Reducing Child Maltreatment: The Role of Mandatory Reporting Laws

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Abstract

Child maltreatment is responsible for substantial morbidity and mortality and has long-lasting effects on mental health, drug and alcohol misuse and criminal behavior. This paper evaluates the effects of the first policy targeted at the reduction of child abuse and neglect at home in the United States in early 1960's. The policy consisted of making mandatory the reporting of child maltreatment by physicians, after the discovery in the medical field of the "Battered Child Syndrome." This discovery increased national awareness about the extent of child maltreatment in the country, and by 1970, all fifty states had passed some form of mandatory reporting law. We exploit the staggered introduction of these laws across states to evaluate its effect on the number of cases reported and referrals to foster care and in measures of child wellbeing such as height, mortality, and mental health in adulthood. At this preliminary stage, we find that the policy reduced mortality for children under one year of age, especially in the states that had penalties in place associated to the failure of reporting. We do not find mortality decreases for other ages.

Keywords: child maltreatment, reporting laws, mortality

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I. Introduction

Child maltreatment, which includes both abuse and neglect, is a serious and prevalent public health problem in America. It is responsible for substantial morbidity and mortality and has long-lasting effects on mental health, drug and alcohol misuse and criminal behavior (Gilbert et al., 2009). Every year referrals to child protective services involve 6.6 million children, and around 3.2 million of those children are subject to an investigated report. In 2017 there were 674,000 victims of child maltreatment. This corresponds to a rate of 9.1 per 1,000 children in the population. The number of child fatalities amounts to 1,720 child deaths due to abuse or neglect (Children's Bureau, 2019). By age 18, about 30 percent of all children will have been victims in an investigated case of child maltreatment. While all reports are not confirmed, nearly 13 percent of all children will have a confirmed case of maltreatment by age 18 (Wildeman et al. 2014). Survey data suggest that rates of actual child maltreatment are even higher than officially reported rates; in 2011, researchers found that 4 in 10 children report experiencing maltreatment by the time they are ages 14 to 17 (Finkelhor et al. 2013).

A growing body of correlation-based evidence suggests that experiencing maltreatment is detrimental to children's health (Felitti et al. 1998 and Bruce et al. 2009;) and that maltreatment has significant costs for society (Currie and Widom 2010). Specifically, evidence shows that children who experience child abuse and neglect are three times more likely to die in childhood (Sabotta and Davis, 1992), are nine times more likely to become involved in criminal activity (DOJ, 1999 and Currie and Tekin, 2012), are more likely to experience mental health problems (Mills et al. 2013, Kisely et al. 2018), and to abuse alcohol and drugs (Jaudes et al. 1995).

Early detection is key in stopping maltreatment and in helping children recover from its negative effects, yet factors that drive early detection remain understudied. Professionals in child health, primary care, mental health, schools, social services, and law-enforcement services all contribute to the recognition of and response to child maltreatment, however, in all sectors, children suspected of being maltreated are under-reported to child-protection agencies (Gilbert et al, 2009).In this paper, we aim at evaluating the first policy attempt to identify and report child maltreatment in the United States. In 1962, Kempe identified the so-called "Battered-Child Syndrome", a term that characterizes a clinical condition in young children who have received serious physical abuse, usually from parents (Kempe et al. 1962, JAMA). This study was motivated by observing several cases of a seemingly inexplicable pattern of injuries in young children, such as mysterious fractures and convulsions, without any known history of trauma. It was only after this medical "discovery" in 1962 that child abuse was recognized as a regular and recurring aspect of family life, not a sensational exception but a common syndrome. This publication generated

great public unrest and led to a rapid change in legislation that had the objective of identifying and stopping child maltreatment, by making mandatory the reporting of cases by physicians. Over time, states implemented various updates to these laws to address possible shortcomings that limited the efficacy of this legislation. States augmented the number of professions for whom it was mandatory to report an abuse, including police officers, school teachers, and social workers; imposed fines or penalties when failing to report; protected this professionals from all liabilities related to the reporting, and waived confidentiality agreements, e.g. doctor-patient confidentiality agreement. How did this policy intervention affect national child maltreatment? To the best of our knowledge, there is no systematic quantitative study of the effect of this policy intervention on national child maltreatment rates.

Assessing the extent of child maltreatment in the 1960s and 1970s is a major challenge due to the lack of official statistics. To fill this gap, we plan to collect individual-level archival data on child abuse reports and foster care files for the period of 1940-1980 for Kentucky, Maryland, New York and Ohio. We focus on these states first since they have archival data on child abuse before and after the introduction of the mandatory reporting laws. We will use the archival data to document how the number of cases and the composition of foster homes changed for the period under analysis.

