to draw more fine-grained conclusions about the channels through which legalization affects the welfare of children. Fourth, our dataset also provides information about entries for reasons that are a priori unlikely to be affected by RML, affording us a persuasive set of falsification tests for the causal interpretation of our results.

Although the placement of children in foster care is motivated by concerns about their wellbeing at home, there is also evidence that placement itself has consequences for outcomes later in life. Doyle (2007, 2008) finds that children placed in the foster care system have a greater likelihood of involvement with the criminal justice system. Similarly, Lindquist and Santavirta (2014) find that children placed in foster care in their teens are more likely to become adult criminals.

We identify the effects of RML on foster-care entries using a straightforward difference-in-differences design, which exploits geographic variation in adoption of RML legislation, as well as the timing of that adoption. The validity of this approach rests strongly on the hypothesis that foster-care entries would have evolved similarly in RML and non-RML states in the absence of legalization, and we present placebo tests that support this hypothesis. Our most conservative estimates imply that legalization causes at least a 10% decrease in total admissions to foster care, with larger effects in years further after legalization and for admissions into foster care due to specific child-welfare concerns. When we disaggregate our results by reason for entry into foster care, we find that the overall decrease is driven by reduced placements due to parental drug abuse, parental alcohol abuse, physical abuse, parental neglect, and parental incarceration. Although we find some tentative evidence of increases in entries due to child drug abuse, we also find that the estimated effects of RML on entries for this reason are heterogeneous and sensitive to the method used to estimate them. We find no evidence that RML increases foster-care placements for any reason, on average.

Our route-specific estimates help clarify how RML affects entries into foster care. Although legalization may decrease entries mechanically by changing policies and attitudes toward the use of marijuana per se (with no way to distinguish between the two), the estimated reductions in placements due to factors such as parental alcohol abuse and physical abuse imply that legalization does have substantive welfare consequences for children. And although we only observe total entries due to parental abuse of any illicit drug (precluding us from directly identifying effects arising from substitution toward marijuana from other illicit drugs), the declines in placements due to alcohol abuse that we estimate show that substitution from other substances toward marijuana can be an important part of how RML affects foster-care entries.

In Section 2, below, we provide a short overview of the history of marijuana legalization in the United States, as well as some background on the foster-care system. In Section 3, we detail the data used in our analysis. Our empirical strategy and results are presented in Section 4. We conclude in Section 5.

2 | BACKGROUND

2.1 | Marijuana legalization in the United States

Marijuana was legal in the United States until the Marijuana Taxation Act of 1937, which sought to limit the consumption and cultivation of the crop, although 21 states had previously restricted the sale of marijuana after the Harrison Act of 1914 (Hardaway, 2018). In *Leary v. United States*, 395 U.S. 6 (1969), the United States Supreme Court ruled this act unconstitutional. However, in 1970 President Nixon signed the Controlled Substances Act, which aimed to regulate drugs with a "high potential for abuse." This further strengthened the regulation of marijuana possession and use, and listed marijuana as a schedule-one drug (i.e., one considered to have a high potential for abuse, with limited medical use). Even though marijuana remains a schedule-one drug at the federal level, as a result of changing attitudes toward marijuana, states were given guidance to enact laws governing its use and sale for recreational purposes in what is commonly known as the Cole Memo (Cole, 2013).

States started implementing marijuana-related laws, with decriminalization for possession of limited amounts, in the 1970s. However, marijuana possession arrests went up significantly during the "war on drugs," rendering these laws ineffective (King & Mauer, 2006). The legalization of marijuana at the state level started with California legalizing marijuana for medical purposes (MML) in 1996. Currently, 33 states and Washington DC have legalized marijuana for medical purposes. Recreational Marijuana Legalization (RML) started with Colorado and Washington passing laws to make the sale and consumption of marijuana legal in 2012. This was followed by Oregon, Alaska, and Washington DC in 2014. Maine, Massachusetts, California and Nevada also passed RML in 2016, as did Vermont, Michigan and Illinois after 2017 (we summarize the dates of legalization for different states in Appendix Table A1). These laws in their varied forms allow individuals to cultivate limited amounts of marijuana at home while allowing for dispensaries to sell in commercial quantities.[2]

2.2 | The foster care system

The foster care system provides "24-hour substitute care for children" who cannot under any circumstances stay with their parents or caregivers (Department of Health and Human Services, 2000). The state agency takes responsibility for the child in terms of placement and care. Children may enter foster care as a result of parental abuse or neglect, child delinquency, disability or mental health problems, among other reasons. The goal of the foster care system is to provide temporary care and responsibility while a permanent placement solution is found. The child may be reunited with his or her parents if conditions change in the home, or they may be placed with a foster family, childcare institution or in a pre-adoptive home (Swann & Sylvester, 2006). While each state has its own process for removing a child from parents or placing a child into foster care, every state's process involves the mandatory use of reporters who are required by law to report incidents of abuse or neglect that come to their attention. In most cases, entry into the foster care system begins with a report of child abuse or neglect, which triggers investigations into whether the abuse can be substantiated. Efforts are made to remedy the situation through the provision of medical, mental health, educational and other support that may be needed by the caretaker. Removal happens when these efforts have been unsuccessful and it is obvious that allowing the child to stay with its caregivers would endanger its welfare (Brooks & Webster, 1999; DePanfilis, 2018).

By the end of 2017, there were about 440,000 children in foster care, a decline from about 488,000 in 2007. However, there has been a consistent increase in the number of children in foster care since 2012, from about 397,000 in 2012 to 437,000 in 2016. The same applies to the number of entries into foster care, which increased by 7.9% from 2012 to 2016 (Children's Bureau, 2017).

3 | DATA

Our study combines data from several sources. The data on the number of children admitted into foster care is from the Adoption and Foster Care Analysis Reporting System (AFCARS), and covers the years 2000–2017.[3] The AFCARS provides information at the case level from states and tribal title IV-E agencies on all children in foster care. The AFCARS reports the dates of the foster child's last removal from their home, as well as of their entry into foster care. In addition, the data provide details about the reasons for the removal, including parental neglect, parental drug use, parental alcohol abuse, and parental incarceration, among others.[4] This helps us to examine the key routes of entry into foster care and, ultimately, identify some of the causal channels through which the legalization of recreational marijuana impacts child welfare. We use these data to calculate the total number of children who entered foster care in a fiscal year, as well as the number who entered for each of the specific reasons indicated in the data.[5] The AFCARS also provides demographic information (age, gender, race, and ethnicity), the method of removal (either by court or personal decision), the location of placement, and time spent in the foster home per placement.

We combine these data with time-varying state-level variables from the Integrated Public Use Microdata Series (Ruggles et al., 2019) samples of the American Community Survey for the same years. These variables include the population size, the proportion who are female, the proportions between the ages of 0–17, 18–24, 25–44, and above 44, the proportions who are white, Hispanic and Black, and the proportions with less than a high-school diploma, with a diploma, with a college degree, or with more education. We also use data on median income and the rates of unemployment and poverty from the Census Bureau and on the rate of alcohol consumption from the National Institute on Alcohol Abuse and Alcoholism (alcohol consumption is measured as the per capita consumption of ethanol from all alcoholic beverages, calculated using state level alcohol sales; see Slater & Alpert, 2019). In addition, we use information on the date of legalization of marijuana for recreational use, by state, from the National Organization for the Reform of Marijuana Laws (NORML) and Procon.[6] We also used these sources to determine the date of legalization of marijuana for medical use.

Table 1 provides summary statistics for selected variables from the AFCARS and other data sources. The variables summarized in the table include the total number of entries into the foster-care system and the number of entries occurring through specific channels, including those which we believe may be directly affected by marijuana use (parental neglect, physical abuse, parental incarceration, and parental drug and alcohol abuse) and some which we think are unlikely to be (child disability and deficient housing, e.g.,). We also provide summary statistics for key demographic variables. The table summarizes these variables for all 50 States and DC, as well as separately by RML status.

In the average state, about 5417 children entered the foster-care system in every year between 2000 and 2017. Entries into the foster-care system were higher on average in RML states than non-RML states. The main reason for child

TABLE 1 Descriptive statistics

	All		RML states		Non-RML states	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Total entries	5416.89	6003.28	7315.68	10,822.39	5010	4225.72
Parental neglect	2813.52	3660.05	4120.45	6435.081	2533.47	2644.76
Physical abuse	794.37	1077.87	1041.01	1450.447	741.51	972.95
Parental drug abuse	1266.67	1667.3	1121.08	923.369	1297.86	1785.78
Sexual abuse	265.76	391.66	301.586	474.331	258.08	371.48
Parental alcohol abuse	339.09	439.55	493.852	677.314	305.92	361.37
Parental incarceration	355.47	376.67	312.333	254.968	364.71	397.47
Child disability	118.92	331.85	55.877	112.38	132.44	360.59
Abandonment	250.18	339.01	146.414	99.121	272.42	367
Relinquish child	53.84	80.99	49.778	80.219	54.71	81.18
Housing difficulties	493.16	640	414.796	416.037	509.95	677.48
RML	0.03	0.18	0.179	0.385	0	0
MML	0.3	0.46	0.852	0.356	0.18	0.39
Covariates						
Female ratio	50.77	0.82	50.446	1.308	50.84	0.65
Age: (0–17)	24	2.26	23.389	2.812	24.13	2.1
Age; (18–24)	36.98	2.31	38.715	3.499	36.61	1.76
Below high school	25.43	3.06	24.08	2.809	25.72	3.04
High school	29.81	3.09	27.531	3.707	30.29	2.71
Bachelor's degree	12.47	2.36	13.933	2.666	12.16	2.17
Proportion black	11.9	11.25	10.681	15.824	12.16	10
Proportion white	79.97	12.83	77.107	16.6	80.59	11.8
Proportion hispanic	9.94	9.76	14.22	10.577	9.02	9.33
Alcohol consumption	2.7	8.75	2.774	0.514	2.69	9.64
Poverty rate	12.7	3.42	12.313	2.855	12.79	3.53
Median household Inc	58,418.01	12,018.65	63,932.4	11,646.21	57,236.3	11,772.68
N	918		162		756	

Note: Authors' calculations.

removal and entry into foster care is parental neglect, with an average of 2814 entries. This is followed by parental drug abuse, which led to about 1267 entries, on average. Removals due to physical abuse, parental alcohol abuse, housing difficulties and parent incarceration also contributed significantly to total entries into the foster-care system. With respect to our controls, most of the averages are not noticeably different between RML and non-RML states, although RML states apparently differ from non-RML states in their educational distribution and racial composition, and have higher median incomes.

