Child Maltreatment Referrals and Mandatory Reporting Laws

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Abstract

In 2017, an estimated 7.5 million children in the United States were referred to Child Protective Services for maltreatment, an increase of 1.7 million children (29%) from 2007. A referral can be made by anyone, but is mandated for certain professions. These "mandatory reporters" vary by state and over time. I create a state panel of mandatory reporter job classifications, child maltreatment referrals and reports, case disposition, and child population from 2004 to 2017. The mandatory reporter job classifications come from state mandatory reporting laws, and the child maltreatment data come from the National Data Archive on Child Abuse and Neglect. I use a two-way fixed effects model to estimate how changes in mandatory reporters correspond to changes in child maltreatment referrals and reports. I do not find any evidence that changes in mandatory reporting legislation impacts child maltreatment referrals. However, findings suggest that increasing the number of jobs classified as mandatory reporters increases screened-in reports by 4 percent in the long-run. Most of these changes are driven by changes in unsubstantiated reports.

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1. Introduction

In 2017, an estimated 7.5 million children in the United States were referred to Child Protective Services for maltreatment, an increase of 1.7 million children (29%) from 2007 (ACF, 2008; ACF, 2019). Child Protective Services (CPS) is the government agency in each state accountable for children's well-being with the main responsibility of responding to child maltreatment referrals. The referral is the first step in a lengthy process to substantiation. A referral can be made by anyone, but is mandated for certain professions. These "mandatory reporter" classifications vary by state and over time. In 2004, the most common reporters were teachers (19%), law enforcement (18%), and social services staff (11%). In 2017, the most common reporters were education (20%), legal (20%), social services (12%), and medical (11%) personnel. After a referral is made, it is screened in or out based on state criteria. Consistent with the child welfare literature, I refer to referrals that are screened-out as "referrals screened-out" and referrals that are screened-in as "reports." There is no additional follow-up for referrals that are screened-out, but reports are then investigated for child maltreatment. A caseworker visits the family and observes the living environment to determine a disposition. A report is substantiated if there is enough evidence to prove maltreatment occurred, and unsubstantiated otherwise. The purpose of this paper is to understand how child maltreatment reporting responds to changes in mandatory reporter legislation.

Observed child maltreatment is a function of the interaction between maltreatment and reporting. The true nature of maltreatment is unknown and widely regarded as underreported (MacMillan et al., 2003; Fallon et al., 2010; GAO, 2011; National Research Council, 2014). In response, there has been considerable effort trying to identify behaviors and circumstances that are predictive of maltreatment.

Poor economic conditions such as unemployment, low-income, and inadequate housing are correlated with increases in child abuse and neglect (Weinberg, 2001; Paxson & Waldfogel, 2002; Slack et al., 2003; Seiglie, 2004; Lindo et al., 2013; Warren & Font, 2015; Berger et al., 2017; Lindo et al., 2018; Brown & De Cao, 2020). A randomized control trial in Delaware and an evaluation of the Welfare Reform Act of 1996 found that less generous welfare programs increase child maltreatment (Fein & Lee, 2003; Paxson & Waldfogel, 2003). Alternatively, safety net programs can reduce the nature of child maltreatment. Berger and colleagues (2017) show that an increase in income from the earned income tax credit reduces CPS's involvement, Raissian &

Bullinger (2017) find that increasing the minimum wage reduces child maltreatment, and Brown & De Cao (2020) show that states with longer durations of unemployment insurance had fewer maltreatment reports during the Great Recession. Additionally, more generous child support payments reduce the number of screened-in reports of maltreatment (Cancian et al., 2013).

Environmental factors, such as availability of services, substance abuse, and temperature, might also impact child maltreatment. Access to abortions have been found to reduce child maltreatment (Bitler & Zavodny, 2004; Seiglie, 2004). Substance abuse, regardless of the drug, impacts parental functioning and places a child at risk of maltreatment (Wells, 2009). While there is no evidence relating temperature to child maltreatment,[1] there is emerging evidence that hotter temperatures increase violence (Anderson et al., 2000; Anderson, 2001; Almas et al., 2019; Heilmann & Kahn, 2019). Lastly, family structure contributes to maltreatment. Specifically, changes in family structure to single parenting or blended families (step parents and siblings) is associated with abuse and neglect (National Research Council, 2014; Schneider, 2016).

Since the underlying causes of maltreatment have been investigated in great detail, this paper focuses on the second factor that determines the number of maltreatment investigations, reporting. Reporting is a function of social norms around reporting maltreatment and the number of mandatory reporters. Recently, there has been increased interest in the role mandatory reporters play detecting child maltreatment, especially school teachers.[2] However, there is limited conclusive research around mandatory reporting laws and referrals. On one hand, universal reporting laws[3] are associated with higher report rates for abuse and neglect (Krase & DeLong-Hamilton, 2015; Palusci et al., 2016). Alternatively, Ho et al. (2017) conclude that child maltreatment reporting did not differ across states with and without universal mandatory reporting laws in 2013. These papers make comparisons across counties and states, so their findings are not necessarily causal. Instead these differences may be confounded by demographic, economic, and cultural differences across places.

[1] One notable exception is Jessica Pac's third dissertation chapter (Pac, 2019).
[2] Recent working papers that discuss the role of teachers in detecting child maltreatment early include Fitzpatrick et al. (2020) and Cabrera-Hernandez & Padilla-Romo (2020).
[3] Universal reporting laws require all persons to report suspected maltreatment regardless of their profession.

I contribute to this body of research by investigating the effect of changing mandatory reporter laws over a 14-year period in the United States on child maltreatment referrals and reports. Specifically, I answer the following three research questions:

1. How has mandatory reporter legislation changed over time?
2. What is the relationship between changes in mandatory reporter legislation and child maltreatment referrals and reports?
3. Do different professions have different impacts?

Mandatory reporting laws designate professions that are required to report child abuse or neglect and establish the reporting process, such as who to call and what details to provide. Some legislation adds new job classifications to the list of mandatory reporters, whereas other legislation provides clarification about making a report and privileged communication or adds training requirements. I use changes in the list of mandatory reporters from 2004 to 2017 to estimate how increasing the number of professions designated as mandatory reporters impacts child maltreatment reporting.[4]

The predicted change in maltreatment referrals and reports is theoretically ambiguous, but empirically testable and important. Behavioral responses from both reporters and perpetrators are possible explanations for long-run changes in reporting. As more people understand the dangers of child maltreatment, recognize it as a public concern, and better understand their and CPS's role, then the quantity of referrals and reports may increase, holding the true amount of child maltreatment constant. Better identification of child maltreatment would be indicated by increased substantiation rates and is important in detecting the true nature of maltreatment. Alternatively, the bystander effect may explain a decrease in reporting, holding child maltreatment constant.[5] Lastly, if increasing reporting and awareness of child maltreatment deters perpetrators, then we would

[4] While there are differences across states in their standards for making a report and privileged communication, these differences are mostly invariant over time from 2004 to 2017. Alternatively, training requirements have become more popular in recent years and considerable effort is needed to track down the changes over time for all 50 states and DC. For these reasons, I do not focus on these changes and leave them for future research.

[5] The bystander effect refers to the phenomenon where increasing the number of people that witness a crime reduces the probability of reporting. This phenomenon was first discovered by psychologists Darley & Latané after a woman was murdered in New York in front of dozens of neighbors (ESRC, nd). Factors contributing to the bystander effect have since been researched in detail (e.g. Fischer et al., 2011); however, no examples, to my knowledge, of the bystander effect have been reported in the child maltreatment literature.

expect to see a decline in reporting, holding the nature of reporting constant. Deterring perpetrators would be a beneficial impact of mandatory reporter legislation.

Using a two-way fixed effects model with a state-level linear time trend, I do not find any evidence that the referral rate responds to changes in mandatory reporters, and I can rule out responses larger than 10 percent. Alternatively, I find that increasing the number of jobs listed as mandatory reporters increases the report rate by 4 percent. While there are more reports, most of the increase is driven by unsubstantiated reports. Moreover, a legislation change that increases the number of mandatory reporters leads to a smaller increase in reports in states with more mandatory reporters, relative to states with fewer mandatory reporters.

I also investigate whether any changes in reporting can be detected in the short-run. If the legislation change was highly publicized, then we might expect a larger temporary shock in the short-run compared to the long-run. However, the direction of this shock is ambiguous. On the one hand, as mandatory reporters learn about their new responsibility, they may either report more or less, depending on the size of the bystander effect. Alternatively, perpetrators may change their behavior temporarily to prevent getting caught. Using an event-study design and difference-in-differences approach, I compare child maltreatment reporting five months before and after a legislation change within a state. I find suggestive evidence that reporting is overall unresponsive to mandatory reporter legislation changes in the short-run, and I can rule out increases and decreases greater than 8 percent. This is perhaps unsurprising, since in most states mandatory reporter legislation seems to be modified with little publicity.

In addition, I advance the existing research by quantifying the effect of specific professions added to the list of mandatory reporters. Coaches, college staff, and camp staff were the most commonly added professions between 2004 and 2017. Different professions are expected to have different impacts on reporting due to the degree of interaction with children and their reason for being included. Moreover, reports made by professionals have higher substantiation rates than reports made by nonprofessionals (Wolfe, 2012; King et al., 2013). I find that the majority of added professions do not significantly impact reporting. As states contemplate adjusting mandatory reporter laws, they may question whether more professions need to be listed, and if so, which ones.

Child maltreatment reports reflect the true nature of maltreatment and impact the demand for child protective services. In addition, mandatory reporter laws are policy levers that states can

enact and modify rather cheaply. For these reasons, it is important to understand how changing who is required to report may or may not effectively detect child maltreatment.

2. Background on Mandatory Reporter Laws and Their Impact on Reporting

Under the Child Abuse Prevention and Treatment Act, states are required to designate certain professionals as mandatory reporters. A mandatory reporter is someone required by law to report when they know or suspect child maltreatment. Some states require universal reporting, where all adults are considered mandatory reporters, whereas other states designate a list of professions as mandatory reporters. Typically, these mandatory reporters work in professions that interact with children regularly, such as teachers, pediatricians, and childcare providers. In some states with specific industries, professions such as film and photograph processors, computer technicians, and camp counselors are required to report.

Over the years, more professions have been added to the list of mandatory reporters. In 2004, the most common professions listed as mandatory reporters across all states included social workers, teachers, health care workers, mental health professionals, childcare providers, and law enforcement officers (CWIG, 2003). On average, eight broad job categories were indicated as mandatory reporters, with some states indicating zero professions[6] and others indicating as many as 15. By 2017, an average of nine job categories were indicated, with as few as zero and as many as 19. Common professions added during this time period include coaches, university staff, and camp staff.

Currently, some states are discussing scaling back their list of mandatory reporters because they fear the additional unsubstantiated referrals are overburdening CPS and support is not being allocated effectively (i.e. to those most in need).[7] While these concerns are frequently brought up (Melton, 2005; Mathews & Kenny, 2008; Cecka, 2015; Raz, 2017), the evidence is less clear. Cross country analyses find that mandatory reporting laws increase the number of reports (Mathews & Kenny, 2008; Donald, 2012). In Australia, mandatory reporting legislation increased the detection of sexual abuse (Mathews et al., 2016), and in Canada, mandatory reporting laws increased contact with CPS for severe and frequent maltreatment, but it is unclear whether this increased contact with CPS improved child wellbeing (Tonmyr et al., 2018).

[6] States that do not indicate certain professions have a universal reporting law, in which all people are required to report regardless of their profession.
[7] For example, Idaho passed a bill in February 2020 to eliminate some mandatory reporting requirements.