We exploit the staggered introduction of child maltreatment reporting laws across states to examine its effect on child wellbeing. The identification strategy relies on the natural variation resulting from the different timing of the adoption of mandatory reporting laws across states. We use an event-study research design at the cohort-level to estimate the dynamic response of each outcome of interest to the law change. We exploit variation in the introduction of reporting laws by physicians (first wave), school-teachers (second wave), and whether the law imposed penalties when failing to report a case. We evaluate the introduction of these laws in terms of infant and child mortality, and longer run outcomes such as height and mental health variables.

The data to conduct this research comes from different sources. Information on the dates when each state passed a mandatory reporting law is self-collected from States Session laws. We recorded the specific year and statutory code that mandates physicians, teachers, and other agents to report as well as the penalties and responsibilities imposed by the laws. Data on child mortality is assembled from two sources: the main source is the Multiple cause-of-death Mortality Data from the National Vital Statistics System of the National Center for Health Statistics tabulated by NBER for the years 1959-2017. These data provide mortality data by multiple causes of death for all deaths occurring within the United States. We extended these data by digitalizing the period 1951 - 1959 from the Vital Statistics of the United States volumes. To study the long-run effects of mandatory reporting laws we used data on mental health,

family relationships, risky behaviors, and drug and alcohol use for the cohorts born from 1959 to 1990. Restricted access to the individual-level data from the National Survey on Drug Use and Health (NSDUH) has been approved and we are waiting for the background checks and security clearing to access these data on the Census RDC Center at UCLA. Data on height by age and gender comes from waves National Health Examination Survey (NHES) waves I, II and III and National Health and Nutrition Examination Survey (NANHES) waves I, II, III and 1999-2000, and subsequent continuous waves up to the year 2017-2018.

Our results suggest that the introduction of mandatory reporting of child maltreatment by physician reduce infant mortality in around 8%. This result arrived with lag, the first years after the adoption saw almost no changes in mortality, but after 4 years infants' deaths declined increasingly until they reached a total decline of 1.9 deaths per 1000 infants. This amounts to a reduction of 2,573 deaths per year. We do not find evidence of reductions in mortality at other ages (1 to 4 years), or related to the inclusion of teachers as reporters. We hypothesize that this null result is related to the very low levels of child mortality above 1 year of age, and not the inefficiency of the policy. We will be able to provide more evidence on this once we have completed our analysis on other child and adult outcomes.

The contribution of this paper is twofold. First, it provides policymakers with an evaluation of how effective are the often-used policies of mandatory reporting of crimes such as sexual and work harassment for example. Second, it contributes to the literature on the impact of interventions for children from disadvantaged backgrounds and broadens our understanding of how the stressors that many children face early in life affect their emotional development and behaviors in adult life.

II. Child protective services in the early 20th century and the role of mandatory reporting

The history of child protection in America was until very recently one of private charitable efforts, and one only available to children in large cities. Organized child protection emerged in New York City after the rescue in 1874 of 9-year-girl, who was routinely beaten and neglected by her guardians. A religious missionary who learned about the girl's condition consulted the police and child-helping charities, but they lacked interest or authority to intervene in the case. Eventually, help came from the founder of the American Society for the Prevention of Cruelty to Animals. By this time the institutions around animal cruelty were more developed, and thus his founder would in 1875 found the New York Society for the Prevention of Cruelty to Children (NYSPCC), America's first entity devoted entirely to child protection. Soon after, by 1922, around 300 nongovernmental child protection societies were scattered across America (Myers, 2004). This progress was stopped by the Great Depression, which hastened the demise

of nongovernmental child maltreatment societies. Private contribution fell dramatically, and by 1956, only 84 nongovernmental agencies provided services (DeFrancis, 1956). By this time most of the children these societies looked after were orphans and child workers, and there was little awareness of abuse and maltreatment at home, and instead it was widely believed that the safest place for a child is with their family, and that a parent knows what is best for their child.

The 1960s witnessed an explosion of interest in child maltreatment inside the family, and physicians played a key role in this awakening. Prior to this decade, medical schools provided little or no training on child abuse, and medical texts were largely silent on the issue. Even pediatricians were largely uninformed. In 1962 the publication of the blockbuster article "The Battered-Child Syndrome" by pediatrician Henry Kempe and his colleagues, played a leading role in bringing child abuse to national attention during the 1960s and 1970s. Following this publication, national news outlets like Newsweek, Saturday Evening Post, Parents magazine, Time magazine, Good Housekeeping, and Life turn their attention to child maltreatment and published emotional stories of abuse, often citing "The Battered-Child Syndrome" and Henry Kempe. Local media had always covered noteworthy cases, as when a child was beaten to death, but coverage by national media was uncommon prior to the 1960s.