4 | EMPIRICAL EVIDENCE

4.1 | Identification and estimation

To identify the effects of RML on entry into the foster-care system, we use a straightforward difference-in-differences design that relates foster-care entries to variation in legalization across states and over time. Under the usual parallel trends assumption that entries would have evolved similarly in RML and non-RML states were it not for the legalization of marijuana for recreational purposes, these comparisons identify the causal effects of RML on foster-care entries. Our key dependent variable is the log of entries into foster care for any reason, although we also examine how RML affects (the logs of) entries through specific routes.

A series of papers has shown that traditional regression-based approaches to difference-in-differences and event-study regressions can produce misleading estimates of the causal effects of the treatment when there is variation in treatment timing and the effects of the treatment are heterogeneous with respect to treatment groups and time periods (Abraham & Sun, 2020; Borusyak & Jaravel, 2017; de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021). To address these problems, we use one of the estimators developed by Callaway and Sant'Anna (2020), CS hereafter, for differences in differences with multiple groups and time periods. This approach estimates the average effect of the treatment for each treatment-timing group, in each time period in which that group is treated, using a simple 2×2 difference-in-differences estimate that compares the change in outcomes for that group relative to a reference period to the same change in a control group (which we take as the set of states that never pass RML during our sample period).[7] These group×time-specific estimates are then averaged to summarize the causal effects of the treatment. We supplement these estimates with those from traditional regressions of outcomes onto state and year fixed effects and an indicator for RML.

We also present estimates that control for a vector of state-level covariates that may simultaneously relate to both RML and foster-care entries. We control for state-level demographics, including the natural logarithm of the state population and the size of the population at risk for entry into foster care (i.e., the proportion of the population younger than 18 years old). To control for parental demographics that may affect the likelihood of maltreatment and subsequent placement in foster care, we also include the proportions of the population between the ages of 18 and 24, 25 and 44, and above 44 years, as well as the proportions of females, Hispanics, blacks, and whites. Because economic factors may affect parents' ability to care for their children, potentially contributing to the removal of the child for reasons of parental neglect (Lindo et al., 2013; Paxson & Waldfogel, 2002), we also control for the unemployment rate, the poverty rate, and household median income, all at the state level. Similarly, because education may affect a parent's ability to take care of, and propensity to mistreat, their children, we include measures of state-average education. We also control for state-level alcohol consumption per capita and, in some specifications, indicators for whether the state has legalized marijuana for medical use.

When we use the CS approach with covariates, we assume that parallel trends holds conditional on pre-treatment realizations of those covariates. Hence, for our CS estimates these covariates are measured during the reference period of the relevant 2×2 difference-in-differences estimate. When we use the traditional regression approach, we allow these controls to be time varying (although this assumes that the effect of the treatment does not depend on the covariates, that there are no covariate-specific trends, and that the treatment itself does not influence the covariates; see Callaway & Sant'Anna, 2020a; Sant'Anna & Zhao, 2020).

4.2 | RML and total entries into foster care

To provide empirical evidence on the validity of our identification strategy, and to motivate our difference-in-differences estimates, we present in Figure 1 estimates of the dynamic effects of RML, estimated using the CS approach.[8] Panel (a)

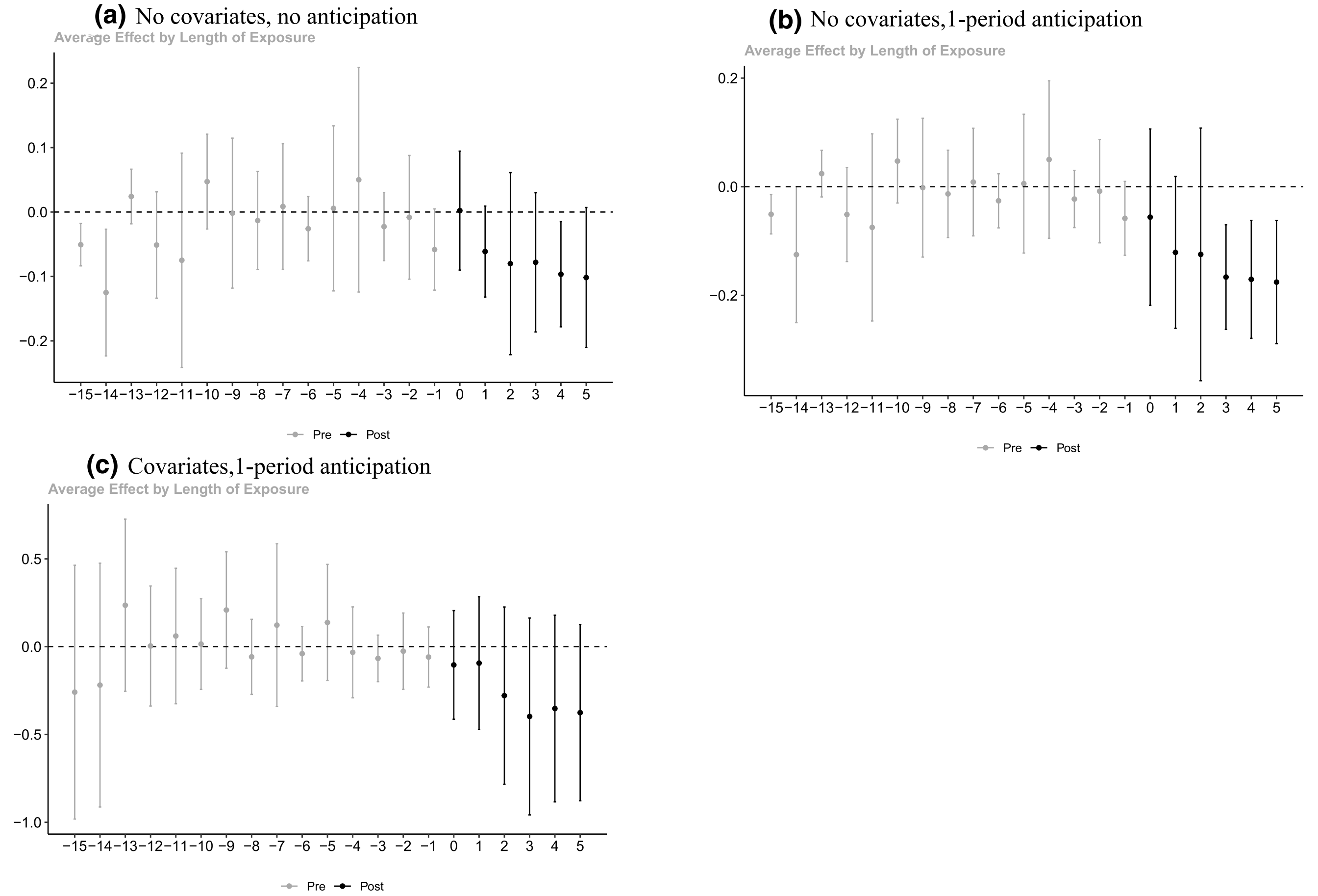

FIGURE 1 Dynamic effects of RML. Callaway and Sant'Anna (2020) dynamic treatment effects. Panel (a) assumes unconditional parallel trends and no anticipation, panel (b) assumes unconditional parallel trends and one period of anticipation, panel (c) assumes conditional parallel trends (using all of the covariates described in the text, including MML) and one period of anticipation

shows the estimated dynamic effects for a baseline case that does not include covariates and uses the year immediately preceding the passage of RML as the reference period for each group (these dynamic effects average all group×time effects that correspond to a particular amount of time until, or since, passage of RML, weighting by group size). As panel (a) shows, foster-care entries tend to decline in the period just preceding RML, relative to the previous period. Although this difference is not statistically significant, it is consistent with the possibility that the passage of RML is anticipated (or perhaps that RML ballot initiatives themselves can affect foster-care entries). To allow for this possibility, in panel (b) we present dynamic effect estimates that allow for one period of anticipation (i.e., that use the period 2 years before passage as the reference period). This resulting estimated post-treatment dynamic effects that are somewhat larger in absolute value than those in panel (a). Panel (c) presents analogous effects obtained after conditioning on the set of covariates described previously, and allowing for one period of anticipation. The conditional and unconditional estimates are broadly similar, although the confidence intervals around the conditional estimates are somewhat larger, possibly owing to a lack of overlap between the covariate distributions for RML and non-RML states.[9]

Two important conclusions emerge from the plots in Figure 1. First, the flow of children into foster care in non-RML states closely tracks that in RML states in the 10 years preceding legalization in RML states, with the estimated pre-treatment effects uniformly small and statistically insignificant. This pattern strongly suggests that non-RML states represent a valid control group for RML states (i.e., that parallel trends is satisfied). Second, following passage of RML, there is a noticeable and persistent drop in the total number of children who enter foster care homes in RML states relative to non-RML states, the magnitude of which tends to increase over time, in most cases becoming statistically significant a few years after legalization.