In the United States, the evidence is mixed. A presentation at the American Economic Association in 2020 found preliminary evidence that the first laws of mandatory reporters in the 1960s and 70s[8] led to increased awareness of child abuse and reductions in child mortality for children under one years old (Arteaga & Barone, 2020). These reductions become apparent 4 to 5 years after the policy was enacted. In a county-level study, universal reporting laws are associated with higher report rates for abuse, and clergy reporting requirements are associated with increased total reports, although not all are substantiated (Palusci et al., 2016). In a state-level study, universal reporting laws are associated with higher report rates for neglect (Krase & DeLong-Hamilton, 2015). Alternatively, Ho et al. (2017) conclude that child maltreatment reporting did not differ across states with and without universal mandatory reporting laws. Overall, the research around mandatory reporting laws and referrals is scant, and few explanations for differences are given.

In this paper, I discuss potential mechanisms and explain their implications. Behavioral responses from both reporters and perpetrators are possible explanations for long-run changes in reporting. Specifically, there is a knowledge effect, bystander effect, and deterrent effect. Child maltreatment is underreported for a variety of reasons including a lack of understanding what constitutes maltreatment and fear that steps following the report will make the situation worse (National Research Council, 2014). As more people understand the dangers of child maltreatment, recognize it as a public concern, and better understand their and CPS's role, then the quantity of reports may increase, holding the true amount of child maltreatment constant. This situation describes the knowledge effect and explains how reporting might increase in response to a mandatory reporter legislation change.

The bystander effect refers to the phenomenon where increasing the number of people that witness a crime reduces the probability of reporting. Two primary factors that contribute to this phenomenon are the diffusion of responsibility (Darley & Latané, 1968) and not understanding the environment (Darley & Latané, 1970). Moreover, the bystander effect is reduced in emergency situations, when perpetrators are present, and when the costs of intervening are physical, and not financial or opportunity costs (Fisher et al., 2011; Panchanathan et al., 2013). Child maltreatment reporting may be especially prone to the bystander effect for three reasons. First, in states with relatively more mandatory reporters, requiring more people to report allows mandatory reporters

[8] These first mandatory reporter laws listed doctors to prevent "Battered Child Syndrome."

to diffuse their responsibility of reporting more easily. In fact, some mandatory reporters already report passing on the responsibility to their supervisors (McTavish et al., 2017). Second, there is evidence that mandatory reporters do not understand the reporting environment. For example, seasoned mandatory reporters indicate needing better training to identify and report maltreatment (McTavish et al., 2017). Lastly, child maltreatment incidences have the opposite characteristics of the situations in which the bystander effect is mitigated. They are generally not emergencies and learned about after the fact when the child is away from the perpetrator, and mandatory reporters face non-physical costs to intervene. The bystander effect might be one explanation for a decline in reporting.

The deterrent effect might be another explanation for a decline in reporting. An increase in mandatory reporters and awareness of child maltreatment may deter perpetrators. In this situation, we would expect to see a decline in reporting, holding the nature of reporting constant. Disentangling the bystander effect from the deterrent effect is difficult; however, the decline in reports from these two mechanisms have very different policy implications. The bystander effect implies children may be going unreported and the deterrent effect implies fewer children are being maltreated. One way to disentangle the deterrent effect from the bystander effect is by examining how substantiated and unsubstantiated reports respond. For example, a decrease in substantiated reports, or victims, relative to unsubstantiated reports, holding constant the investigation process, would indicate less maltreatment. Deterring perpetrators would be a beneficial impact of the mandatory reporter legislation, whereas the bystander effect would be an unintended consequence.

In the short-run there might be a publicity effect, or a temporary shock in reporting, if the legislation change was highly publicized. However, the direction of this shock is ambiguous. On the one hand, as mandatory reporters learn about their new responsibility, there may either be more or less reports, depending on the size of the mechanical effect, the bystander effect, and the deterrent effect. The mechanical effect is the effect of increasing the number of people required to report maltreatment, holding constant reporter and perpetrator behavior. For example, as more people are required to report, the quantity of reports may simply increase as a result of children interacting with more mandatory reporters, assuming these people know of their new responsibilities. Alternatively, mandatory reporters may diffuse their responsibility to report. The bystander effect relies on the assumption that mandatory reporters know there are other people required to report maltreatment. Lastly, perpetrators may change their behavior temporarily to

prevent getting caught. If legislation changes are not well publicized, then we would not expect to see evidence of the publicity effect.

3. Data

I create a state panel of mandatory reporter legislation, child maltreatment referrals and reports,[9] case dispositions, demographic characteristics, and economic conditions from 2004 to 2017. I start in 2004 because this is the first year in which all but two states report child maltreatment data to the National Data Archive on Child Abuse and Neglect (NDACAN).[10] I end in 2017 because this is when the timing variation in the number of jobs listed as mandatory reporters starts to wind down.[11]

The mandatory reporter job classifications for each state come from changes in mandatory reporting laws.[12] I construct the first state-level panel of mandatory reporter legislation changes from 2004 to 2017.[13] Table 1 summarizes these changes over time.[14] From 2004 to 2017, 27 states have updated their list of mandatory reporters at least once. One-third, 9 states, have only made one change, whereas the remaining 18 states have made two to six changes. Each change adds anywhere from one to four more broad categories of professions to the list of mandatory reporters. During this time period, only one state, Virginia, removed a profession, clergy, from the list in 2007, so overall a change in legislation represents an increase in mandatory reporters.

The child maltreatment data come from National Child Abuse and Neglect Data System (NCANDS) housed by NDACAN. I use both the agency and child files. The NCANDS agency file contains the number of referrals screened-out in a state during a specific year. One drawback of using just the agency file is that it only includes referrals, which are volatile from year to year

[9] See Appendix Figure 1 and 2 for the average report rate by state and year, respectively.
[10] Maryland and Michigan are missing one year of child maltreatment data in 2006 and 2007, respectively. Oregon and North Dakota are missing multiple years of child maltreatment data. North Dakota started reporting child maltreatment data in 2010 and Oregon started reporting in 2011.
[11] Mandatory reporter legislation changes after 2017 deal more with training requirements.
[12] I am very grateful to the Child Welfare Information Gateway librarians, especially John Vogel and Sara-Jane Ziaya, for sending me the archived documents on mandatory reporting laws. Dates are verified with the NCSL and Westlaw databases.
[13] Mathematica publicly released the SCAN Policies Database April 2021, which provides differences in state policies for one year, 2019. Per email correspondence with the database team, state policies in 2021 will be released in 2022. While this will be a valuable resource in tracking policy changes over time, it is limited to changes starting after 2019, whereas the panel I constructed starts in 2004.
[14] A more detailed table with effective and enacted dates and added professions is available upon request.

as a result of changes that are not always related to mandatory reporting legislation.[15] Alternatively, the NCANDS child file contains detailed information about a report of maltreatment and the child characteristics. Reports are recorded during the fiscal year in which the disposition was decided, but I restructure this data to obtain the number of reports, unique children, and victims in a given month, year, and state. The average time to disposition is 56 days, so this structure does not drastically change the results.

Finally, state characteristics and economic conditions, which are included as controls in robustness specifications, come from the Annie E. Casey foundation KidsCount data book and the University of Kentucky Center for Poverty Research welfare dataset.

4. Empirical Strategy

I first analyze trends in reporting using annual state-level data. I use a linear time trends model with fixed effects to estimate how changes in child maltreatment referrals and reports correspond to changes in mandatory reporters from 2004 to 2017. The main regression equation is:

$$Y_{sy} = \beta_0 + \beta_1 MR_{sy} + \gamma_s + \gamma_y + \gamma_s * year + \varepsilon_{sy} \quad (1)$$

Where Y_{sy} is the child maltreatment rate, per 100,000 children for state s in year y. The four specific rates are total referrals, screened-out referrals, reports (i.e. screened-in referrals), and children investigated. Effects on screened-out referrals and reports are included to determine the composition of total referrals. MR_{sy} is a discrete variable that tracks changes in mandatory reporters. State fixed effects, γ_s, allow for within state comparisons to avoid confounding the impact of changes in mandatory reporters with state differences in culture and other policies. Year fixed effects, γ_y, are included to capture any time shocks that may impact the entire country similarly, such as the Great Recession. Lastly, state-specific linear time trends, $\gamma_s * year$, are imposed to capture the trends in child maltreatment over time within a state.[16] The error term is ε_{sy}.

[15] For example, in 2010 Alabama's screened-out referrals dropped by almost 100 percent as a result of correcting an error from previous years (ACF, 2011). In Arkansas, the number of referrals screened-out increased as a result of changing child maltreatment statutes and staff training (ACF, 2014).

[16] As a robustness check, I include state characteristics, such as racial, age, educational, and family structure composition, and economic conditions, such as the poverty and unemployment rate. Results are similar. Some of these variables will impact the underlying behavior of child maltreatment. Controlling for these variables assumes that the underlying nature of child maltreatment is constant, whereas excluding these allows the nature of child maltreatment to adjust to the mandatory reporter legislation.

$\beta_1$estimates the effect of changing mandatory reporter legislation on child maltreatment referrals and reports within a state. The validity of this approach relies on the assumption that states did not change their mandatory reporter legislation concurrently with other child welfare legislation that affects referrals and reports. For example, changing the intake process or what constitutes abuse and neglect might bias the results. Delaware and Pennsylvania are two examples of states that enacted a comprehensive package of child welfare reforms to better protect children in response to high profile incidences (ACF, 2012; ACF, 2018), but the majority of states did not change mandatory reporting and other child welfare legislation concurrently. Such incidences are uncommon but should be kept in mind when interpreting results.

In addition to an annual analysis, I construct a state-level panel of monthly observations. One advantage of this approach is that more observations can lead to more statistical power. In addition, the timing of treatment is more precise. On the other hand, monthly report rates are more volatile than annual report rates. Figure 1 shows how child maltreatment reports fluctuate across months.[17] For this reason, the monthly analysis includes a year-by-month fixed effect, in place of the year fixed effect, in equation (1).

There may also be differential effects by the number of changes and amount of mandatory reporters. Figure 2 plots the average number of reports by change in mandatory reporter legislation, and Figure 3 plots the average number of reports by number of jobs listed as mandatory reporters.[18] Both of these figures suggest there are non-linear effects, and the interaction between legislation changes and number of mandatory reports is complex. For example, a legislation change in states with a relatively low number of reporters may have a different impact than a legislation change in states with a relatively high number of reporters. I estimate the following equation to quantify this interaction effect:

$$Y_{smy} = \delta_0 + \delta_1 changeMR_{smy} + \delta_2 totalMR_{smy} + \delta_3(changeMR_{smy} X\ totalMR_{smy}) + \gamma_s + \gamma_{my} + \gamma_s * year + \varepsilon_{smy} \quad (2)$$

Where Y_{smy} is the report rate in state *s* for month *m* of year *y*, *changeMR* indicates the number of legislation changes, *totalMR* indicates the number of broad job categories classified as mandatory reporters, and γ_s, γ_{my}, $\gamma_s * year$ are the same as equation (1). δ_1 estimates the effect when a state changes it mandatory reporter legislation, δ_2 estimates the effect when a state adds another job to

[17] See Appendix Figure 2 for the average report rate by year.
[18] These averages are weighted by the population size.

the list of mandatory reporters, and δ_3 estimates the effect of a policy change for states with more mandatory reporters, relative to states with fewer mandatory reporters.