State legislators responded quickly to these developments and by 1967 all states had passed laws regarding mandatory reporting of child maltreatment. The first state to pass a mandatory reporting law was the state of California in 1962, followed by Colorado, Florida, and Idaho in 1963, Kentucky, Louisiana, New Jersey, Rhode Island, and South Dakota in 1964.In most cases the reporting responsibility fell exclusively on physicians and other medical professionals. Soon after the first laws were passed, it was evident that the first wave of legislation was incomplete and, states started to add other professionals to the list of persons for whom it was mandatory to report, such as: school teachers, police officers and social workers among others. Some states also imposed fines for failing to report a case, provided liability protection for reporters and waived patient confidentiality agreements.

Prior to 1974, the federal government played only a minor role in child protection. The Children's Bureau was founded in 1912, but the bureau paid little attention to maltreatment until the 1960s. The Social Security Act of 1935, as amended in 1962, provided money to expand child welfare services. It was only until 1974 that Congress assumed a leadership role with passage of the Child Abuse Prevention and Treatment Act CAPTA authorized federal funds to improve the state response to physical abuse (PA), neglect, and sexual abuse. CAPTA focused attention on improved investigation and reporting.

III. Hypothesized Effects of Mandatory Reporting Laws of Child Maltreatment

Early detection is key in stopping maltreatment and in helping children recover from its negative effects. In practice, identifying child maltreatment as early as possible depends on early, consistent observation of the child by individuals likely to report the maltreatment they witness or signs of it that they observe. As Becker (1991) notes, child abuse may be viewed as an exception to the typical altruism found within families. Economic models of crime appear to be more fruitful than models of altruism in the study of child abuse and neglect (Doyle and Aizer, 2018).

Child maltreatment reporting relies *exclusively* on agents in contact with the victims since very few actions can be taken by children to report abusive behavior.[1] This is different to the reporting of other crimes where victims' propensity to report could be affected by their expected benefits under the current state of the legislation. Next, we formalized our main hypotheses for the effect of mandatory reporting laws on child abuse and neglect reporting by modeling the behavior of physicians and potential offenders.

Physicians – We model the physician's behavior as the main agents expected to report alleged cases of maltreatment, but the main forces driving their behavior will also be relevant in the behavior of other mandated reporters such as teachers and police officers. We denote the decision to report an alleged case of maltreatment by $\kappa = \{0,1\}$. Reporting ($\kappa = 1$) occurs when the expected benefits from reporting exceed the cost. If there were no cost of reporting, a physician would repot an alleged case of maltreatment if the level of suspicion is high enough. That is, each physician could be thought to have some threshold θ^* above which she shall report and comply with the law. Put another way, we model the "reasonable suspicion" criterion propose by the legislation as a threshold rule. This threshold is a function of the injuries detected after child's examination; the explanation given by parents of the injuries, and all the information from which the physician generate a level of suspicion. This threshold varies across physicians. Flahertyet al. (2008) finds that the physician's level of suspicion for abuse is a strong indicator of whether the physician would report a child to Child Protective Services. Nonetheless, factors other than levels of suspicion play a role in the decision to report: knowledge of and experiences with the family and knowledge about previous CPS involvement (Jones, 2008). In our stylized model, we abstract of these other factors and denote the level of suspicion by θ . Under these assumptions: $P(\kappa = 1) =$

[1]The range of these actions varies by age, with younger children having very limited options and elder children being able to speak out to relatives or teachers. Also, granting more rights to kids could potentially increase their awareness of abuse. We abstract from the responses of children to defend themselves or report abuse. Thus, in our model, we assume that the change in regulation did not trigger a response from children.

$P\ (\theta \ \geq \theta^*)$ and the expected (net) benefits from reporting are given by $V_p = P\left(\theta \ \geq \theta^*\right) \times v$ where v denotes the value of reporting for any physician and could be normalized$v = 1$.

The cost of reporting can include fear of retaliation against the victim (e.g. less medical checks) as well as retaliation against the physician herself. The laws directly tackled this last concern as almost every state put in place some type of immunity to protect reporters at the time of mandating reports.[2] Specifically, the reporting laws also provide criminal and civil liability, protecting the reporter in any judicial proceeding resulting from the report. At the same time, the legislation provided abrogation of the physician-patient privilege, reducing the legal concerns involved in a report. Thus, we expect mandatory reporting to decrease the cost of reporting by providing guarantees to reporters.

Some states also incorporate penalties from failing to report. Those states that include a penalty, consider a willful failure to report to be a misdemeanor punishable by a fine ranging from $100 to $500 (approximately $574.18 to $2,871 in 2016 dollars), or between 15 days to 6 months of imprisonment or both. The inclusion of a penalty represents an enforcement mechanism of the law. We model the presence of a penalty as a negative cost of reporting. Thus, the cost of reporting in a state with imposed fines for not reporting is higher than the cost of reporting in a state without such fine. Put another way, our model predicts that the introduction of mandatory reporting with a penalty leads to a sharper reduction in the cost of reporting. Denote the cost function of reporting by C_p, the net benefits from reporting are given by

$$V_p = P\left(\theta \ \geq \theta^*\right) - C_p(\ inmunity, penalty, other)$$

Under these assumptions, a physician reports a case c if expected benefits from reporting exceed the cost, i.e. if $V_p \geq 0$. The introduction of mandatory reporting laws represent a reduction of the cost of reporting, thus we expect some physicians will be shift into reporting augmenting the number of reports. [3]