We present our baseline difference-in-differences estimates of the average effect of RML on foster-care entries in Table 2. The CS estimates are presented in the top panel. When we estimate the effects of RML under the assumption

TABLE 2 RML and foster-care entries

	Callaway and Sant'Anna				
	No covariates		**Covariates**		
	No anticipation	**1-Period anticipation**	**No anticipation**	**1-Period anticipation**	**1-Period anticipation (with MML)**
Group-specific					
2012	−0.077	−0.151***	−0.408***	−0.348**	−0.409***
	(0.039)	(0.038)	(0.106)	(0.150)	(0.158)
2014	−0.081***	−0.120***	−0.144*	−0.278**	−0.258
	(0.020)	(0.025)	(0.071)	(0.117)	(0.128)
2015	0.025	0.045	0.243	−0.127	0.036
	(0.092)	(0.184)	(0.220)	(0.297)	(0.278)
2016	−0.063	−0.148***	0.004	0.050	0.010
	(0.030)	(0.034)	(0.076)	(0.101)	(0.123)
2017	−0.022	−0.068***	−0.116	−0.172	−0.168
	(0.021)	(0.017)	(0.073)	(0.104)	(0.117)
Averages					
Simple	−0.052*	−0.113**	−0.142	−0.204*	−0.197
	(0.031)	(0.052)	(0.125)	(0.112)	(0.142)
Group	−0.044**	−0.102***	−0.054	−0.139	−0.113
	(0.022)	(0.035)	(0.058)	(0.085)	(0.105)
		Regression			
		No covariates		**Covariates**	
RML		−0.132		−0.133*	−0.137*
		(0.089)		(0.076)	(0.075)
MML					−0.022
					(0.080)
N		918		918	918

Notes: Dependent variable is the log of entries into foster care in a given state and year. Covariates include state-level age distribution (proportion between 0-17, 18–24, 25–44, 45 and beyond), proportion female, racial composition (proportions Hispanic, black, and white), education distribution (proportion below high school, proportion with high school, proportion with Bachelor's degree and beyond), median household income, unemployment rate, poverty rate, population, alcohol consumption rate and, in some specifications, an indicator for medical marijuana legalization. These covariates are measured in the comparison period for the Callaway and Sant'Anna estimates and are time varying for the regression estimates. Callaway and Sant'Anna estimates obtained using the outcome-regression approach, using never-treated units as the comparison group. The treatment groups are 2012 (Colorado and Washington), 2014 (Oregon), 2015 (Alaska and Washington, DC), 2016 (California, Maine, and Massachusetts), and 2017 (Nevada). Standard errors, reported in parentheses, are clustered on state (for the Callaway and Sant'Anna estimates, the standard errors are based on a cluster bootstrap; when we bootstrapped the estimates more than once to obtain multiple confidence bands, we report the second set of bootstrap standard errors).

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

that its passage is unanticipated, we find that most of the group-specific averages are negative, though statistically insignificant (these group-specific estimates represent averages over all post-passage periods for each treatment group).[10] We also present two overall summary measures of the average effect of RML on foster-care entries in RML states. The "simple" average represents the average of all the estimated group-time specific average treatment effects, weighted by the sizes of the treatment groups. The "group" average represents an average of the group-specific average treatment effects, again weighted by group size (see Callaway & Sant'Anna, 2020a). With no covariates and no anticipation, these summary measures suggest that RML decreased foster-care entries in RML states by between four and five percent, on average. As we note above, there is some evidence that the passage of RML is anticipated in states that pass it, which may lead our estimates to understate the causal effects of RML when the year preceding passage is used as the reference period. When we estimate our unconditional effects by examining changes relative to the period 2 years before passage, the magnitudes of the estimated effects are considerably larger, with all but one treatment group experiencing statistically significant declines in foster care entries. Allowing for 1 year of anticipation, the simple and group summary measures result in estimated average declines in entries of about 10%.[11]

The remaining columns of the top panel of Table 2 present estimates that condition on the pre-treatment covariates described above. Assuming no anticipation, and focusing for the moment on estimates that do not condition on medical marijuana legalization, the majority of the point estimates are larger in absolute value than the corresponding unconditional estimates, although they are estimated less precisely. Allowing for one period of anticipation increases these magnitudes even further, producing simple- and group-average declines of about 20% and 14%, respectively, although only the former is statistically significant. As we note above, the imprecision of our conditional estimates may partially be a consequence of limited overlap in the covariate distributions for RML and non-RML states. Since limited overlap may also affect the point estimates themselves, and since the dynamic effects presented in Figure 1 suggest that parallel trends holds unconditionally, we have greater confidence in the unconditional estimates.

Thirty-three states and DC have legalized marijuana for medicinal (MML) purposes. Evidence suggests that states that passed MML experienced increases in marijuana consumption (Chu, 2014; Pacula et al., 2015). These states may tend to be more receptive to marijuana use relative to non-MML states, and children in homes where medical marijuana is used might be exposed to medical marijuana or paraphernalia that may lead to contact with child protective services. Since these phenomena may confound our results, we also present estimates obtained after conditioning on MML. The resulting estimates are nearly identical to those that do not condition on MML, indicating that the declining foster-care entries that we document are better explained by RML itself, which is more expansive than medical legalization. Aside from the greater reach of RML laws, another potential explanation for the limited impact of MML is that parents or caregivers using medical marijuana may be able to justify their use of marijuana, therefore limiting the ability of caseworkers to support claims of drug use against them when marijuana use is involved in a foster-care case.

In the bottom panel of Table 2, we supplement our CS estimates with traditional regression estimates of the effects of RML. The point estimates all suggest declines in entries of about 13%, which become statistically significant after we include time-varying state-level covariates. These estimates are also similar to the average CS unconditional estimates that we obtain when allowing for one period of anticipation. This similarity is not coincidental. In Appendix Table A2, we present decomposition results that show that the relative weight that our regression places on comparisons between states that adopted RML later in the sample and those that adopted it earlier (which Goodman-Bacon, 2021, argues is the most severe source of bias in such regressions) is less than 0.01.[12]

The dynamic estimates summarized in Figure 1 suggest that the effects of RML on foster-care entries tend to increase over time. One potential explanation for this is that there is administrative and institutional overhead associated with changing procedures and attitudes regarding marijuana use. Evidence from Colorado, where only two out of 35 counties involved in a health impact assessment in 2015 had implemented marijuana-specific policies relating to child welfare, supports this interpretation (Ng & Tung, 2016). There, RML resulted in the need for a comprehensive review of county-level procedures for removing children from their homes, as well as for guidance regarding how child welfare should be considered in cases involving marijuana use (Ng & Tung, 2016). This suggests that the impact of RML (at least in the case of Colorado) on child welfare decision-making was not immediate due to inadequate provision of guidelines relating to marijuana use. Pacula et al. (2015) find evidence of a similar lag when estimating the effect of MML dispensaries on marijuana-related treatment admissions.[13] Another potential explanation for this phenomenon is that there is a delay between the passage of RML legislation and the widespread availability of marijuana in legal dispensaries, although as we discuss below, we obtain similar results when we define treatment status in terms of dispensary openings. Regardless of the underlying reasons for these delayed effects, our dynamic estimates suggest that the summary measures presented above may understate the longer-run effects of RML on foster-care entries.[14]

4.3 | Route-of-entry-specific effects

As we discuss in the introduction, RML may influence foster-care placements through its potential effects on child welfare, or by changing policies and attitudes toward marijuana use. To investigate the channels through which RML affects foster-care entry, we also estimate the effects of RML on specific routes of entry into foster care.

One entry route of interest is removals due to parental drug use. A child can be removed from the home if the parent or caretaker uses drugs consistently, which may have negative effects on the child (National Data Archive on Child Abuse and Neglect, 2018). The first panel of Figure 2 plots the estimated dynamic effects of RML when the dependent variable is the log of entries due to parental drug abuse (these estimates do not condition on covariates, and the post-treatment effects allow for one period of anticipation).[15] As the figure shows, the pre-legalization differences between RML and non-RML states are all small and statistically insignificant in every period but the one immediately preceding legalization. After legalization, entries due to parental drug abuse decrease in RML states in every period. The corresponding CS estimates, summarized in the first column of Table 3, show that RML decreases entries into foster care due