Next, to investigate the sensitivity of reporting in the short-run, I use an event study design, similar to Leslie & Wilson (2020). I investigate the effect five months before and after the legislation change, centering around the month in which the change occurred. Since monthly report rates are volatile, I include a control period to make comparisons in the same month before and after the legislation change. The regression equation is:

$$Y_{smy} = \sum_{\tau=-5}^{5} \beta_\tau [1(Month_{smy} = \tau) * Treat_{my}] + \sum_{\tau=-5}^{5} \theta_\tau 1(Month_{smy} = \tau) + \alpha Treat_{my} + \gamma_s + \gamma_m + \gamma_y + \varepsilon_{smy} \quad (3)$$

The outcomes are the report rate, substantiation rate, and victim rate (per 100,000 children) in state *s* for month *m* in year *y*. The indicator function $1(Month = \tau)$, for all $\tau \in \{-5,5\}$, takes a value of one if the month is within the five months before or after the effective date of the mandatory reporter change. $Treat$ is a binary variable that equals one in the five-month period both before and after the legislation change in the period the change occurred, and zero in the five-month period both before and after the legislation change one year earlier, i.e. the control period. I also control for state differences, monthly trends, and annual changes. As a result, β_τ estimates the effect for each of the five months before and after the legislation change within a state relative to the previous period. A valid design will not have pre-trends, that is β_{-5} through β_{-1} will not differ from zero.

To quantify the five-month average effects, I also estimate a difference-in-differences model. This approach estimates the short-run impact of changing mandatory reporters, relative to the same time period in the earlier year. This approach is validated by the lack of pre-trends from the event study.

5. Results

5.1. Annual Results

Table 2 reports the results from equation (1) for the annual analysis. There are 589 state-by-year observations.[19] The first panel reports the effect of an additional change in mandatory reporter legislation and the second panel reports the effect of an additional job category added to the list of

[19] New York, North Carolina, and Pennsylvania are excluded because they do not have data for these outcomes.

mandatory reporters. Column 1 reports the results for the total referral rate, per 100,000 children. The coefficient on the change in mandatory reporter legislation is -102.2 and the coefficient on the change in mandatory reporters is -107.1. Neither effect size is statistically different from zero; however, I can rule out large effects and conclude that total referrals do not increase or decrease by more than 10 percent in response to changes in mandatory reporter legislation. One potential explanation of this result is that changes in referrals screened-out and referrals screened-in (reports) might move in opposite directions. In other words, mandatory reporter laws might change the composition of total referrals.

Column 2 reports the results for screened-out referrals and column 3 reports the results for screened-in referrals (reports). Regardless of the independent variable, change in legislation or number of jobs, the coefficients on referrals screened-out are negative, and the coefficients on reports are positive. However, none of these coefficients are statistically significant. Lastly, I investigate whether the number of children investigated for maltreatment responds to mandatory reporter legislation (column 4). While the coefficients are positive, they are indistinguishable from zero.

The imprecise estimates from the annual analysis persist even when I separately add a quadratic time trend and state controls for demographic composition and economic conditions. They also persist when I construct a balanced sample of the 26 states that appear in the data all years from 2004 to 2017.[20] Two reasons for these imprecise estimates may be lack of power or vague timing of legislation changes. For example, a legislation change in January probably has a different impact on reporting the following year compared to a legislation change in November. To address these concerns, I use the NCANDS child file and estimate the monthly effect.

5.2. Monthly Results

Table 3 reports the results of the monthly analysis. Although there are more than 12 times the number of observations and more precisely defined treatment periods compared to the annual analysis, the effect of changes in mandatory reporter legislation on reporting is similar. The effect size on the number of reports (column 1) is marginally significant. All else equal, a change in mandatory reporting legislation is associated with an increase of 12.9 reports per 100,000 children (or 5.5 percent). Similarly, adding a job to the list of mandatory reporters increases the report rate

[20] These results are available upon request.

by 9.34 reports per 100,000 children (or 4 percent). This impact is driven by an increase in unsubstantiated reports, and not substantiated reports. All else equal, a change in mandatory reporting legislation leads to a 6 percent increase in the unsubstantiated report rate (column 3). The remaining results, substantiated rate (column 2), children investigated (column 4), and victim rate (column 5) are statistically insignificant. However, I can rule out effects sizes larger than 16 percent. In other words, changes in mandatory reporting legislation will not impact substantiation rates by more than 16 percent. These results persist when state-level controls are added and different combinations of fixed effects are included.[21]

Following the recent difference-in-differences discussion, treatment effects likely differ over time and by intensity (Callaway & Sant'Anna, 2020; de Chaisemartin & D'Haultfoeuille, 2020; Goodman-Bacon, 2021). One way to understand how the impact may differ by time and intensity is to estimate the group-time average treatment effect (Callaway & Sant'Anna, 2020). Similar to Callaway and Sant'Anna (2020), I estimate differential effects by treatment group. In this setup, states are grouped by the number of mandatory legislation changes they experienced from 2004 to 2017. There are nine states with one legislation change, eight with two changes, five with three changes, four with four changes, and one with six changes.[22] The overall average effect should be the weighted average of the group effects. The results from this exercise are provided in Appendix Table 1. The key takeaway from this exercise is that the states with one, four, and six changes are driving the results, depending on the outcome. In other words, there are differential, non-linear effects by the number of policy changes. This result, in combination with Figures 2 & 3, motivates estimating the interaction effect between the policy change and number of mandatory reporters. The differences in the number of mandatory reporters across states may be able to explain some of the differential effects by policy change.

5.2.1. Interaction Effects

Table 4 reports the results from estimating equation (2). All else equal, both a change in mandatory reporter legislation and an increase in the number of jobs classified as mandatory reporters leads to an increase in the report rate. States with relatively more mandatory reporters

[21] Alternatively, the results are statistically significant with at least 95 percent confidence when standard errors are clustered at the month-by-state level and year-by-state level. It is good practice to be conservative and avoid bias by using bigger, more aggregated clusters (Cameron & Miller, 2015), so I only report results clustered at the state-level. Results are available upon request.

[22] Refer to Table 1 for the list of states in each group.

experienced a smaller increase in reporting. This effect comes from fewer unsubstantiated reports and not substantiated ones. To further investigate differential effects, I estimate the short-run effects up to five months after the first policy change.

5.2.2. Short-run effects

The results for the short-run analysis, estimating equation (3), are given by the event study figures and the difference-in-differences table. The event study analysis, Figure 4, confirms that there were no pre-trends five months prior to the change in legislation. After the change, there does not seem to be a statistically significant impact either.

The difference-in-differences results are reported in Table 5. On average, over the five months after the mandatory reporter legislation change, there are no statistically significant differences in the report rates relative to the same time period in the prior year.[23] Although the effect sizes are imprecisely estimated, I can rule out effect sizes greater than 8 percent. In other words, changes in mandatory reporter legislation will not increase or decrease child maltreatment reporting by more than 8 percent in the short-run.

To check the sensitivity of this short-run analysis, I estimate this effect for policy changes that occurred between 2012 and 2014. Most of these policy changes added university staff, camp staff, and athletic coaches. The results of this sensitivity analysis are provided in Appendix Table 2, and are similar to the results from Table 5.

6. Who's Reporting?

In addition, I investigate who is making these reports because they potentially impact the probability of substantiation (Wolfe, 2012; King et al., 2013). Education personnel and law enforcement report the most cases of maltreatment across all states. Table 6 reports the changes in report source as additional jobs are added to the list of mandatory reporters for all states (panel 1), the 9 states with only one change in mandatory reporter legislation (panel 2), and for the years post 2012 (panel 3). Across all analysis samples, as more professions are added to the list of mandatory reporters, the report rates by education personnel, law enforcement, medical personnel, and social services personnel are unchanged. However, the report rate from other sources increases by 3 to 10 percent, depending on the sample. The increase in reports from other sources and the increase

[23] See Appendix Figure 3 to see these trends graphically in a handful of states after the first legislation change. Appendix Figure 3 further supports this finding.

in unsubstantiated reports from earlier supports the idea that reports by nonprofessionals have lower rates of substantiation and complements related work (e.g. Wolfe, 2012; King et al., 2013).

A supplemental analysis (Table 7) investigates differences in report rates by profession, recognizing that some people have more interaction with children than others. These differences might contribute to differences in reporting. For example, adding camp staff would likely have a smaller impact than adding coaches because camp staff interact with children less often than coaches. Including the job classifications that had at least two changes over 2004 to 2017 demonstrates that camp staff do have a smaller impact than coaches, although neither impact is statistically significant. In fact, only computer technicians contribute to a statistically significant change in reports. Interestingly, computer technicians contribute to a decline in reports. Although not statistically significant, adding clergy and coaches has the biggest impact of a 10 to 16 percent increase in reports, whereas adding camp staff and university staff has the smallest impact of a 2.5 to 3.2 percent decline. For comparison, Baron et al. (2020) and Cabrera-Hernandez and Padilla-Romo (2020) find that reporting declined by 21 to 30 percent during the COVID-19 pandemic as a result of reduced interactions between children and teachers. In addition, Palusci et al. (2016) find that counties in states that added clergy to the list of mandatory reporters between 2000 and 2010 had significantly more reports, but fewer substantiated reports; however, they do not provide percent changes.

This analysis provides a starting point in understanding how reports vary by profession. Future research should continue to investigate why certain professions were added to the list to mandatory reporters and try to better understand the role these professions play in detecting child maltreatment.[24] Understanding which professions can correctly detect maltreatment could inform states' decisions on who to add to the list of mandatory reporters and how to prepare and regulate training.

7. Conclusion

The Penn State scandal, USA Gymnastics scandal, and COVID-19 pandemic have highlighted the vital role mandatory reporters play in protecting the wellbeing of children. Mandatory reporters

[24] There has been considerable effort in understanding the role teachers, pediatricians, clergy, and police play in detecting child maltreatment (e.g. Baron et al., 2020; Cabrera-Hernandez & Padilla-Romo, 2020; Fitzpatrick et al., 2020; Warner & Hansen, 1994; Palusci et al., 2016; Edwards, 2019), but there is no information, to my knowledge, about the role of coaches, camp staff, computer technicians, university staff, humane officers, and other professions that have recently been added to the list of mandatory reporters.

have a legal obligation to report suspected maltreatment. Since child maltreatment is believed to be underreported, laws that increase the number of professions required to report may be an easy way for policymakers to approach the true amount of child maltreatment. Alternatively, competing forces, unintended consequences, or weak salience could render these policies ineffective.

In response to a mandatory reporter legislation change, there are numerous ways reporters, perpetrators, and CPS may respond. Reporters may be more or less likely to report child maltreatment depending on the strength of the knowledge and bystander effect, perpetrators may be deterred, and CPS may modify their intake and investigation processes and allocate resources differently. An important limitation of this work is the inability to disentangle each of these mechanisms. Future research may want to concentrate on disentangling these mechanisms as their competing forces might attenuate the overall effectiveness of mandatory reporter laws, and each mechanism implies different policy responses.

One potential unintended consequence of a mandatory reporter legislation change might be failing to identify and support the children with the greatest need for intervention (Raz, 2017). Determining whether changes in referrals are beneficial or burdensome depends on the composition of referrals screened-out and in. In the annual-level analysis, I find that after a mandatory reporter legislation change fewer referrals are screened-out and more referrals are screened-in and investigated. This finding suggests that mandatory reporter legislation may help identify cases of maltreatment. However, none of these coefficients are statistically significant, so this is speculative at best.

In the monthly-level analysis, I find that increasing the number of mandatory reporters leads to a 4 percent increase in reporting. This effect size is smaller than the impact of a one-dollar increase in the minimum wage and a one percentage point change in the unemployment rate.[25] When there is an increase in reporting, CPS may have to modify their operations (e.g., changing the amount of time spent on each investigation, employing additional staff, differential investigation process, etc.). It is unclear which modification dominates or is more beneficial, but a 4 percent increase in reports is modest and may not require substantial changes to CPS' operations.

[25] A one-dollar increase in the minimum wage reduced neglect by 9.6 percent (Raissian & Bullinger, 2017), and a one percentage point increase in the unemployment rate increased overall abuse by 10 percent (Brown & De Cao, 2020).