Potential Offenders -In this paper, we model perpetrator's behavior as in Miller A. and Segal, C (2018) and consider the offender a rational agent who chooses to commit a crime if the net expected benefit of committing a crime is greater than the net benefit of not doing it. We normalize the net benefit of committing abuse or neglect to zero. Thus, a potential offender will commit the crime if and only if, the

[2]Specifically, all states except California stated the immunity of reporters in their first mandatory reporting law. California did so 3 years later (by 1965) the fist legislation on mandatory reporting (1962). We will come back to this discussion when looking at heterogeneous effects based on features of the laws in the results section. For the purpose of the model we abstract from this distinction and consider that all states have implemented some immunity.

[3]Mandatory reporting laws also increase awareness of maltreatment, changing the threshold rule followed by physicians. More aware physicians are better at detecting maltreatment, thus $P(\theta \ \geq \theta^*)$ goes to 1 as the level of suspicion is more accurate. The sign of this change is observationally equivalent to a reduction in reporting costs. In this version, we abstract from this channel putting all the action in the cost channel.

expected benefit of doing it is positive. The expected costs of committing a crime are the criminal and social penalties if caught times the probability of getting caught. We hypothesized that the introduction of mandatory reporting increases the probability of being caught, which will increase the expected cost of committing child maltreatment. Also, mandatory reporting increases awareness of the negative consequences faced by neglected and battered children, which will decrease the net benefit of abuse. Thus, we expect some potential perpetrators to stop abusive behavior or reduce the abusive behavior rates. Nonetheless, often perpetrators are the ones in charge of taking the children to the doctor office or send them to school. Thus, perpetrators will also reduce the rate at which they expose children to reporters, which in turn could decrease the numbers of observed cases without an *actual* change in maltreatment incidence.

We have identified two forces that act in opposite directions. On the one hand, the introduction of mandatory reporting laws is expected to shift some physicians into reporting; augmenting the number of reports. Nonetheless, the incidence of child maltreatment is expected to decrease due to a higher probability of being caught (or a smaller rate of physician's visits), reducing the number of reports. Thus, whether reporting of child maltreatment cases increased as a result of the change in legislation is an empirical question that we intended to address in the next section.

IV. Data

Data to conduct this research comes from different sources. Information on the dates when each state passed a mandatory reporting law has been self-collected from States Session laws. We recorded the specific year and statutory code that mandates physicians, teachers, and other agents to report, as well as the penalties and responsibilities imposed by the laws. Tables A.1. and A.2. present a list of the dates when each state mandated physicians (first wave) and teachers to report (second wave). We also summarize this information in Figure1, showing the distribution of the number of states with a mandatory reporting law for each year. Early-adopters are more likely to only mandate physicians to report while late-adopters are more likely to enact "comprehensive" reporting laws.

Data on child mortality is assembled from two sources: the main source is the Multiple Cause-of-Death Mortality Data from the National Vital Statistics System of the National Center for Health Statistics tabulated by NBER for the years 1959-2017. These data provide mortality counts by multiple causes of death for all deaths occurring within the United States. We extended these data by digitalizing the period 1951 - 1959 from the Vital Statistics of the United States volumes. The goal of this exercise is to observe a longer period of time to assess the pre-trends assumption of our main empirical strategy. Figures 2 and 3

present aggregate and state level trends of infant mortality (less than one years of age) centered at the time of the law change.

To study the long-run effects of mandatory reporting laws on adult behavior, we use data on mental health, family relationships, risky behaviors, and drug and alcohol use for the cohorts born from 1959 to 1990. Individual-level data for these outcomes are observed on the restricted access data from the National Survey on Drug Use and Health (NSDUH). [4]

Data on height by age and gender comes from waves of the National Health Examination Survey (NHES) waves I, II and III and National Health and Nutrition Examination Survey (NANHES) waves I, II, III and 1999-2000. We look at height for two reasons. First, while the number of fatalities is a subset of the number of abuse children, adult height is sensitive to environmental conditions experienced in childhood. The correlation between height in childhood and adulthood is approximately 0.7 for both men and women, so that tall children are much more likely to become tall adults (Case and Paxson, 2008). Also, children who experience deprivation may experience an extension of the growth period that can last several years (Steckel, 1995). While, an extended adolescent growth spurt can help shorter children gain a similar amount of height as other children do during adolescence, on average, this does not erase height deficits that developed in early childhood (Martorell et al 1994, Martorell et al 1990, Satyanarayana et al 1989, Hack et al 2003). Second, height has been liked to economic earnings; taller adults hold jobs of higher status and, on average, earn more than other workers (Case and Paxson, 2008).