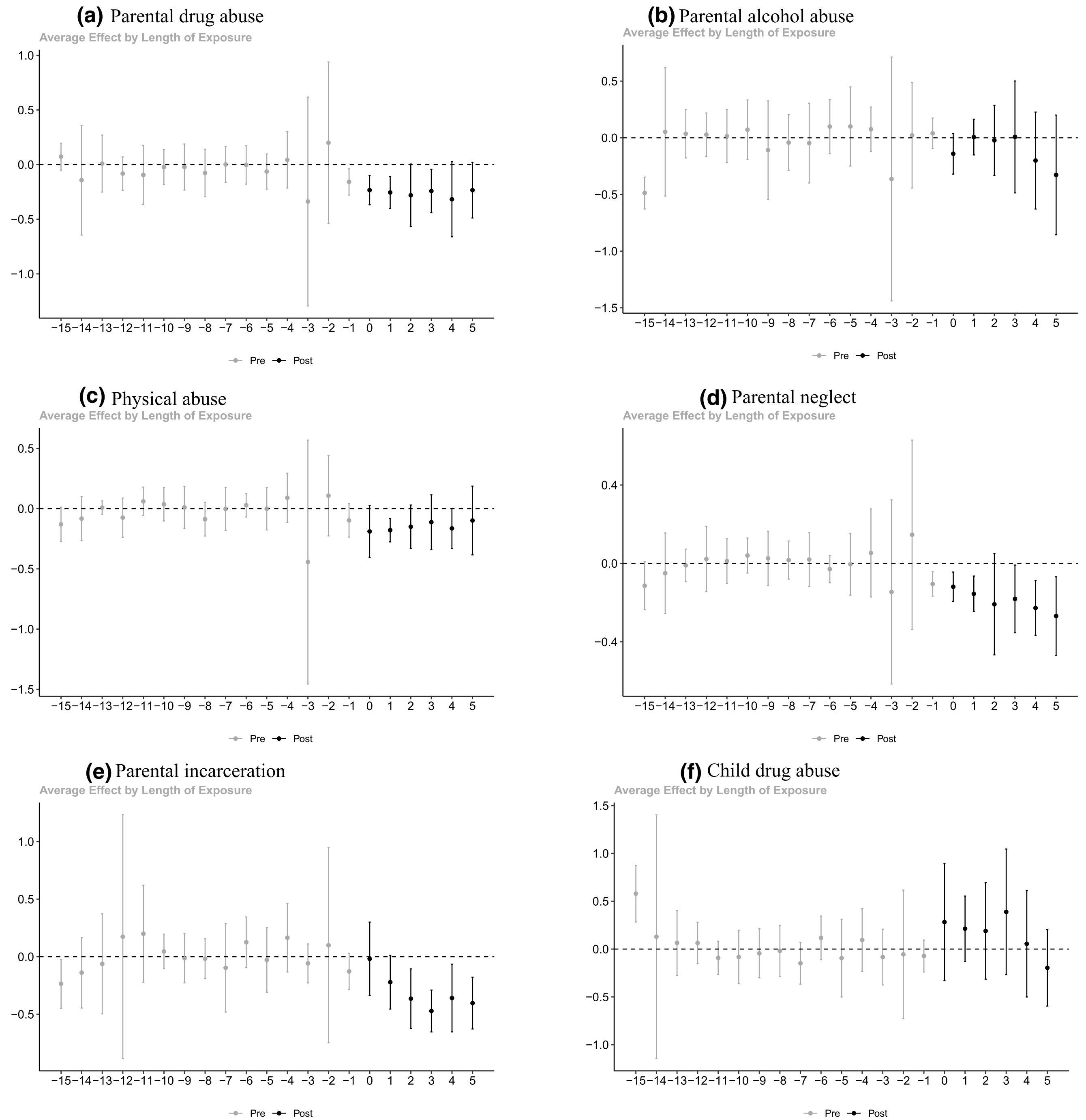

FIGURE 2 Dynamic effects of RML by route of entry. Callaway and Sant'Anna (2020) dynamic treatment effects, obtained using one period of anticipation, without conditioning on covariates. Bars show 95% confidence intervals, after clustering at the state level

TABLE 3 RML and foster-care entries by route of entry

	Parental drug abuse	Parental alcohol abuse	Physical abuse	Parental neglect	Parental incarceration	Child drug abuse
Callaway and Sant'Anna						
Group-specific						
2012	0.210***	−0.185	−0.164**	−0.175***	−0.277***	−0.003
	(0.072)	(0.174)	(0.063)	(0.45)	(0.058)	(0.151)
2014	−0.166***	−0.196**	−0.091	−0.140***	−0.547***	0.875***
	(0.059)	(0.088)	(0.046)	(0.043)	(0.057)	(0.103)
2015	−0.344***	0.071	−0.116***	−0.267***	0.005	0.565***
	(0.057)	(0.112)	(0.034)	(0.040)	(0.081)	(0.098)
2016	−0.342***	−0.127	−0.149	−0.154***	−0.077	−0.037
	(0.071)	(0.081)	(0.093)	(0.038)	(0.092)	(0.093)
2017	−0.332***	−0.222***	−0.656***	−0.025***	−0.142***	0.196
	(0.066)	(0.051)	(0.035)	(0.023)	(0.040)	(0.055)
Averages						
Simple	−0.254***	−0.088	−0.163***	−0.170***	−0.235***	0.207
	(0.055)	(0.064)	(0.049)	(0.034)	(0.084)	(0.170)
Group	−0.286***	−0.290	−0.205***	−0.156***	−0.184***	0.185
	(0.047)	(0.210)	(0.054)	(0.027)	(0.059)	(0.507)
Simple (covariates)	−0.318	−0.221	−0.302*	−0.308*	−0.269	0.361
	(0.222)	(0.201)	(0.172)	(0.172)	(0.353)	(0.454)
Group (covariates)	−0.210	−0.309	−0.326*	−0.227*	−0.110	0.185
	(0.206)	(0.210)	(0.170)	(0.143)	(0.234)	(0.527)
Regression						
RML	−0.451**	−0.300	−0.340**	−0.067	−0.024	0.041
	(0.181)	(0.191)	(0.178)	(0.100)	(0.272)	(0.230)
RML (covariates)	−0.379**	−0.380*	−0.324**	−0.107	−0.129	−0.047
	(0.184)	(0.197)	(0.153)	(0.081)	(0.247)	(0.226)
N	891	885	905	905	894	856

Notes: Callaway and Sant'Anna estimates obtained using the outcome-regression approach, using never-treated units as the comparison group, and one period of anticipation. The treatment groups are 2012 (Colorado and Washington), 2014 (Oregon), 2015 (Alaska and Washington, DC), 2016 (California, Maine, and Massachusetts), and 2017 (Nevada). Group averages are for unconditional estimates. Covariates include state-level age distribution (proportion between 0−17, 18–24, 25–44, 45 and beyond), proportion female, racial composition (proportions Hispanic, black, and white), education distribution (proportion below high school, proportion with high school, proportion with Bachelor's degree and beyond), median household income, unemployment rate, poverty rate, population, alcohol consumption rate and an indicator for medical marijuana legalization. Covariates are measured in the comparison period for the Callaway and Sant'Anna estimates and are time varying for the regression estimates. Standard errors, reported in parentheses, are clustered on state (for the Callaway and Sant'Anna estimates, the standard errors are based on a cluster bootstrap; when we bootstrapped the estimates more than once to obtain multiple confidence bands, we report the second set of bootstrap standard errors).

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

to parental drug abuse for every single treatment group. The CS estimates suggest average declines in entries of between 20% and 30%, depending on whether covariates are included and how the average is taken, while the traditional regression estimates (which are less precise) suggest average declines of around 40%.

While these results imply that RML decreases entries due to parental drug abuse, our data do not specify which drug's use resulted in the removal of the child, precluding us from determining whether these declines result from changes in the legal status of marijuana or from changes in the use of drugs for which marijuana is a substitute. As we note in the introduction, there is evidence that marijuana and harder drugs are substitutes. For example, Chu (2015) finds that, in the aftermath of MML laws, arrests for hard-drug use decline, as do heroin-related admissions to treatment centers. To provide evidence on substitution effects in the foster-care context, we use the Treatment Episode Data Set: Admissions (TEDS-A, Substance Abuse and Mental Health Services Administration, 2019; the same treatment admissions data used in Chu, 2015) to calculate the (logs of) state-by-year admissions to addiction treatment centers for the use of marijuana, cocaine, heroin, opioids, methamphetamines, and benzodiazepines.[16] We then estimate traditional difference-in-difference regressions that include these admissions variables as proxies for the local time-varying demands for different drugs (in addition to our other time-varying covariates).[17] The results, presented in Appendix Table A3 for brevity, do provide some indirect evidence of substitution effects. In particular, after conditioning on drug-specific treatment-center admissions, the estimated effect of RML on entries due to parental drug abuse declines by about five log points, corresponding to a reduction of about 13% in the estimated treatment effect. However, since treatment-center admissions may be noisy proxies for drug use (e.g., because the population seeking treatment may differ from that using drugs or at risk of having their children removed), we hesitate to draw from these results firm conclusions about the extent to which substitution toward marijuana explains declining entries for parental drug abuse.

There is also evidence that marijuana substitutes for alcohol and that, in states that legalize marijuana, alcohol consumption decreases (Anderson et al., 2013; Dragone et al., 2019). Furthermore, the AFCARS data do report removals for parental alcohol abuse separately from other forms of parental drug use, enabling us to provide some direct evidence on the relative importance of substitution effects. The second panel of Figure 2 summarizes the dynamic effects for parental alcohol abuse. The pre-legalization estimates closely mirror those for parental drug abuse, with small and statistically insignificant differences between RML and non-RML states. After legalization, entries for parental alcohol abuse appear to decrease, although by less, and more slowly, than for parental drug abuse. The difference-in-difference estimates in Table 3 show that all of the group-specific average effects are negative, although they are only statistically significant for two of the treatment groups. The CS summary measures imply (insignificant) average declines of between around 10% and 30%, while the regression estimates imply average declines of between 30% and 40% (when, in Appendix Table A4, we allow for three periods of anticipation, we find that this effect is statistically significant, although the corresponding estimate for parental neglect, which we discuss below, is not). Although these estimates are less precise than those for parental drug abuse, they do imply that substitution from other substances toward marijuana can be an important part of the mechanism through which RML impacts foster-care placements.