Of the reports screened-in for investigation, it is important to understand how the composition of substantiated and unsubstantiated reports changes. Increased reporting may result in better identification of child maltreatment or increased workload for the investigation staff. Better identification would be indicated by relatively more substantiated reports, whereas increased workload would be indicated by relatively more unsubstantiated reports. Regardless of whether the investigation process remains the same or responds to an increase in reports, more unsubstantiated reports are indicative of an increased workload for the child welfare staff. For example, if staff have more reports to investigate and remain as diligent as before, then the increase in unsubstantiated reports is an indicator of increased workload. The staff have to investigate more, potentially less-severe, reports of maltreatment. Alternatively, if the investigation process remains the same and staff have more reports to investigate, then they may do so less diligently, thus not substantiating as many reports. In this scenario, the increase in unsubstantiated reports is a consequence of the increased workload. I find that the increase in reporting is entirely driven by unsubstantiated reports. While an increase in unsubstantiated reports is indicative of increased workload for the child welfare staff, it is unclear whether an unsubstantiated finding is helpful or harmful to the children and families involved. In 2019, almost one-third of the children involved in unsubstantiated reports received follow-up support and services (ACF, 2021). Alternatively, involvement in the child welfare system can have detrimental effects, especially for low-income and minority families (Fong, 2020; Merritt, 2020).[26]

Recognizing the potential for differential effects by treatment and timing, I interact legislation changes with the number of mandatory reporters already listed and conduct a short-run analysis. I find that an additional legislation change leads to a smaller increase in reporting for states with more mandatory reporters, relative to states with fewer mandatory reporters. This finding suggests that mandatory reporter legislation may have diminishing marginal returns. In the short-run, I find that child maltreatment reporting is somewhat unresponsive to mandatory reporter legislation changes. I can rule out increases and decreases greater than 8 percent. In other words, I do not find any evidence of a publicity effect. This finding is unsurprising as it appears that many of the mandatory reporter legislation changes occurred with little publicity. A policy implication of these

[26] See Wilson et al. (2020) for a detailed qualitative review of children's experiences with CPS. Experiences vary from being traumatized by the investigation process to appreciating the material support provided by CPS.

results relating to saliency could be to add more training that addresses both how to detect maltreatment and explains the responsibilities of reporters. Future work should investigate how changes in mandatory reporters interacts with training requirements and punishments for failing to report.

In summary, I use the NCANDS agency and child files to examine how screened-out referrals, reports, and substantiated and unsubstantiated reports responded to changes in mandatory reporter laws from 2004 to 2017. It is important to note that the results of this paper are strictly related to reporting and cannot make inferences about welfare effects. Without tracking children's long-run outcomes, it is unclear whether more reporting guarantees improved child wellbeing. Another limitation of this study is that the NCANDS data only reports intrafamily child maltreatment. However, mandatory reporters are responsible for reporting child maltreatment that occurs both within and outside the family. While I do not find evidence of coaches, college staff, and other recently added professions impacting child maltreatment within a family, that does not mean that these professions have not impacted child maltreatment from perpetrators outside the family. Nonetheless, understanding the nature and reporting of intrafamily child maltreatment is important for the entire child welfare system because maltreatment makes up the demand for child protective services, including family preservation and foster care placements. Presently, simply adding professions to an arbitrary list of mandatory reporters has done little to improve child maltreatment detection and reporting. In a system already overburdened with cases and limited funds (Giammarise, 2017; Raz, 2017; CBS News, 2019), thoughtful consideration pertaining to who should be included on the mandatory reporter list coupled with training and incentives may be a relatively cheap way to more effectively detect child maltreatment and manage child welfare.

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Tables and Figures

Table 1: Mandatory reporter law changes in each state from 2004 to 2017

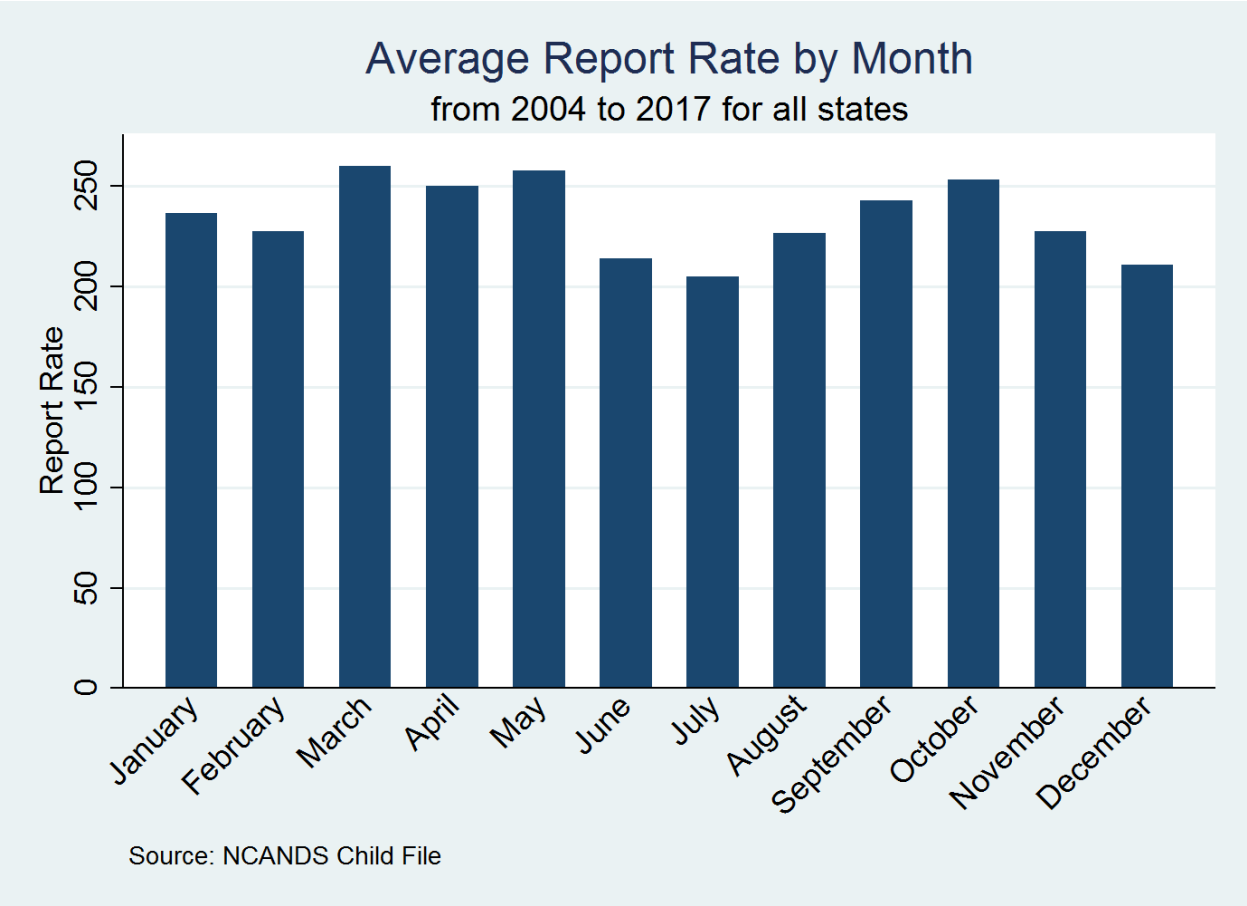
State	Number of Mandatory Reporter Changes	Number of Professions in 2004	Number of Professions in 2017	Number of Added Professions
Arizona	0	6	6	0
Connecticut	0	14	14	0
Florida	0	8	8	0
Hawaii	0	9	9	0
Idaho	0	7	7	0
Indiana	0	2	2	0
Iowa	0	10	10	0
Kentucky	0	8	8	0
Maryland	0	4	4	0
Massachusetts	0	13	13	0
Michigan	0	9	9	0
Minnesota	0	8	8	0
Mississippi	0	8	8	0
Missouri	0	11	11	0
Montana	0	9	9	0
Nebraska	0	3	3	0
New Hampshire	0	8	8	0
New Jersey	0	0	0	0
New Mexico	0	6	6	0
North Carolina	0	0	0	0
Rhode Island	0	1	1	0
Texas	0	5	5	0
Utah	0	1	1	0
Wyoming	0	0	0	0
Alabama	1	8	9	1
Alaska	1	10	11	1
Delaware	1	4	5	1
North Dakota	1	9	10	1
Oklahoma	1	3	4	1
Oregon	1	12	15	3
Pennsylvania	1	9	13	4
South Carolina	1	14	15	1
Tennessee	1	8	9	1
Arkansas	2	12	14	2
California	2	15	19	4
Georgia	2	7	11	4
Kansas*	2	9	9	0
Maine	2	14	15	1

State	Number of Mandatory Reporter Changes	Number of Professions in 2004	Number of Professions in 2017	Number of Added Professions
South Dakota	2	10	12	2
Washington	2	8	10	2
West Virginia	2	9	12	3
Illinois	3	14	19	5
Louisiana	3	11	15	4
New York	3	11	13	2
Ohio	3	10	11	1
Wisconsin	3	10	12	2
Colorado	4	9	14	5
DC	4	7	10	3
Nevada*	4	13	13	0
Vermont	4	10	11	1
Virginia	6	9	13	4

Notes: The table is organized by number of changes and then alphabetical order. The number of changes is determined from reading state legislation provided by the Westlaw database. Number of professions in 2004 and 2017 is the number of broad job categories listed in the state statutes provided by the CWIG in 2003 and 2015, respectively. A value of zero indicates that no broad job categories are listed, i.e. the state has a universal mandatory reporting law and requires all persons to report. The number of added professions is the difference between the 2017 and 2004. In states with no change, there are no added professions. A detailed state panel of effective and enacted dates and professions added from 2002 to 2020 is available upon request.