V. Empirical Strategy & Results

The identification strategy relies on variation resulting from the different timing of the adoption of mandatory reporting laws across states. We use an event-study research design at the cohort-level to estimate the dynamic response of each outcome of interest to the law change. We employ OLS to estimate the causal effect of the law change via the following regression:

$$y_{sc} = \sum_{\tau=-5}^{10} \delta_{\tau} [Timetotreatment = \tau_s] + \delta_{-6+} BIN_{low} + \delta_{11+} BIN_{\square ig\square} + \Gamma X_{sc} + \alpha_s + \alpha_c + \varepsilon_{sc} \quad (1)$$

Where *s* denotes the state and *c* denotes cohort, y_{sc} is the outcome of interest, e.g. mortality rate of children younger than 1 year old. $\boldsymbol{X}_{sc}$ is a vector of control variables that includes state-time varying

[4]Access to these data has been approved by the National Center of Health Statistics (NCHS) but the researchers are waiting to get approval to access Census Bureau facilities.

controls.[5] The variable *Time to treatment* is equal to the difference between the calendar year and the year in which the state law was passed (denoted by τ). The coefficients δ_τ are reported as the change in the deaths per 1000 infants due to the adoption of mandatory reporting laws τ number of years ago. These coefficients map out the full dynamic response of the mortality rate to the law change. States fixed effects (α_s) control for variation in outcomes across states that are constant over time. Cohort fixed effects (α_c) control for variation in outcomes over time that is common across all states. ε_{sc} is an error term, standard errors are clustered at the state level. Our preferred specification equation (1), follows Schmidheiny and Siegloch (2019), who recommend to address the identification of dynamic treatment in this setting by binning endpoints: BIN_{low} takes the value of one if $Time\ to\ treatment \in [-10,-6]$ similarly BIN_{high} takes the value of one if $Time\ to\ treatment \in [11,15]$. That is, we introduce parameter restrictions that help to separate trends in the dynamic treatment and secular time effects: we assume that the effect δ_τ is constant for all $\tau \in [-10,-6]$. Specifically, we impose: $\delta_{-10} = \delta_{-9} = \delta_{-8} = \delta_{-7} = \delta_{-6} = \delta_{-6^+}$. Note that the analogous argument holds for δ_{11^+}.

Figure 4 shows the event-study coefficient plot of equation (1). Table 1 shows the regression estimates without any control (column 1), and with controls (column 3), columns 2 and 4 show estimated effect in percentage terms. According to our estimation, first there is no evidence of the presence of a trend before the introduction of the legislation, and these coefficients are jointly zero with a p-value of 0.92. Second, there is evidence of declines in infant mortality after the introduction of the laws. By the end of our estimation period we find that infant mortality rates decreased by 1.9 infants per 1000, which translates into an 8% fall. This estimate is statistically significant at the 10 percent level, as well as all the leads from δ_4 to δ_{10}.

To gain efficiency in our estimation we follow Bailey and Goodman-Bacon (2015), who proposed a binned estimation of the parameter in equation (1). We estimate the following semi-parametric version of equation (1):

$$y_{sc} = \delta_{PRE}\, 1[\tau < 0] + \delta_{+3} 1[\tau \in [1,3]] + \delta_{+6} 1[\tau \in [4,6]] + \delta_{+9} 1[\tau \in [7,9]] + \delta_{+12} 1[\tau \in [10,12]] + \delta_{+15} 1[\tau \in [11,15]] + \Gamma X_{sc} + \alpha_s + \alpha_c + \varepsilon_{sc} \quad (2)$$

Where $\delta_{+\tau}$ captures the effect of the mandatory reporting law (up to) τ years after the law's adoption. This specification summarizes the results of the event-study and reduces the number of coefficients to be

[5] Our controls include share of rural population, share of black population, share of population under the poverty line, unemployment rate, and share of babies born with low weight (less than 2,500 gr).

estimated. In Table 2 we show the results on equation (2), and find that the point estimates are very similar to our baseline regression, suggesting that the results are robust to alternative binning of the pre and post period, and as expected our standard errors are smaller.

When treatment effects change (monotonically) over time, the difference-in-difference (DD) estimate is biased away from the sign of the true effects (Goodman-Bacon, 2019).[6] We provide evidence of such pattern in the estimated treatment effects (Figure 4 and Table 1) so, we refrain from providing a DD estimate of the treatment effect; we summarize the coefficients using the semi-parametric model of equation (2).