As we note in the introduction, it is unclear whether the use of marijuana per se represents a harm to the welfare of children. The next two outcomes that we study provide evidence on the effects of RML on outcomes that are unequivocal correlates of child welfare. Panel (c) of Figure 2 plots the dynamic effects for log placements due to physical abuse. As the plot shows, there are no significant pre-legalization differences between RML and non-RML states in entries due to physical abuse. After legalization, there is an immediate and pronounced decline. The corresponding difference-in-differences estimates in Table 3 show that these dynamic effects correspond to large average declines in entries due to physical abuse. The simple and group CS average declines range from 16% to 33%, consistent with traditional regression estimates in excess of 30%. Although there is little evidence that the use of marijuana itself leads to violence, our estimate confirms Rashid and Waddell's (2018) finding that legalization of medical marijuana led to a decrease in child maltreatment, mainly through physical abuse. One possible explanation for this effect is that legalization causes some parents to substitute toward marijuana from drugs that are associated with violent behavior. Regardless of the underlying reason, this finding suggests that legalization has direct positive implications for child welfare.

Panel (d) of Figure 2 plots the dynamic effects for entries due to parental neglect, which as we document in Table 1 is the primary route of entry into the foster-care system. There is no systematic evidence of pre-legalization divergence between RML and non-RML states in entries due to neglect (with the exception of the period immediately before legalization) and clear evidence of negative divergence after legalization. The difference-in-differences estimates mirror this finding, showing significantly negative (though heterogenous) declines for every group, with CS average estimates of between 16% and 31%, depending on the summary measure and whether covariates are included. Although less precise, the traditional regression estimates indicate declines in entries due to parental neglect as well.[18]

One of the major arguments in favor of legalizing marijuana is that doing so will reduce arrests for marijuana possession. The penultimate panel of Figure 2 plots the dynamic effects of RML on parental incarceration. Like the other outcomes analyzed in the figure, the plot shows that there are no systematic pre-legalization differences between RML and non-RML states in entries due to parental incarceration, and significant relative declines for RML states in some post-legalization periods. The CS estimates in Table 3 show that RML led to statistically significant declines in entries due to parental incarceration among three of the treatment groups, with unconditional simple and group-average declines of about 24% and 18%. The conditional CS averages and traditional regression estimates imply comparable (though statistically insignificant) declines. Although these estimates are consistent with the hypothesis that legalization decreases foster-care entries in part by reducing the likelihood that parents are incarcerated for reasons related to marijuana use, because the AFCARS does not disaggregate these entries by the crimes for which parents were incarcerated, it is unclear whether these effects represent decreases in arrests due to marijuana possession, possession of other drugs, or for other reasons that may be related to RML. Moreover, the anecdotal evidence presented in the introduction (Secret, 2011) suggests that children are sometimes placed in foster care due to instances of parental marijuana possession that do not result in prosecution for drug crimes.

As we discuss in the introduction, the literature on the effect of marijuana legalization on the use of marijuana by children and teens is inconclusive. In the last panel of Figure 2, we plot the dynamic effects for entries due to child drug abuse. The figure suggests that there are no systematic differences between RML and non-RML states in years prior to legalization. Post legalization, however, entries due to child drug abuse appear to increase, although these increases are relatively small and statistically insignificant, and disappear by the fourth year of legalization. The difference-in-differences estimates reflect this ambiguity. For two of the treatment groups, we find significant increases in entries for child drug abuse, while the effects for the remaining groups are insignificant, and in some cases, negative. Moreover, none of the CS averages or traditional regression estimates suggest that RML systematically affects entries due to child drug abuse in either direction. Our interpretation of these estimates is that the effect of RML on entries for child drug abuse is heterogenous, making it difficult to draw firm conclusions about the average effect of RML on entries via this route.

4.4 | Treatment-effect heterogeneity

Children enter foster care at different ages. Children younger than 6 years spend most of their time with their parents or caregivers since they are not of school-going age in most states. Thus, any effect of parental marijuana use on children may be more pronounced on children within that age category, although their parents may also be less likely to report maltreatment or abuse. On the other hand, older children encounter teachers and other people who are more likely to report any form of abuse to child protective services than children who are not yet in school. To test for heterogeneity in the effect of RML on total and route-specific foster-care admissions, we partition our sample into children five or younger, those between the ages of six and ten, and those older than ten.

We present results for total entries and key entry routes for the three age categories in the top panel of Table 4 (for brevity, we only report simple CS averages and regression estimates, without covariates). The point estimates show declines in foster-care entries for all three age groups, although the estimates are generally larger and more precise for children five and younger. This pattern also holds for entries due to parental drug abuse, parental alcohol abuse, physical abuse, parental neglect and parental incarceration. For child drug abuse, the CS averages indicate a large positive effect for children between the ages of six and ten, but insignificant effects otherwise, while none of the regression estimates evince any effects in either direction.

One potential implication of the broad similarity of the estimated effects for younger children, who spend more time at home with caretakers, and older children, who spend more time at school with teachers, is that RML does not have an appreciable impact on the typical parties responsible for initiating referrals to CPS. Because the AFCARS does not disaggregate foster-care entries by referring party, however, this is only suggestive. To provide additional evidence on this point, in Table 5 we present difference-in-difference estimates obtained from samples of entries into foster care that took place during non-school months (i.e., excluding either June and July or those months in addition to August).[19] For total entries, the point estimates are essentially identical to the corresponding estimates obtained using the full sample (Tables 2 and 3), which further suggests that the impact of RML on foster-care entries does not operate primarily through changes in the parties who refer children to CPS. We also estimate gender- and race-specific effects of RML on entry into foster care. The results, summarized in the bottom panel of Table 4, show that the effects of RML on total entries, as well as most of the specific routes of entry, are more pronounced for girls (although this is not true in every case, and the gender-specific point estimates are generally within sampling error of one another). Across all routes of entry, the race-specific estimates are generally similar as well, although for total entries the point estimates for Blacks

TABLE 4 RML and foster-care entries by gender, race and age

	Total entries	Parental drug abuse	Parental alcohol abuse	Physical abuse	Parental neglect	Parental incarceration	Child drug abuse
(a) Age							
Callaway and Sant'Anna							
≤ 5	−0.153***	−0.289***	−0.182***	−0.052***	−0.207***	−0.256***	0.075
	(0.045)	(0.059)	(0.122)	(0.016)	(0.033)	(0.073)	(0.281)
6–10	−0.096*	−0.237***	−0.078	−0.037*	−0.118***	−0.296***	0.571***
	(0.055)	(0.060)	(0.108)	(0.020)	(0.041)	(0.082)	(0.207)
> 10	−0.082	−0.191**	−0.053	−0.028*	−0.124***	−0.138	0.164
	(0.066)	(0.075)	(0.120)	(0.016)	(0.042)	(0.150)	(0.163)
Regression							
≤ 5	−0.158*	−0.479***	−0.336	−0.070**	−0.078	−0.070	0.057
	(0.090)	(0.178)	(0.207)	(0.032)	(0.093)	(0.278)	(0.297)
6–10	−0.165	−0.439**	−0.281	−0.065*	−0.085	−0.022	0.255
	(0.101)	(0.182)	(0.200)	(0.036)	(0.114)	(0.288)	(0.245)
> 10	−0.130	−0.309*	−0.290	−0.052	−0.030	0.113	0.082
	(0.093)	(0.161)	(0.193)	(0.034)	(0.118)	(0.245)	(0.202)
(b) Gender and race							
Callaway and Sant'anna							
Males	−0.103**	−0.232***	−0.032	−0.036***	−0.155***	−0.232***	0.264
	(0.052)	(0.066)	(0.136)	(0.014)	(0.033)	(0.080)	(0.219)
Females	−0.125**	−0.283***	−0.174	−0.036***	−0.185***	−0.241**	0.087
	(0.057)	(0.048)	(0.106)	(0.011)	(0.039)	(0.098)	(0.111)
Blacks	−0.142***	−0.241**	−0.255**	−0.031**	−0.147***	−0.117	0.328
	(0.053)	(0.116)	(0.103)	(0.014)	(0.053)	(0.108)	(0.253)
Whites	−0.113**	−0.217***	−0.080	−0.031***	−0.173***	−0.274***	−0.174
	(0.056)	(0.051)	(0.107)	(0.012)	(0.046)	(0.087)	(0.211)
Hispanics	−0.127	−0.283***	−0.119	−0.018	−0.221***	−0.422**	0.134
	(0.089)	(0.078)	(0.127)	(0.025)	(0.062)	(0.170)	(0.333)
Regression							
Males	−0.115	−0.408**	−0.221	−0.064**	−0.064	−0.017	−0.032
	(0.089)	(0.184)	(0.181)	(0.032)	(0.097)	(0.263)	(0.262)
Females	−0.151*	−0.436**	−0.378*	−0.057**	−0.071	−0.004	0.227
	(0.090)	(0.166)	(0.195)	(0.029)	(0.105)	(0.278)	(0.196)
Blacks	−0.103	−0.291*	−0.248	−0.083	−0.012	−0.028	0.154
	(0.114)	(0.197)	(0.260)	(0.060)	(0.105)	(0.207)	(0.252)
Whites	−0.062	−0.370**	−0.171	−0.034	0.020	0.069	0.192
	(0.096)	(0.155)	(0.193)	(0.030)	(0.100)	(0.314)	(0.249)

TABLE 4 (Continued)

	Total entries	Parental drug abuse	Parental alcohol abuse	Physical abuse	Parental neglect	Parental incarceration	Child drug abuse
Hispanics	−0.117	−0.282*	−0.276	−0.073	−0.049	0.141	0.246
	(0.135)	(0.154)	(0.191)	(0.059)	(0.119)	(0.263)	(0.253)

Notes: Callaway and Sant'Anna estimates represent simple averages, obtained using the outcome-regression approach, using never-treated units as the comparison group and one period of anticipation. The treatment groups are 2012 (Colorado and Washington), 2014 (Oregon), 2015 (Alaska and Washington, DC), 2016 (California, Maine, and Massachusetts), and 2017 (Nevada). Both sets of estimates assume unconditional parallel trends. Standard errors, reported in parentheses, are clustered on state (for the Callaway and Sant'Anna estimates, the standard errors are based on a cluster bootstrap; when we bootstrapped the estimates more than once to obtain multiple confidence bands, we report the second set of bootstrap standard errors).