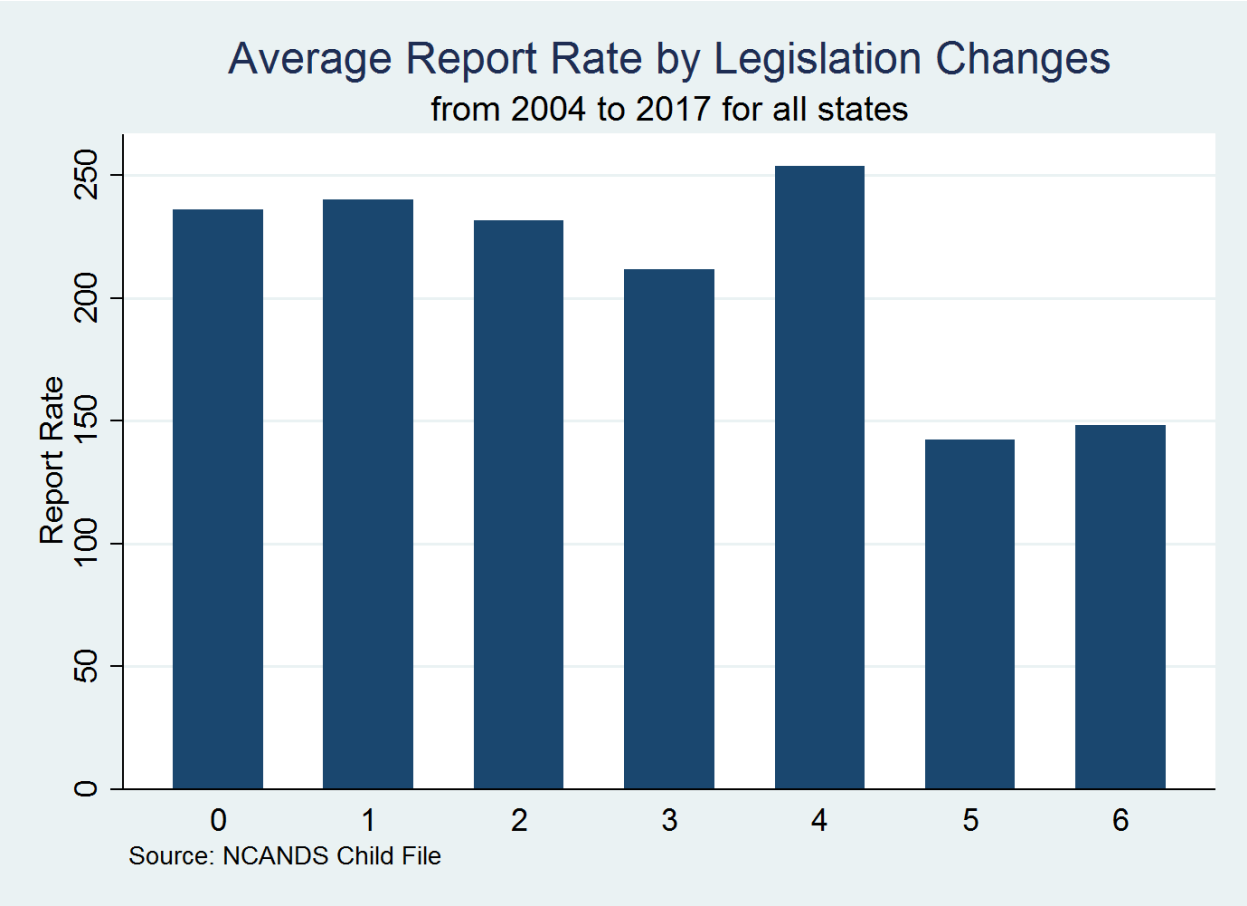
* Both Kansas and Nevada have multiple mandatory reporter legislation changes, but report an unchanged number of professions because the professions added to the list do not fall under the general categories considered in this analysis.

Figure 1: Average report rate by month



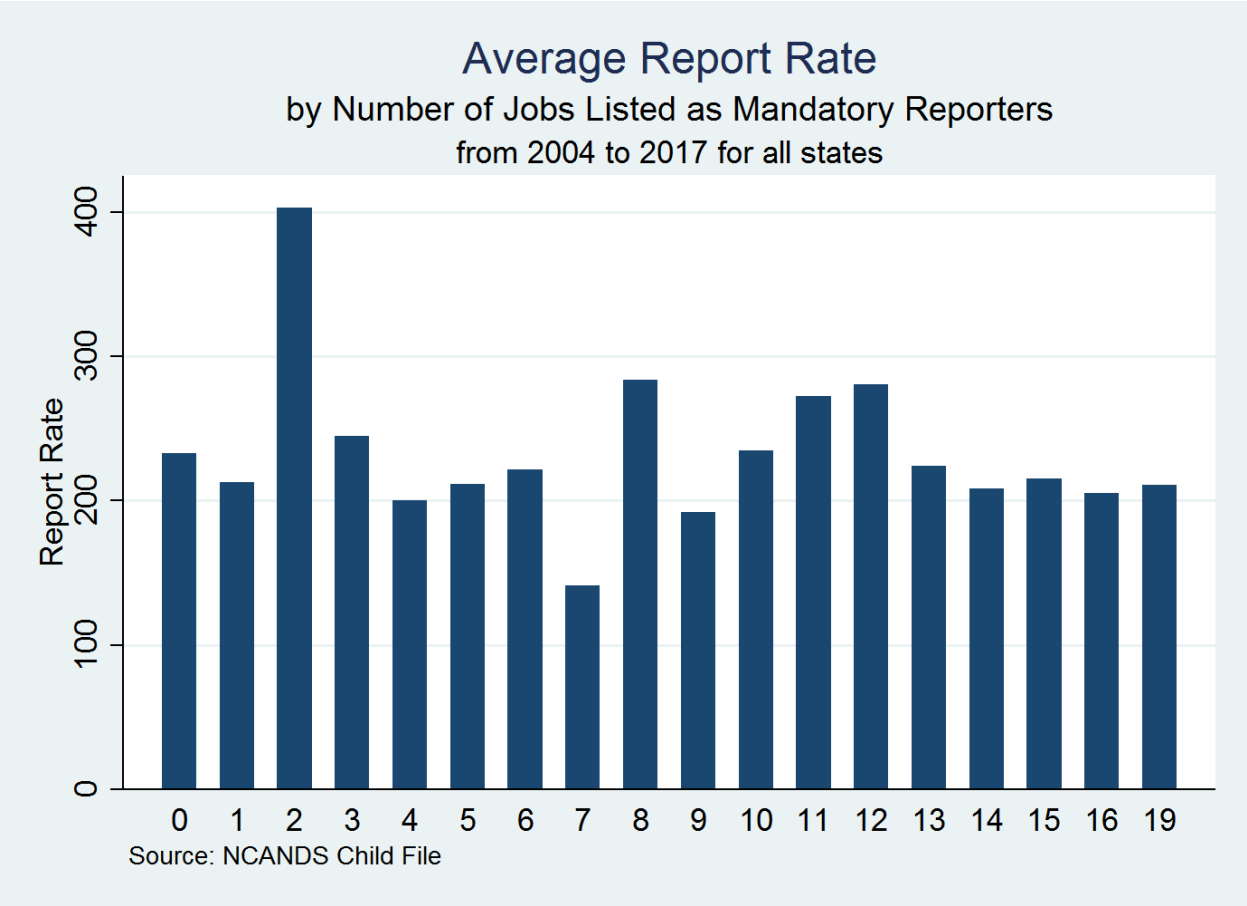
Notes: The report rate is calculated per 100,000 children. In practice, the number of reports screened-in is divided by the number of children and then multiplied by 100,000. The average is taken across all states and years and is weighted by the state population. This figure masks within state and across year variation.

Figure 2: Average report rate by number of changes in mandatory reporter legislation



Notes: The report rate is calculated per 100,000 children. In practice, the number of reports screened-in is divided by the number of children and then multiplied by 100,000. The average is taken across all states and years and is weighted by the state population. This figure masks within state and across year variation.

Figure 3: Average report rate by number of jobs listed as mandatory reporters



Notes: The report rate is calculated per 100,000 children. In practice, the number of reports screened-in is divided by the number of children and then multiplied by 100,000. The average is taken across all states and years and is weighted by the state population. This figure masks within state and across year variation.

Table 2: Regression results from annual analysis

	(1) Total referrals (per 100,000 children)	(2) Referrals screened out (per 100,000 children)	(3) Reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)
Panel 1				
Number of mandatory reporter policy changes	-102.2	-238.3	136.1	228.7
	(147.6)	(244.6)	(178.9)	(300.5)
Mean of dep. var.	4,419	1,821	2,598	4,364
Observations	589	589	589	589
Adjusted R^2	0.769	0.684	0.547	0.547
Panel 2				
Number of jobs listed as mandatory reporters	-107.1	-236.4	129.3	217.3
	(182.4)	(173.4)	(116.3)	(195.3)
Mean of dep. var.	4,419	1,821	2,598	4,364
Observations	589	589	589	589
Adjusted R^2	0.770	0.689	0.553	0.553

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. In the first panel, the independent variable is the number of changes in the mandatory reporter legislation, and in the second panel, the independent variable is the number of jobs listed as mandatory reporters. The mean of the dependent variable is weighted by the population average. Regressions include state fixed effects, year fixed effects, and a state-specific linear time trend. New York, North Carolina, and Pennsylvania are excluded because they do not have data for these outcomes.

Table 3: Regression results from monthly analysis

	(1) Reports (per 100,000 children)	(2) Substantiated reports (per 100,000 children)	(3) Unsubstantiated reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)	(5) Number of victims (per 100,000 children)
Panel 1					
Number of mandatory reporter policy changes	12.90* (7.41)	1.86 (2.36)	11.03* (5.71)	28.76 (19.06)	2.76 (4.26)
Mean of dep. var.	234.3	55.0	179.3	427.7	86.1
Observations	8,422	8,422	8,422	8,422	8,422
Adjusted R^2	0.598	0.431	0.660	0.529	0.416
Panel 2					
Number of jobs listed as mandatory reporters	9.34* (5.23)	1.35 (1.88)	7.99* (4.27)	18.67 (13.72)	1.13 (3.13)
Mean of dep. var.	234.3	55.0	179.3	427.7	86.1
Observations	8,422	8,422	8,422	8,422	8,422
Adjusted R^2	0.598	0.431	0.660	0.528	0.415

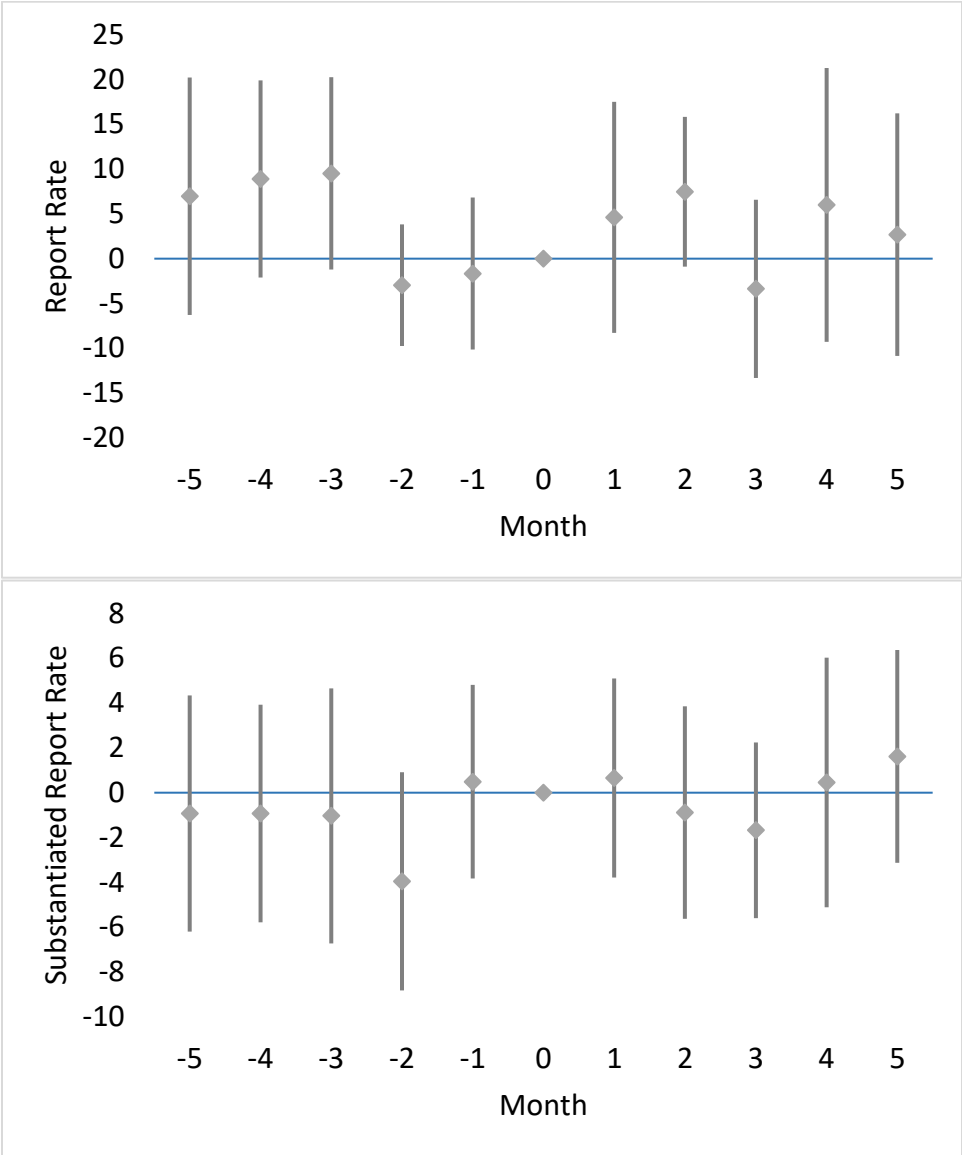
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. In the first panel, the independent variable is the number of changes in the mandatory reporter legislation, and in the second panel, the independent variable is the number of jobs listed as mandatory reporters. The mean of the dependent variable is weighted by the population average. Regressions include state fixed effects, year-by-month fixed effects, and a state-specific linear time trend. Results are similar to including year and month fixed effects, instead of the year-by-month fixed effect. They are also invariant to including the following set of state-level time-varying controls: racial composition, age composition, education composition, living situation, poverty variables (teen birth rate, households without a vehicle, poverty rate, food insecurity), unemployment rate, safety net generosity (TANF & SNAP), governor political affiliation, income per capita, and minimum wage. These two sets of results are available upon request.