Also, Borusyak and Jaravel (2017) advice against the capped specification, i.e. binning end points. The main argument is that when treatment effects have strong dynamics, negative weights may arise due to forbidden extrapolations, i.e post-treatment periods are used to provide counterfactuals for the earlier ones. Thus, when treatment effects are not constant, as assumed in the binned or capped specification, the estimate of the bin coefficients becomes unreliable. But also, through the wrong choice of individual and time fixed effects, short-run effects will be biased. [7] In this case, Borusyak and Jaravel (2017) suggest to not to run regressions which impose any restrictions on the dynamics of treatment effects post treatment. But, estimate the dynamic treatment effects flexibly and average the coefficients manually with some weights. We choose just a simple average.[8]We present the results of this exercise in Table 3. To ease comparison between the two models, we present effect sizes that are directly comparable.

Results presented in Table 3 suggest that both models deliver similar estimates of the dynamic treatment effects. Differences are less than 1.3 percentage points. Also, point estimates from this alternative model are in the range of the confidence interval of our preferred specification. We also test directly the assumption that treatment effects after 10 years are constant. We cannot reject the null hypotheses (p-val = 0.2412) that all estimated coefficients for periods later than 10 years are jointly equal. This is evidence that the model is a good approximation and it lowers the concerns on the presence of negative weights.

[6]Other recent papers call the same result "negative weighting" (Abraham and Sun 2018, Borusyak and Jaravel 2017, de Chaisemartin and D'HaultfŒuille 2018, Strezhnev 2018). Negative weights refer to the way that a regression DD coefficient—even with identical common trends in counterfactual outcomes—weights together theoretical treatment effect parameter.
[7] We refer the reader to figure 4 of ,Borusyak and Jaravel (2017) to gain more insight on this result.
[8] Our sample is balanced around the time of treatment. Thus, a simple average amounts to picking weights in function of sample size.

Heterogeneous effects

As discussed in Section II, we expect the introduction of the law to change physicians and teachers likelihood of reporting a case of child maltreatment, especially in states where there is a penalty attached to failing to report a case. To examine this hypothesis, in Table 4 and Figure 5, we show the results of estimating the effects on infant mortality, separate in states with and without a penalty. We find that there are larger reductions in states where the mandate of a report came with a penalty. Specifically, 9 years after the law was passed in states with a penalty, we estimate a reduction of 1.8 deaths per 1000, whereas in states without it the reduction is of 0.8 infants' deaths. The standard errors are, however, large so the two estimates are statistically the same.

Finally, Figure 6 and Tables 5 show regression results for the specification in equation (1) for other age groups, children aged 1 to 4 years old. Our estimates suggest a null effect of the legislation on mortality of older age children. It is important to note, however, that mortality rates for older ages are very low compared to that of infants (less than one year).[9] Similarly, we exploit variation in the date school teachers are mandated to report child maltreatment following equation (1), and we find also null results (Table 6). We will examine other potential effects of maltreatment such as height and mental health, which will allow us to evaluate the effect of the laws in outcomes that may have higher incidence.

VI. Conclusions

State governments across America have passed legislation over the years to mandate the reporting of criminal conduct and the reporting of abuse of vulnerable individuals. The purpose of these laws has been twofold: public or community safety, or the protection of specific individuals who are limited in their ability to care for themselves: children, vulnerable adults, and victims of domestic partner violence. We study the first major effort to reduce domestic child abuse and neglect. This paper attempts to answer whether these laws achieved the goal of protecting abuse's victims and sheds light in what are the potential reasons for their effectiveness, or lack of it.

[9]For example, average mortality rate for infants is 20 per 1000, whereas it is 1.9 for one year olds and even smaller for older children.