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE 5 RML and foster-care entries during school months

	Total entries	Parental drug abuse	Parental alcohol abuse	Physical abuse	Parental neglect	Parental incarceration	Child drug abuse
Callaway and Sant'Anna							
Excluding June and July	−0.113*	−0.284***	−0.110	−0.034***	−0.183***	−0.208**	11.118
	(0.060)	(0.069)	(0.093)	(0.011)	(0.033)	(0.087)	(17.046)
Excluding June, July and August	−0.114**	−0.292***	−0.115	−0.031***	−0.184***	−0.204**	9.330
	(0.053)	(0.063)	(0.105)	(0.011)	(0.035)	(0.088)	(19.954)
Regression							
Excluding June and July	−0.135	−0.455**	−0.300	−0.056*	−0.070	−0.006	0.131
	(0.092)	(0.193)	(0.198)	(0.029)	(0.102)	(0.262)	(0.223)
Excluding June, July and August	−0.133	−0.467**	−0.314	−0.056	−0.067	−0.004	0.097
	(0.090)	(0.190)	(0.198)	(0.028)	(0.100)	(0.257)	(0.243)

Notes: Callaway and Sant'Anna estimates represent simple averages, obtained using the outcome-regression approach, using never-treated units as the comparison group and one period of anticipation. The treatment groups are 2012 (Colorado and Washington), 2014 (Oregon), 2015 (Alaska and Washington, DC), 2016 (California, Maine, and Massachusetts), and 2017 (Nevada). Both sets of estimates assume unconditional parallel trends. Standard errors, reported in parentheses, are clustered on state (for the Callaway and Sant'Anna estimates, the standard errors are based on a cluster bootstrap; when we bootstrapped the estimates more than once to obtain multiple confidence bands, we report the second set of bootstrap standard errors).

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

and Hispanics exceed those for whites regardless of how we estimate them. None of the gender- or race-specific estimates for child drug abuse are statistically significant.

4.5 | Robustness tests

While the estimates presented above relate foster-care entries to the passage of RML legislation, the opening of marijuana dispensaries may be the more relevant event against which to measure the impact of legalization, and may help explain why the effects of RML on foster-care entries appear to operate on a lag for some entry routes. To examine this possibility, we repeat our primary difference-in-differences analyses, redefining the treatment as the opening of dispensaries rather than the passage of RML (to allow for pre-opening effects of RML, we allow for three periods of anticipation when implementing CS). One challenge with this approach is that some RML states (DC, California, Maine and Massachusetts) had not opened dispensaries by the end of our sample period.

We address this problem in two ways. In panel (a) of Table 6, we present estimates that treat RML states that had not yet opened dispensaries as control states. This exercise produces conflicting results. Across all routes of entry, many of the CS point estimates are slightly smaller when treatment status is defined with respect to the opening of dispensaries. However, since there are fewer post-treatment periods, the estimates are also less precise, and the magnitudes of the estimates for entries due to parental incarceration (for which the dynamic effects exhibit a pronounced increase over time), are noticeably larger. On the other hand, the magnitudes of most of the traditional regression point estimates are larger when we define the treatment as the opening of dispensaries. In panel (b) of the table, we simply exclude RML states that had not opened dispensaries by the end of our sample period. Here we find that both the CS and traditional regression estimates tend to be smaller when the treatment is defined with respect to dispensaries. Thus, while there is some evidence that dispensary openings can be more relevant (as in the case of entries due to parental incarceration), we cannot rule out the possibility that it is legalization itself that matters most for foster-care entries (this is consistent

TABLE 6 Dispensary openings and foster-care entries

	Total entries	Parental drug abuse	Parental alcohol abuse	Physical abuse	Parental neglect	Parental incarceration	Child drug abuse
(a) All states							
Callaway and Sant'Anna							
Group-specific							
2014	−0.153***	−0.205***	−0.190	−0.024	−0.198***	−0.317***	−0.031
	(0.044)	(0.088)	(0.166)	(0.015)	(0.050)	(0.067)	(0.173)
2016	0.063	−0.164	0.203***	−0.003	−0.076	−0.471***	0.876***
	(0.162)	(0.065)	(0.075)	(0.014)	(0.044)	(0.074)	(0.104)
2017	−0.089***	−0.536***	−0.462***	−0.60***	−0.111***	−0.263***	−0.051
	(0.026)	(0.077)	(0.100)	(0.009)	(0.042)	(0.078)	(0.087)
Averages							
Simple	−0.082	−0.228***	−0.143	−0.033	−0.168***	−0.340***	−0.132
	(0.079)	(0.081)	(0.144)	(0.021)	(0.053)	(0.086)	(0.199)
Group	−0.054	−0.278***	−0.160*	−0.053***	−0.146***	−0.342***	0.191
	(0.070)	(0.073)	(0.095)	(0.016)	(0.044)	(0.068)	(0.147)
Regression							
RML	−0.181	−0.602**	−0.357**	−0.054**	−0.161	−0.165	−0.137
	(0.119)	(0.250)	(0.177)	(0.026)	(0.143)	(0.246)	(0.290)
N	918	891	885	905	905	894	856
(b) Omitting DC, CA, ME and MA							
Callaway and Sant'Anna							
Group-specific							
2014	−0.168***	−0.228***	−0.197	−0.028	−0.214***	−0.362***	−0.034
	(0.043)	(0.083)	(0.160)	(0.015)	(0.049)	(0.070)	(0.177)
2016	0.047	−0.199***	−0.196***	−0.004	−0.094**	−0.526***	0.851***
	(0.169)	(0.064)	(0.082)	(0.014)	(0.043)	(0.067)	(0.117)
2017	−0.107***	−0.563***	−0.461***	−0.163***	−0.133***	−0.290***	−0.041
	(0.027)	(0.087)	(0.112)	(0.010)	(0.042)	(0.079)	(0.103)
Averages							
Simple	−0.097	−0.254***	−0.150	−0.036	−0.185***	−0.385***	−0.126
	(0.079)	(0.079)	(0.163)	(0.022)	(0.052)	(0.075)	(0.207)
Group	−0.070	−0.305***	−0.165	−0.056***	−0.164***	−0.385***	0.185
	(0.075)	(0.070)	(0.096)	(0.017)	(0.046)	(0.070)	(0.145)

TABLE 6 (Continued)

	Total entries	Parental drug abuse	Parental alcohol abuse	Physical abuse	Parental neglect	Parental incarceration	Child drug abuse
Regression							
RML	−0.075	−0.380**	−0.276	−0.042	−0.022	−0.076	−0.117
	(0.094)	(0.154)	(0.188)	(0.029)	(0.099)	(0.302)	(0.239)
N	846	819	813	833	833	822	784

Notes: Callaway and Sant'Anna estimates represent simple averages, obtained using the outcome-regression approach, using never-treated units as the comparison group and three periods of anticipation. The treatment groups are 2012 (Colorado and Washington), 2014 (Oregon), 2015 (Alaska and Washington, DC), 2016 (California, Maine, and Massachusetts), and 2017 (Nevada). Both sets of estimates assume unconditional parallel trends. Standard errors, reported in parentheses, are clustered on state (for the Callaway and Sant'Anna estimates, the standard errors are based on a cluster bootstrap; when we bootstrapped the estimates more than once to obtain multiple confidence bands, we report the second set of bootstrap standard errors). Panel (a) includes DC, CA, MA and ME as control states while panel (b) omits them entirely.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

with our finding that, for many routes, the effects of RML begin to materialize immediately after, or in some cases slightly before, legalization).

The dynamic effect analyses for overall and route-specific entries into foster care presented above strongly support the hypothesis that the legalization of recreational marijuana is exogenous with respect to other factors that affect foster-care entry. However, this type of evidence can only ever be suggestive – the possibility remains that legalization and the concomitant decline in foster-care entries are both consequences of unobserved and confounding forces that disproportionately affected RML states. As a further test of the validity of our results, we assess the effects of RML on foster-care entries through routes which we believe are unlikely to be related to marijuana use, but would presumably be influenced by unobserved factors that affect both legalization and marijuana-related foster-care entries.

Specifically, we examine the effect of legalization on entries into foster care due to child alcohol abuse, child disability, child relinquishment, insufficient housing, abandonment by parent, and sexual abuse.[20] Figure 3 presents the relevant dynamic effects plots. These plots show that entries for each of the falsification routes that we examine evolve similarly in RML and non-RML states prior to legalization. Moreover, none of the plots suggest a clear post-RML divergence in entries among RML states. For entries due to child alcohol abuse, child disability, child relinquishment and sexual abuse, neither of the CS averages reported in Table 7 are statistically significant (while some of the group-specific average effects are significant, they tend to have different signs for different treatment groups, which cancel out upon averaging over groups).[21] For entries due to housing insufficiency and abandonment, the CS estimates do suggest significantly negative effects. However, close examination of the dynamic effects plots in Figure 3 reveals that, for both of these routes, entries 2 years before RML tend to be higher in RML states than in non-RML states relative to the previous year, potentially contaminating the CS estimates for these outcomes. When we re-estimate the effects of RML on these routes allowing for two periods of anticipation, the estimated simple and group averages are small and statistically insignificant. At the same time, none of the traditional regression estimates for these routes are statistically significant. Supporting our interpretation of the preceding estimates as causal effects, the results of these placebo tests suggest that our results cannot be explained by unobserved factors that coincide with legalization. Consistent with the dynamic-effect analyses presented above, nor do they suggest that our results can be explained by pre-legalization divergence in rates of foster-care entry between RML and non-RML states.