Table 4: Interaction effect between policy changes and number of jobs listed as mandatory reporters

	(1) Reports (per 100,000 children)	(2) Substantiated reports (per 100,000 children)	(3) Unsubstantiated reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)	(5) Number of victims (per 100,000 children)
Policy changes	46.47***	2.182	44.29***	109.7**	13.89
	(16.44)	(8.598)	(13.21)	(42.59)	(11.78)
Jobs	10.99**	0.946	10.04***	21.30*	1.276
	(4.333)	(2.022)	(3.509)	(11.86)	(3.281)
Policy changes x Jobs	-3.534***	-0.0933	-3.441***	-8.147***	-1.004
	(1.175)	(0.630)	(0.982)	(3.033)	(0.853)
Mean of dep. var.	234.3	55.0	179.3	427.7	86.1
Observations	8,422	8,422	8,422	8,422	8,422
Adjusted R^2	0.604	0.431	0.667	0.536	0.417

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. The coefficient for "Policy changes" is the effect when a state changes it mandatory reporter legislation, the coefficient for "Jobs" is the effect when a state adds another job to the list of mandatory reporters, and the coefficient on the interaction term, "Policy changes x Jobs," is the effect of a policy change for states with more mandatory reporters, relative to states with fewer mandatory reporters. The mean of the dependent variable is weighted by the population average. Regressions include state fixed effects, year-by-month fixed effects, and a state-specific linear time trend.

Figure 4: Event-study analysis for first legislation change



Notes: This figure plots the coefficients from equation (2) where the outcome is the report rate per 100,000 children (top) or substantiated report rate per 100,000 children (bottom) at the state-by-month level. Each coefficient five months before and after the mandatory reporter legislation change is plotted with its 95 percent confidence interval. These values represent the change in the treatment year, relative to the prior year (control year). State, year, and month fixed effects are included.

Table 5: Short-run effects of the first legislation change

	(1) Reports (per 100,000 children)	(2) Substantiated reports (per 100,000 children)	(3) Unsubstantiated reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)	(5) Number of victims (per 100,000 children)
Post	-14.73*	-4.185**	-10.55*	-31.29	-6.619**
	(7.490)	(1.810)	(6.162)	(18.61)	(3.135)
Treatment	-22.18**	-3.662	-18.52**	-45.50**	-4.994
	(8.786)	(2.364)	(8.008)	(19.20)	(3.882)
Post x Treatment	0.184	1.101	-0.916	3.689	2.644
	(5.065)	(0.982)	(5.111)	(12.53)	(1.983)
Mean of dep. var.	230.0	54.0	176.0	425.6	84.1
Observations	568	568	568	568	568
Adjusted R^2	0.378	0.224	0.384	0.286	0.202

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. "Post" is equal to one five months after the legislation change and zero for the five months prior. "Treatment" is equal to one in the year of the legislation change and zero in year prior. Post x Treatment estimates the effect in the months after the legislation change, relative to the same time period the year before. The mean of the dependent variable in the post months of the control year is provided. Regressions include state, year, and month fixed effects. The 26 states included in this analysis are Alabama, Alaska, Arkansas, California, Colorado, DC, Delaware, Georgia, Illinois, Kansas, Louisiana, Maine, Nevada, New York, North Dakota, Ohio, Oklahoma, Pennsylvania, South Carolina, South Dakota, Tennessee, Vermont, Virginia, Washington, West Virginia, and Wisconsin.

Table 6: Source of reports

	(1) Education personnel and day care providers	(2) Legal, law enforcement, or criminal justice	(3) Medical personnel	(4) Social services personnel	(5) Other source
Panel 1: All States					
Number of jobs listed as mandatory reporters	0.215 (1.524)	1.519 (1.141)	-0.0295 (0.512)	0.956 (1.278)	6.674** (2.927)
Mean of dep. var.	44.3	42.5	21.7	27.7	98.4
Observations	8,377	8,377	8,377	8,377	8,377
Adjusted R^2	0.715	0.630	0.561	0.431	0.536
Panel 2: States with no or one change					
Number of jobs listed as mandatory reporters	-1.040 (1.573)	0.282 (1.702)	-0.605 (1.642)	0.286 (0.639)	10.97*** (3.571)
Mean of dep. var. before change in control states	42.6	48.5	23.7	27.3	108.6
Observations	5,353	5,353	5,353	5,353	5,353
Adjusted R^2	0.725	0.631	0.564	0.494	0.581
Panel 3: Post 2012					
Number of jobs listed as mandatory reporters	-0.603 (0.681)	-0.125 (0.354)	-0.430 (0.360)	-0.981 (1.017)	3.720* (2.099)
Mean of dep. var. before change in control states	42.7	47.4	23.6	25.6	111.2
Observations	3,672	3,672	3,672	3,672	3,672
Adjusted R^2	0.701	0.523	0.499	0.355	0.584

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a different report source rate (per 100,000 children). The first panel includes all states. In the second panel, the analysis sample is limited to the 9 states with just one mandatory reporter change between 2004 to 2017 and the 27 states with no changes. The third panel is for the years between 2012 and 2017. The mean of the dependent variable is weighted by the population average. Regressions include state fixed effects, year-by-month fixed effects, and a state-specific linear time trend.

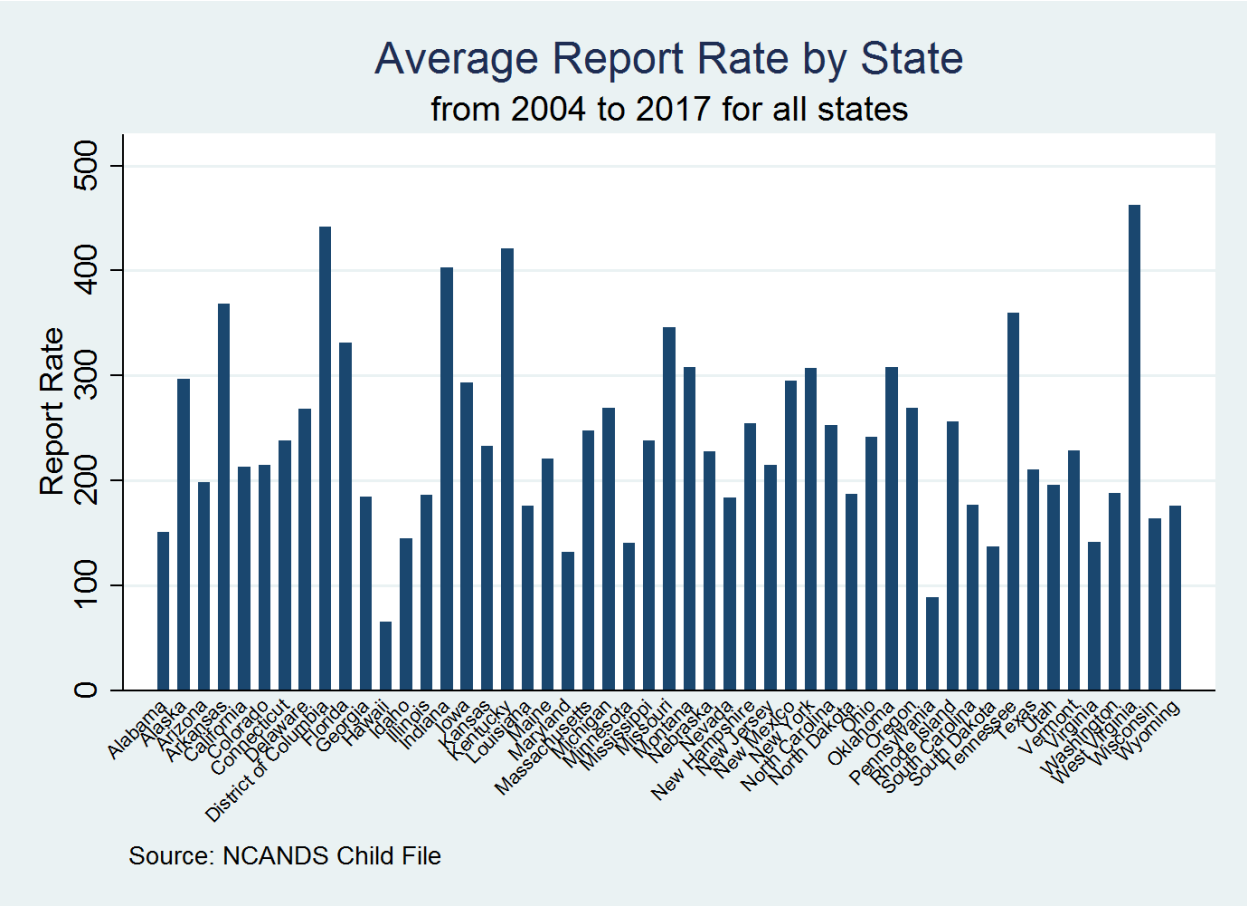
Table 7: Effect of specific jobs

	(1) Reports (per 100,000 children)	(2) Substantiated reports (per 100,000 children)	(3) Unsubstantiated reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)	(5) Number of victims (per 100,000 children)
computer technicians	-22.81*	-6.927	-15.88*	-65.67**	-16.31
	(12.55)	(5.669)	(8.172)	(31.80)	(9.758)
probation and parole officers	19.12	15.41	3.708	39.70	19.07
	(23.48)	(10.38)	(14.13)	(51.25)	(17.36)
camp staff	7.578	8.633	-1.055	-1.534	8.215
	(15.28)	(5.842)	(12.73)	(30.34)	(9.161)
animal control and humane officers	15.84	-6.629	22.47	47.33	-4.505
	(28.17)	(11.77)	(17.21)	(68.40)	(20.58)
CASAs and child advocates	-10.21	-4.437	-5.777	-23.29	-6.594
	(10.21)	(2.778)	(7.947)	(24.24)	(5.415)
clergy members	37.47	11.70	25.77	82.08	0.522
	(27.91)	(8.783)	(30.75)	(65.31)	(6.701)
college staff	-6.040	-8.708	2.668	-23.00	-12.81
	(18.79)	(7.370)	(13.47)	(41.60)	(11.88)
emergency medical	18.28	-0.289	18.57	22.02	-0.672
	(11.43)	(2.668)	(11.68)	(21.53)	(4.643)
coaches or employees of rec sports	23.83	8.522	15.31	73.87	15.70
	(17.65)	(8.433)	(11.75)	(44.60)	(14.83)
Mean of dep. var.	234.3	55.0	179.3	427.7	86.1
Observations	8,422	8,422	8,422	8,422	8,422
Adjusted R^2	0.601	0.443	0.662	0.535	0.601

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. The mean of the dependent variable is weighted by the population average. Regressions include state fixed effects, year-by-month fixed effects, and a state-specific linear time trend.