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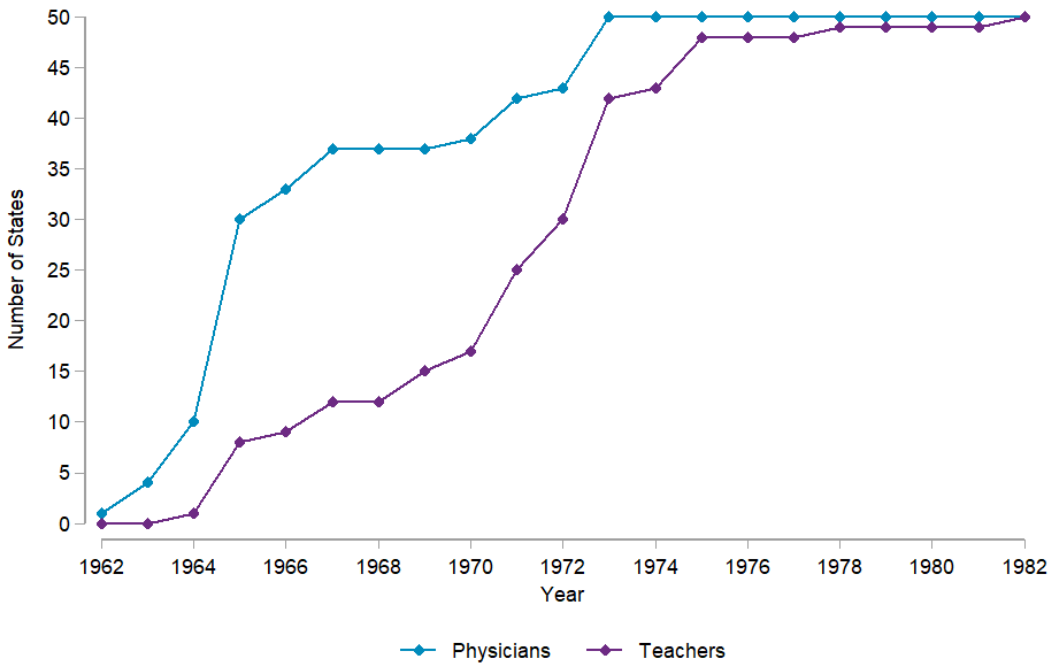
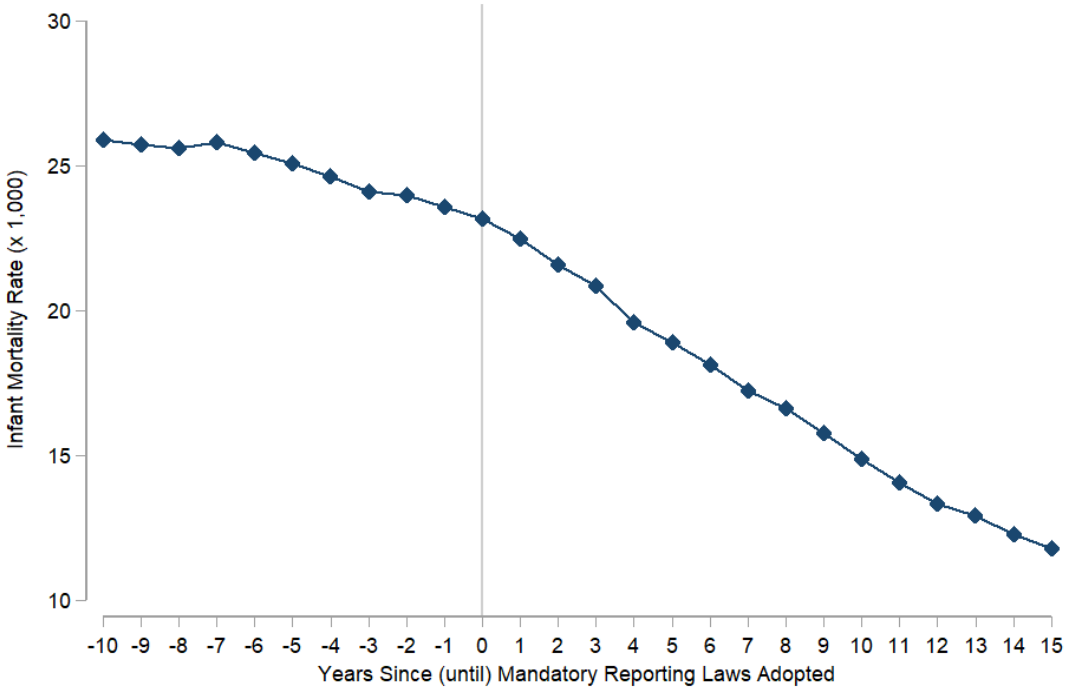
Figures and Tables

Figure 1. Distribution of Dates of Adoption of Mandatory Reporting Laws.