The evidence presented in Table 7 also suggests that our key empirical findings cannot be explained by differential reporting to CPS. If, following legalization, would-be referrers became less likely to report instances of marijuana use, total entries due to marijuana use itself would fall mechanically, and the resulting reduction in investigations from CPS might also reduce entries due to other circumstances considered detrimental to child welfare. Because the AFCARS documents removals, and not reports, we are unable to measure changes in reporting due to RML. However, if the effect of reporting were large (and other welfare threats were more or less uncorrelated with marijuana use), we would expect to find reductions in entries through all routes. As Table 6 shows, we do not. Instead, we find declines in entries for reasons that are credibly related to marijuana use and little change in entries for those that are not.[22]

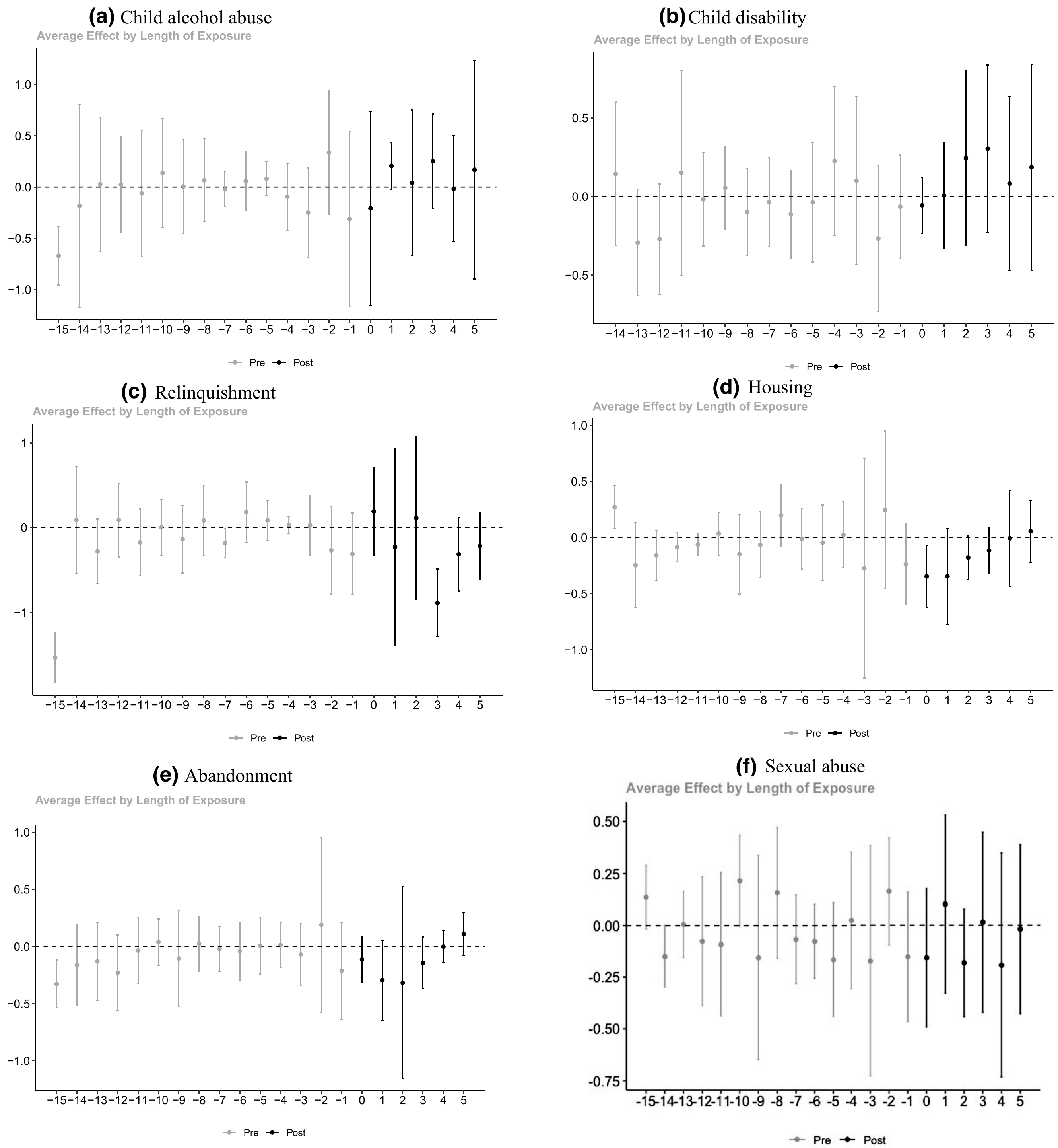

FIGURE 3 Dynamic effects of RML on falsification outcomes. Callaway and Sant'Anna (2020) dynamic treatment effects, obtained using one period of anticipation, without conditioning on covariates. Bars show 95% confidence intervals, after clustering at the state level

5 | CONCLUSION

Some states' legalization of marijuana for recreational use represents a drastic departure from existing policies toward the drug. While legalization has attracted interest from researchers, policy makers, and health officials, among others, we are still in the early stages of understanding its social, economic and public-health consequences. This paper contributes to that understanding by analyzing the effect of recreational-marijuana legalization on entries into the

TABLE 7 RML and falsification outcomes

	Child alcohol abuse	Child disability	Child relinquishment	Housing	Abandonment	Sexual abuse
Callaway and Sant'Anna						
Group-specific						
2012	0.050	0.113	−0.532***	−0.102	−0.005	−0.123
	(0.178)	(0.183)	(0.142)	(0.085)	(0.068)	(0.176)
2014	0.422**	0.220	0.823***	−0.288***	−0.300***	0.013
	(0.173)	(0.160)	(0.085)	(0.079)	(0.043)	(0.040)
2015				−0.737***	−0.538***	0.157***
				(0.078)	(0.055)	(0.050)
2016	0.104	−0.058	−0.246	−0.208***	−0.216	0.041
	(0.166)	(0.125)	(0.263)	(0.057)	(0.231)	(0.119)
2017	−1.740***		1.030***	−0.305***	−0.279***	−0.935***
	(0.104)		(0.057)	(0.039)	(0.055)	(0.043)
Averages						
Simple	0.046	0.086	−0.073	−0.236**	−0.171*	−0.063
	(0.169)	(0.128)	(0.295)	(0.094)	(0.102)	(0.102)
Group	−0.168	0.045	0.097	−0.270***	−0.222**	−0.111
	(0.152)	(0.097)	(0.191)	(0.079)	(0.094)	(0.101)
Simple (covariates)	0.189	0.725	0.670	−0.414	−0.167	−0.191
	(0.500)	(0.486)	(0.862)	(0.262)	(0.198)	(0.420)
Group (covariates)	0.360	0.570	0.824	−0.384*	−0.207	−0.118
	(0.521)	(0.374)	(0.616)	(0.232)	(0.196)	(0.206)
Regression						
RML	0.151	0.010	0.223	−0.227	−0.097	−20.548
	(0.186)	(0.337)	(0.314)	(0.272)	(0.165)	(63.615)
RML (covariates)	0.047	0.034	0.208	−0.210	−0.030	−0.131
	(0.199)	(0.300)	(0.301)	(0.191)	(0.133)	(0.111)
N	827	862	847	871	896	904

Notes: Callaway and Sant'Anna estimates obtained using the outcome-regression approach, using never-treated units as the comparison group, and one period of anticipation. The treatment groups are 2012 (Colorado and Washington), 2014 (Oregon), 2015 (Alaska and Washington, DC), 2016 (California, Maine, and Massachusetts), and 2017 (Nevada). Group averages are for unconditional estimates. Covariates include state-level age distribution (proportion between 0–17, 18–24, 25–44, 45 and beyond), proportion female, racial composition (proportions Hispanic, black, and white), education distribution (proportion below high school, proportion with high school, proportion with Bachelor's degree and beyond), median household income, unemployment rate, poverty rate, population, alcohol consumption rate and an indicator for medical marijuana legalization. Covariates are measured in the comparison period for the Callaway and Sant'Anna estimates and are time varying for the regression estimates. Standard errors, reported in parentheses, are clustered on state (for the Callaway and Sant'Anna estimates, the standard errors are based on a cluster bootstrap; when we bootstrapped the estimates more than once to obtain multiple confidence bands, we report the second set of bootstrap standard errors).

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. With two periods of anticipation, the unconditional simple average for housing is 0.113 (0.317) and the group average is −0.028 (standard error 0.102), the simple average for abandonment is 0.113 (0.344) and the group average is −0.033 (0.156).

foster-care system. Foster-care admissions are a useful measure of the welfare consequences of legalization for several reasons. They reflect the potential harms and benefits of legalization for children, a population with little control over their exposure to marijuana and other drugs. Because children are only placed in foster-care when their wellbeing is thought to be in serious jeopardy, foster-care admissions are also substantively related to child welfare.

Using a difference-in-differences design that exploits state-level variation in the adoption of recreational marijuana laws and the timing of that adoption, we find that legalization decreases placements in foster care by at least 10%. Placebo tests strongly support our interpretation of our estimates as causal.

Although the welfare implications of this finding speak for themselves, we note that the declines in foster-care placements that we estimate are not trivial. In Table 1, we show that the average state places about 5400 children in foster-care per year. Coupled with Zill's (2011) estimate that the average administrative and maintenance costs for a single placement are about \$25,000, a 10% reduction in admissions implies that nationwide legalization would reduce the financial burden of the foster-care system by about \$675 million, annually.