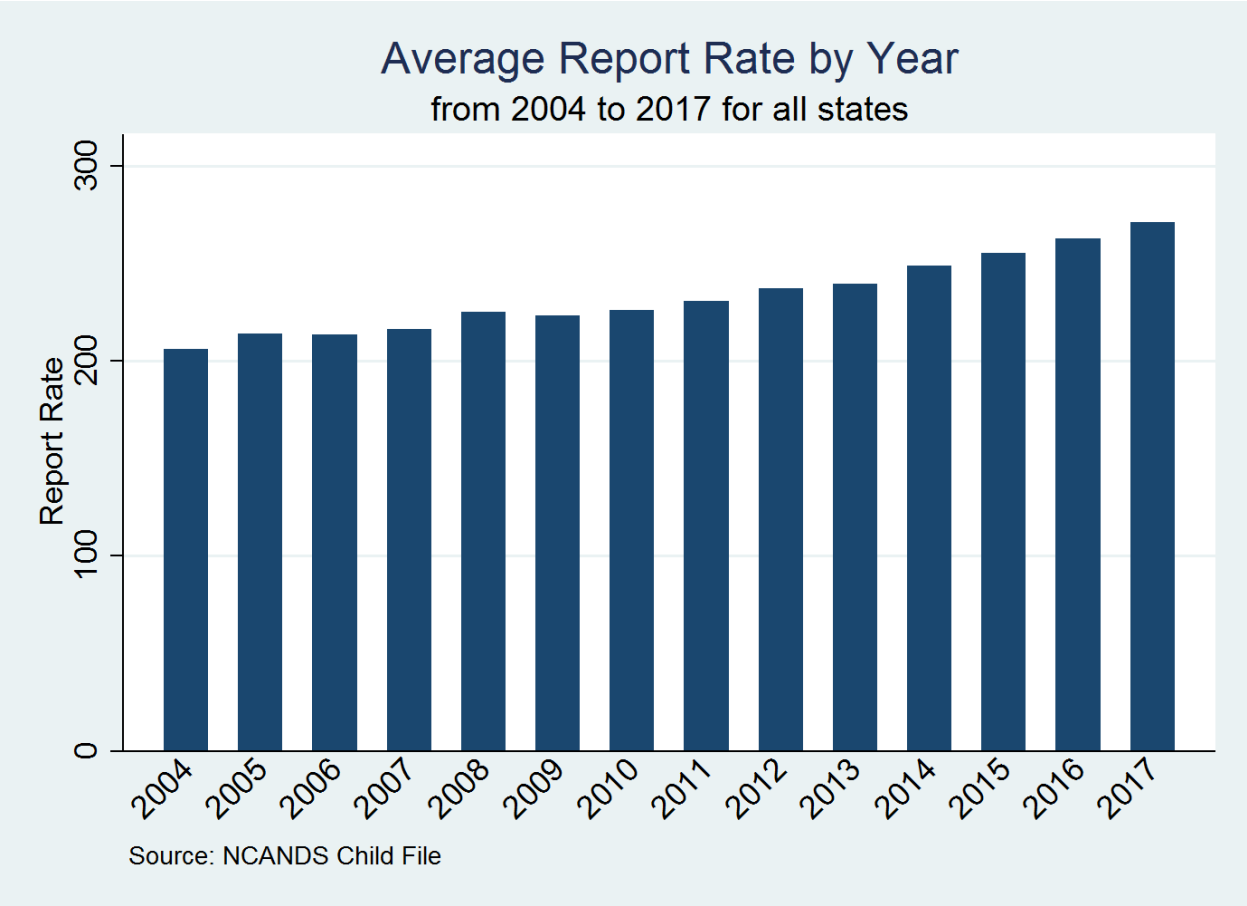
Appendix Tables & Figures

Appendix Figure 1: Average report rate by state



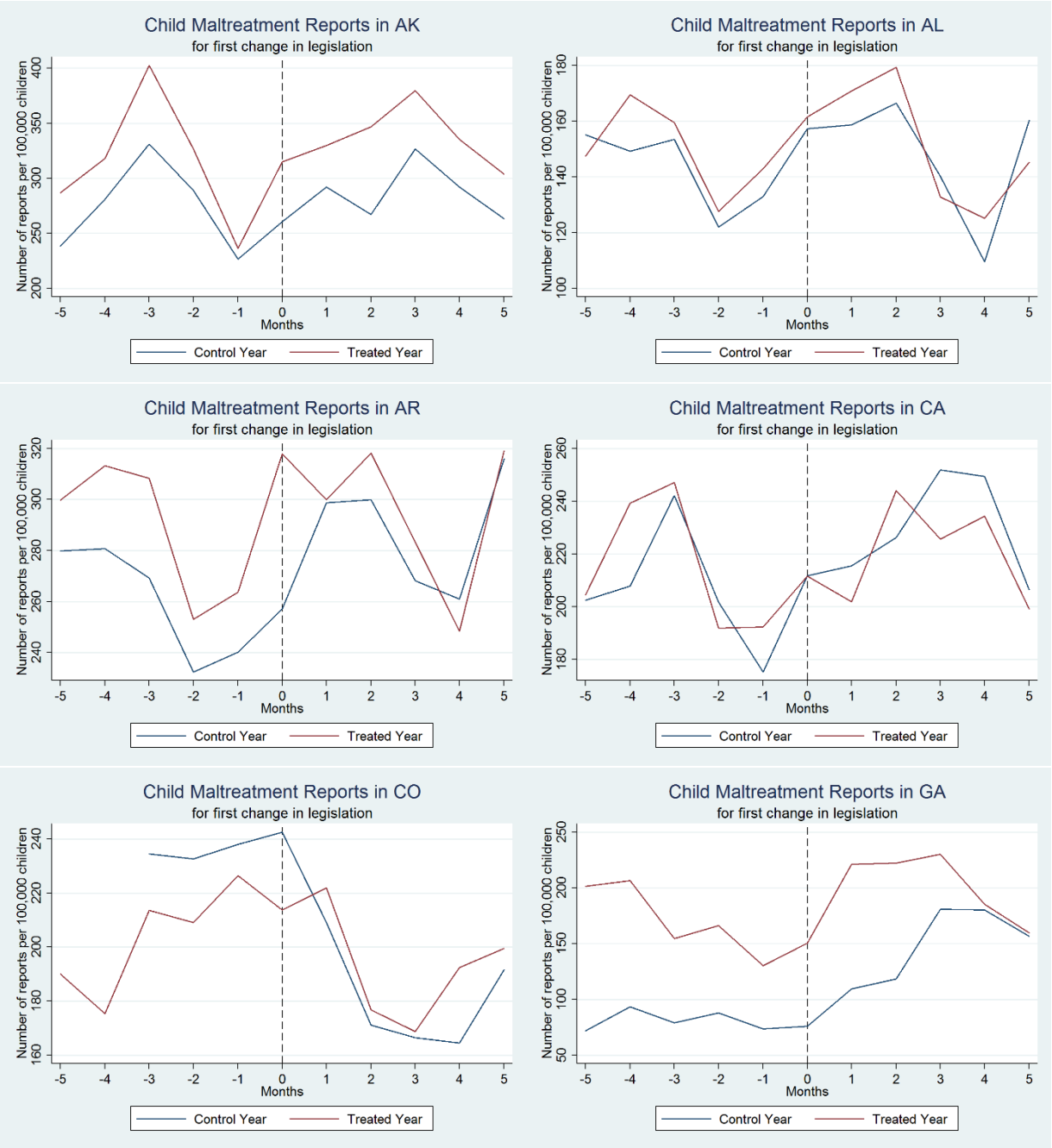
Notes: The report rate is calculated per 100,000 children. In practice, the number of reports screened-in is divided by the number of children and then multiplied by 100,000. The average is taken across all years.

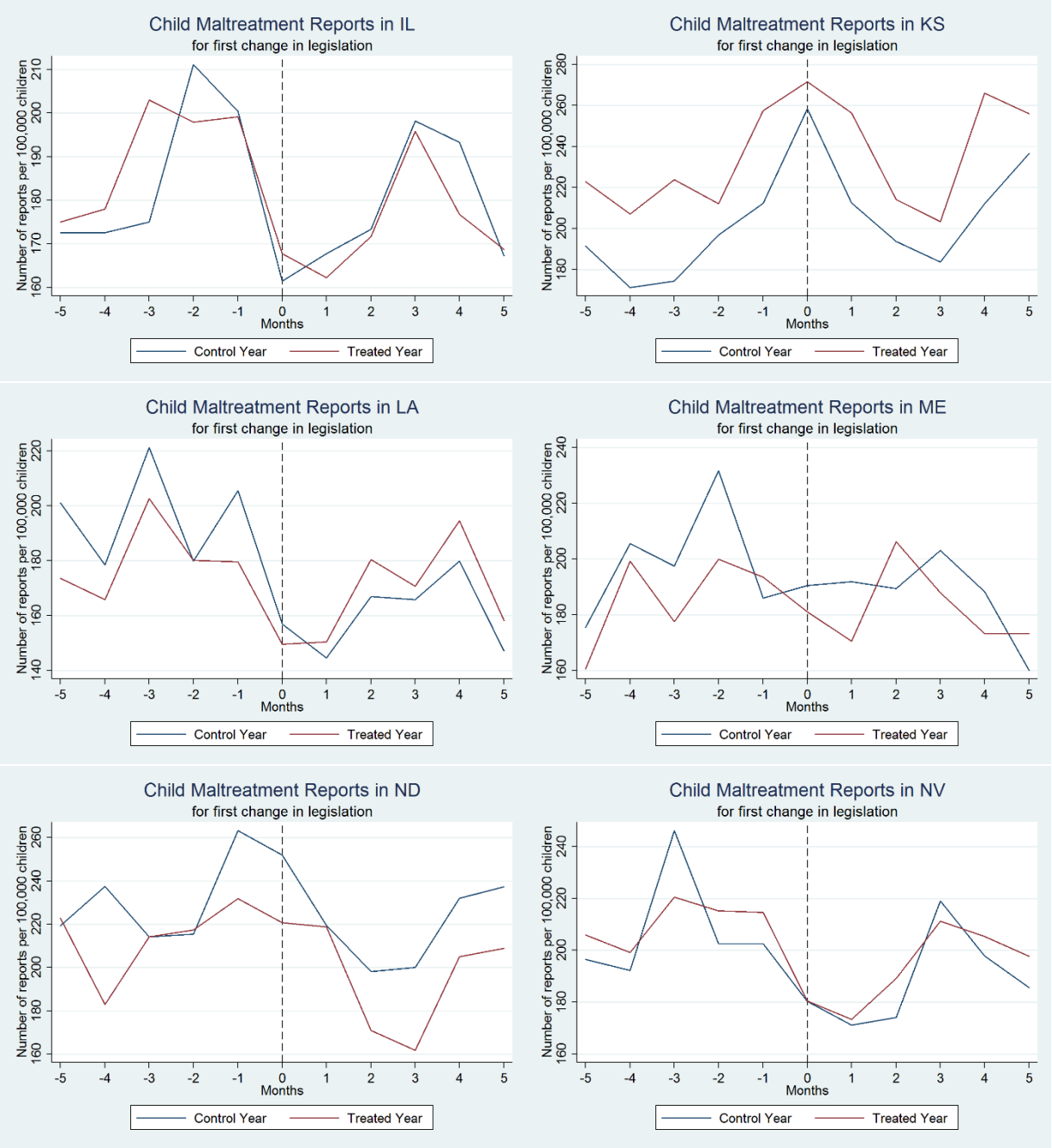
Appendix Figure 2: Average report rate by year

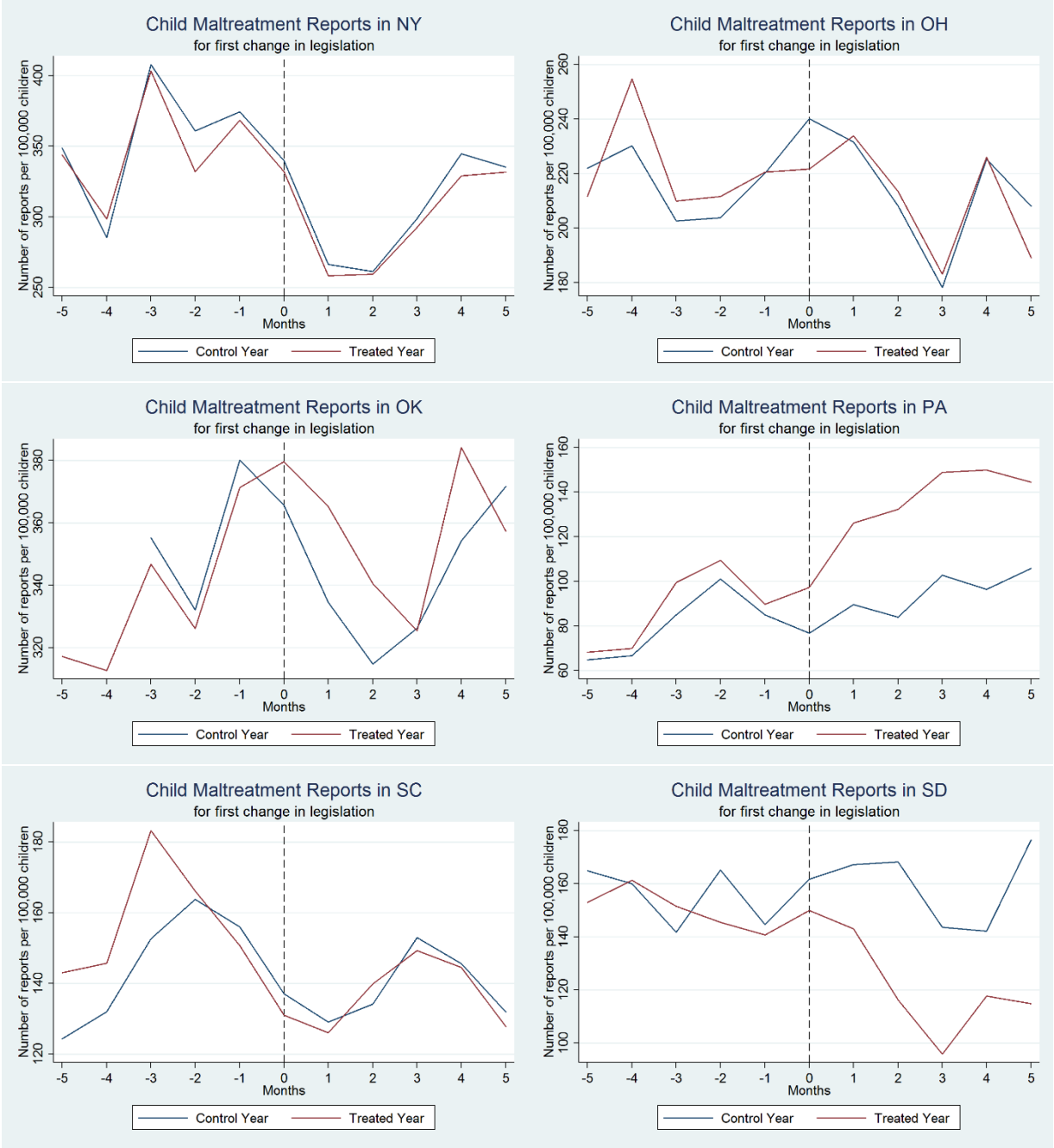


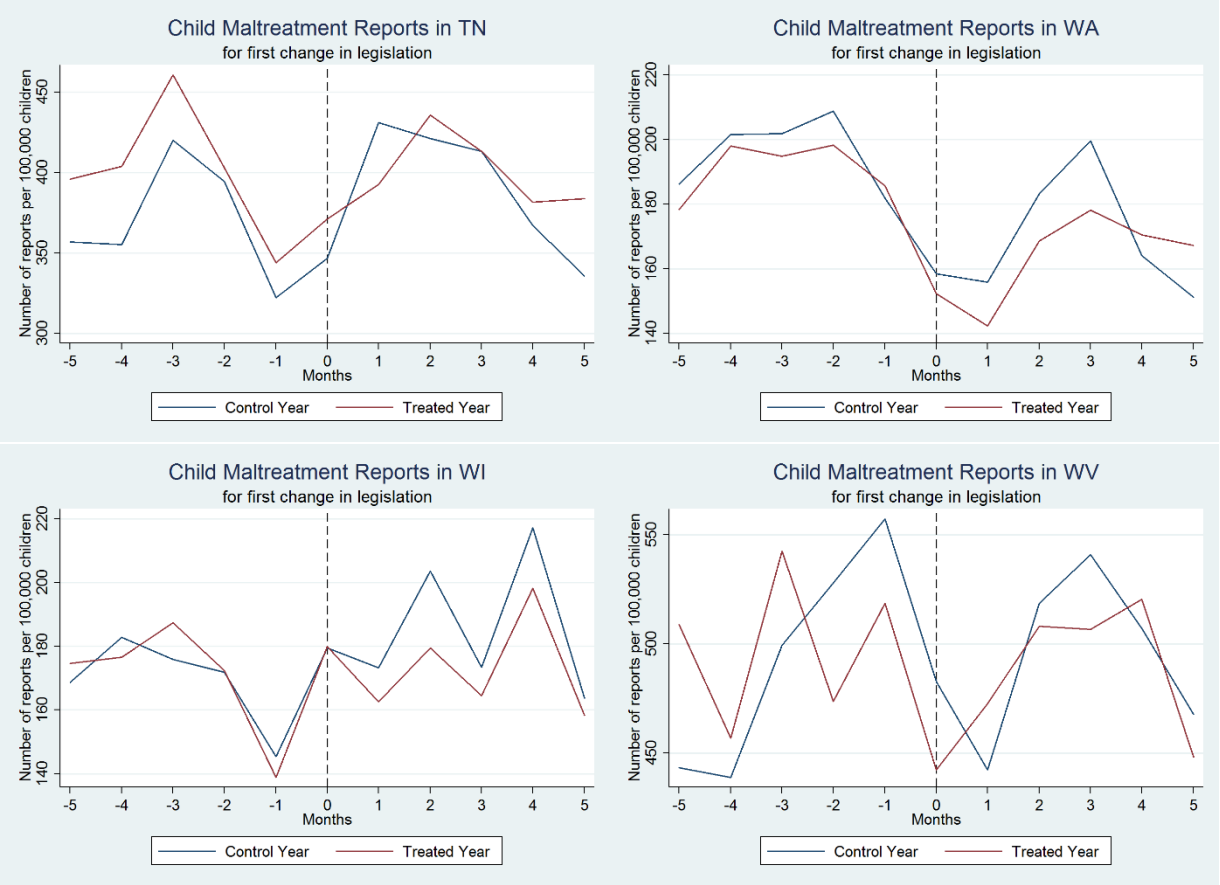
Notes: The report rate is calculated per 100,000 children. In practice, the number of reports screened-in is divided by the number of children and then multiplied by 100,000. The average is taken across all states and is weighted by the state population.

Appendix Figure 3: Selected charts of the report rate before and after the first mandatory reporter legislation change, relative to the previous year




Child Maltreatment Reports in IL
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in KS
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in LA
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in ME
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in ND
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in NV
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year


Child Maltreatment Reports in NY
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in OH
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in OK
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in PA
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in SC
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year
Child Maltreatment Reports in SD
for first change in legislation
Number of reports per 100,000 children
Months
Control Year
Treated Year



Notes: These charts plot the report rate, per 100,000 children, the five months before and after the mandatory reporter legislation change in the year the change occurred (treated year), relative to the previous year (control year) for a handful of states. Pennsylvannia is a good example of what we would expect to see if mandatory reporter legilsation led to an increase in reporting, and South Dakota is a good example of what we would expect if mandatory reporter legislation led to a decrease in reporting.

Appendix Table 1: Group effects for mandatory reporter policy changes

Outcome Var.	Overall Effect	Group (1)	(2)	(3)	(4)	(5)
Reports (per 100,000 children)	12.90*	19.91	12.97	-3.913	20.05*	3.958
	(7.408)	(12.00)	(18.44)	(8.426)	(11.18)	(2.825)
Substantiated reports (per 100,000 children)	1.862	-0.648	0.432	-3.723	6.754	2.469***
	(2.363)	(4.014)	(2.852)	(3.317)	(4.287)	(0.883)
Unsubstantiated reports (per 100,000 children)	11.03*	20.55**	12.54	-0.190	13.29*	1.489
	(5.711)	(9.308)	(17.05)	(5.393)	(7.059)	(2.235)
Number of children investigated (per 100,000 children)	28.76	20.09	31.02	-8.825	51.92	11.96**
	(19.06)	(23.36)	(42.52)	(14.97)	(33.15)	(5.461)
Number of victims (per 100,000 children)	2.756	-3.597	0.648	-6.262	11.47	2.011
	(4.257)	(6.242)	(4.587)	(5.381)	(8.434)	(1.471)
Observations	8,422	5,398	5,376	4,872	4,704	4,200
Number of Treated States	27	9	8	5	4	1

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each cell reports the beta coefficient and standard error for the outcome-group regression analysis combination. The first column reports the overall results again, and then the remaining columns report the results for groups 1 through five. In agreement with Callaway and Sant'Anna (2020), the overall effect is similar to the weighted average of the group effects. Group 1 consists of the 9 states that only had one mandatory reporter legislation change, group 2 consists of the 8 states with two changes, etc. In addition to the treated states, the 24 control states are included in all analyses. These are the states that did not change their mandatory reporter legislation between 2004 and 2017. All regressions include state fixed effects, year-by-month fixed effects, and a state-specific linear time trend.

Appendix Table 2: Short-run effects of the legislation change that added athletic coach, college staff, or camp staff between 2012 and 2014

	(1) Reports (per 100,000 children)	(2) Substantiated reports (per 100,000 children)	(3) Unsubstantiated reports (per 100,000 children)	(4) Number of children investigated (per 100,000 children)	(5) Number of victims (per 100,000 children)
Post	5.145	0.237	4.908	1.930	-0.358
	(10.69)	(3.292)	(8.087)	(17.98)	(5.354)
Treatment	7.127	-0.707	7.835	-1.530	-1.681
	(18.69)	(5.877)	(13.85)	(32.34)	(9.410)
Post x Treatment	-6.168	-1.653	-4.515	-12.40	-2.716
	(4.111)	(1.724)	(4.019)	(7.629)	(3.362)
Mean of dep. var.	237.4	49.9	187.4	438.4	77.4
Observations	308	308	308	308	308
Adjusted R^2	0.384	0.201	0.381	0.296	0.163

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors clustered at the state-level are in parentheses. Each column is a unique outcome. "Post" is equal to one five months after the legislation change and zero for the five months prior. "Treatment" is equal to one in the year of the legislation change and zero in year prior. Post x Treatment estimates the effect in the months after the legislation change, relative to the same time period the year before. The mean of the dependent variable in the post months of the control year is provided. Regressions include state, year, and month fixed effects. The 14 states included in this analysis are Alabama, Alaska, Arkansas, California, Colorado, Georgia, Illinois, Louisiana, New York, Pennsylvania, Tennessee, Virginia, Washington, West Virginia.