Legalization may impact foster-care admissions directly by changing the welfare of children or indirectly by changing policies and attitudes toward marijuana use in the home. Direct effects may arise because marijuana use itself causes behaviors that affect child welfare, or because it changes the likelihood of using other drugs. We find that placements due to parental drug abuse decrease substantially after legalization, although our data do not reveal whether this effect is due to the use of marijuana or other drugs. However, we find a similar decrease in placements due to parental alcohol abuse, which implies that substitution is at least one of the mechanisms through which legalization affects foster-care placements. We also find that placements due to physical abuse, parental neglect, and parental incarceration decrease after legalization, providing evidence that legalization reduces substantive threats to child welfare, although the precise mechanism behind these effects is unclear. The other outcomes that we examine are apparently unaffected by legalization, and we find no systematic evidence of increases in foster-care placements for any reason consequent to legalization. Further research is needed to understand whether the short-to medium-run effects that we estimate also operate in the long run.

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We thank the Co-Editor and two anonymous reviewers for their thoughtful comments and suggestions.

ENDNOTES

[1] Over 80% of entries into foster care are through the court system.

[2] D.C. RML prohibits the sale of marijuana by individuals or dispensaries.

[3] Submissions to AFCARS were voluntary until 1998, when states were mandated to submit (see Cunningham & Finlay, 2012). All 50 States and DC submitted to the AFCARS between 2000 and 2017, which gives us a balanced panel. In the cases of Alaska and New York, there is no data on the reasons for removal from 2000 to 2004 and 2000 to 2007, respectively. Illinois also has some years of data missing for the reasons for removal.

[4] Since some children enter the foster-care system for multiple reasons, the sum of route-specific entries exceeds the total number of entries.

[5] The fiscal year for AFCARS starts in October and ends in September.

[6] Overall, eight states and DC had legalized marijuana within the sample period. Illinois, Vermont and Michigan legalized marijuana after 2017. The District of Columbia has legalized marijuana consumption but not retail sales.

[7] Because there is evidence that legalization in RML states spills over to non-RML states (Hansen, et al., 2017; Hao & Cowan, 2020), this choice of treatment group may attenuate our causal effect estimates, in which case they can be interpreted as lower bounds on the true declines. We thank an anonymous reviewer for noting this.

[8] We obtained these estimates using the R package *did* (Callaway & Sant'Anna, 2020b).

[9] The conditional CS approach requires not just that parallel trends holds conditional on pre-treatment covariates, but that the covariate distributions for the treatment and control groups overlap in the sense that probability of membership in each group is strictly less than one conditional on the covariates and membership in either that group or the control group. However, our outcomes and covariates are measured at the state×year level and our groups are relatively small (consisting of three states or fewer), which can present challenges for overlap. CS can also be implemented semi-parametrically by reweighting observed changes in outcomes according to this generalized propensity score; attempting to implement this in our case results in generalized propensity scores that perfectly predict group membership, suggesting overlap violations and precluding us from using the semiparametric approach. Instead, our conditional CS estimates use regressions that model the change in untreated outcomes as a linear function of the pre-treatment covariates. However, this approach necessarily entails some extrapolation between control and treatment states when overlap is limited.

[10] The treatment groups are 2012 (Colorado and Washington), 2014 (Oregon), 2015 (Alaska and Washington, DC), 2016 (California, Maine, and Massachusetts), and 2017 (Nevada).

[11] In Table A4, we assess the sensitivity of our estimates to our implementation choices. As the table shows, we obtain similar estimates when we allow for three, rather than two, periods of anticipation, and when we use the not-yet-treated group (i.e., states that never pass

RML as well as those that do eventually, but have not yet) as the comparison group for states that have already passed RML, rather than the never-treated group alone.

12 We implemented this decomposition using the Stata package *BACONDECOMP* by Goodman-Bacon et al. (2019).

13 Similar adjustments have been required in states with MML. For example, according to the article by Secret (2011) cited in the introduction: "California, where the medical marijuana movement has flourished, now requires that child welfare officials demonstrate actual harm to a child from marijuana use in order to bring neglect cases, and defense lawyers there say the authorities are now bringing fewer of them." On the other hand, there have also been numerous cases of removals due to parental use of marijuana in states with RML or MML (see Gentry, 2018; Schulte, 2015, e.g.,).

14 Indeed, since no state experiences RML for longer than 5 years in our sample, further research is needed to truly assess the long-run effects of legalization.

15 Route-specific entries are missing in early sample years for three states (Alaska, Illinois and New York). We obtain similar results (not reported) when we exclude these states from the estimation sample.

16 We thank an anonymous referee for suggesting that we use the TEDS data.

17 We use the traditional regression approach for this exercise because it involves conditioning on time-varying covariates.

18 An anonymous reviewer has suggested that RML may also impact child welfare (and hence foster-care admissions) by increasing access to social-welfare programs from which marijuana-using parents might otherwise risk disqualification. For the Supplemental Nutritional Assistance Program (SNAP, formerly "food stamps"), this is unlikely to be an important channel because the US Department of Agriculture does not generally allow states to screen applicants for drug use (although there are exceptions to this policy, they have not been exercised; see McCarty et al., 2016). On the other hand, many states do require drug screening for at least some applicants (e.g., those with prior drug convictions) to the Temporary Assistance for Needy Families (TANF, commonly referred to as "welfare") program. Among states with RML, however, only Colorado and Maine have drug-screening policies in place, and neither policy is especially stringent (screening is determined on a county-by-county basis in CO and only applies to those with drug-related felony convictions in ME; see McCarty et al., 2016). Thus, while this channel is unlikely to be driving our results, it may become relevant as more states legalize recreational marijuana.

19 Unfortunately, our data only specify when the removal took place, not the initial referral. We thank an anonymous reviewer for suggesting this test.

20 Some of these outcomes (such as child alcohol abuse and sexual abuse) could reasonably be considered routes that might be affected by legalization. We find that they are unaffected by RML, regardless of whether they are viewed as placebo outcomes or those potentially affected by legalization.

21 Group-specific averages are missing for some outcomes because not every state reports entries through every route.

22 On the other hand, if legalization increases reporting (e.g., if parents become less secretive about using marijuana), our estimates can be interpreted as lower bounds on RML-induced decreases in entries.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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APPENDIX

TABLE A1 Year of Implementation of RML by States

State	RML year	Opening of dispensaries
Alaska	2015	2016
California	2016	
Colorado	2012	2014
DC	2015	
Maine	2016	
Massachusetts	2016	
Nevada	2017	2017
Oregon	2014	2016
Washington	2012	2014

TABLE A2 Goodman-Bacon decomposition

	Weight	Avg. DD estimate
Treated earlier versus later	0.053	−0.173
Treated later versus earlier	0.008	−0.021
Treated versus never treated	0.939	−0.131

TABLE A3 RML and foster care: Controlling for treatment-center admissions

	Total entries	Parental drug abuse	Parental alcohol abuse	Physical abuse	Parental neglect	Parent_ incarceration	Child drug abuse
(a) All admissions							
RML	−0.1320*	−0.3324*	−0.3103*	−0.2417**	−0.1313*	−0.2408	−0.0460
	(0.0699)	(0.1886)	(0.1593)	(0.1027)	(0.0708)	(0.1859)	(0.1840)
N	898	872	866	886	886	894	856
(b) 18 years and older							
RML	−0.1327*	−0.3298*	−0.3066*	−0.2409**	−0.1317*	−0.2387	−0.2387
	(0.0695)	(0.1861)	(0.1575)	(0.1020)	(0.0705)	(0.1826)	(0.1826)
N	898	872	866	886	886	894	856

Notes: Dependent variable is the log of entries for the indicated route. Controls include state-level age distribution (proportions between 0–17, 18–24, 25–44, 45 and beyond), proportion female, racial composition (proportion Hispanic, black and white), education distribution (proportion below high school, proportion with high school, proportion with Bachelor's degree and beyond), median household income, unemployment rate, poverty rate, population, alcohol consumption rate, and an indicator for medical marijuana legalization. Standard errors, clustered on state, are in parenthesis. TED-A variables include log of individuals who are admitted for use of marijuana, alcohol, cocaine, heroin, opioids, methamphetamines, and benzodiazepines. Panel (a) includes all admissions while panel (b) excludes only includes admissions among those 18 years or older. Standard errors, clustered on state, are in parenthesis.

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

TABLE A4 Robustness tests

Total entries	Parental drug abuse	Parental alcohol abuse	Physical abuse	Parental neglect	Parental incarceration	Child drug abuse
Three periods of anticipation						
−0.154*	−0.395***	−0.459***	−0.545*	−0.162**	−0.068	0.071
(0.085)	(0.153)	(0.276)	(0.326)	(0.077)	(0.409)	(0.348)
Never-treated group (with 1-period anticipation)						
−0.113**	−0.254***	−0.085	−0.163***	−0.170***	−0.235***	0.207
(0.055)	(0.055)	(0.108)	(0.052)	(0.034)	(0.078)	(0.170)

Notes: Callaway and Sant'Anna simple averages, obtained using the outcome-regression approach. Top panel uses three periods of anticipation and the never-treated group; bottom panel uses one period of anticipation and the not-yet-treated group. The treatment groups are 2012 (Colorado and Washington), 2014 (Oregon), 2015 (Alaska and Washington, DC), 2016 (California, Maine, and Massachusetts), and 2017 (Nevada). Both sets of estimates assume unconditional parallel trends. Standard errors, reported in parentheses, are clustered on state (for the Callaway and Sant'Anna estimates, the standard errors are based on a cluster bootstrap; when we bootstrapped the estimates more than once to obtain multiple confidence bands, we report the second set of bootstrap standard errors).

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.