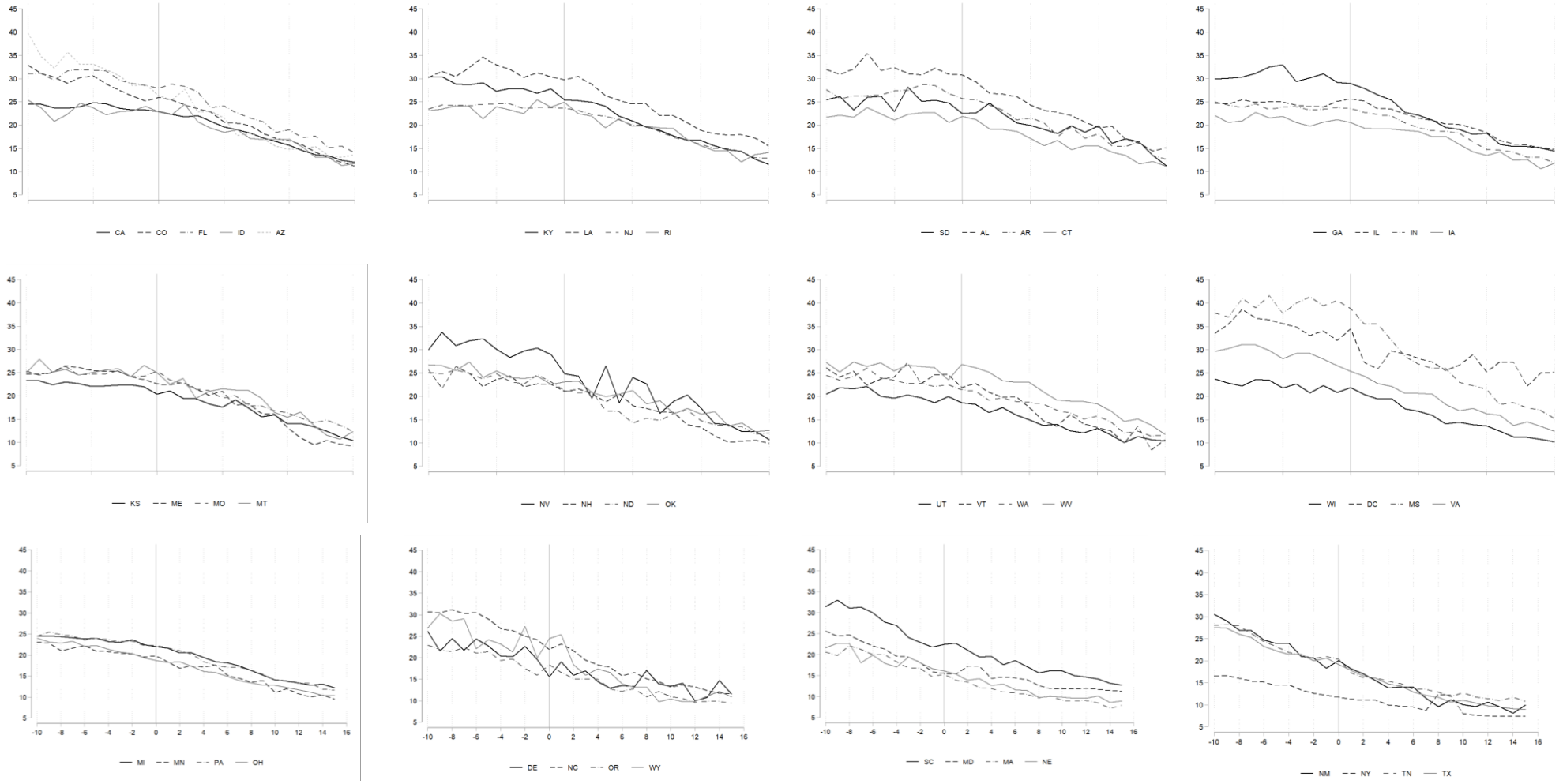
Notes: This figure shows the number of states with a enacted reporting law that mandates: (i) physicians to report(blue) and (ii) teachers to report (violet).Tables A.1. and A.2 provide details on when each state adopted the mandatory reporting law.

Figure 2. Aggregate Infant Mortality Rates



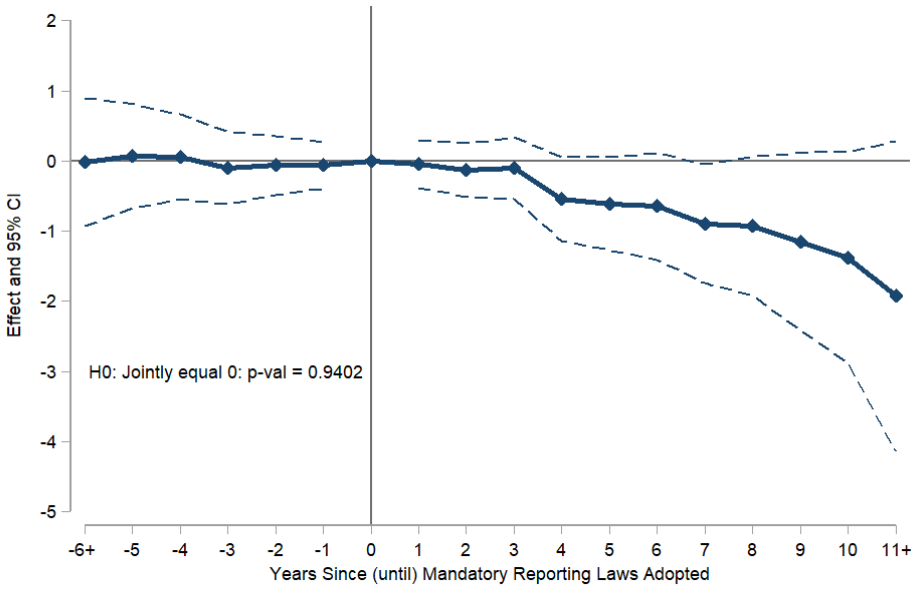
Notes: This figure shows aggregate trends of infant mortality (< 1 years of age) centered at the time of the law change. We define the aggregate infant mortality rate as the weighted average on the infant mortality rate, where weights are infants' deaths at the state level.

Figure 3. Trends in Infant Mortality Rates (x 1,000)



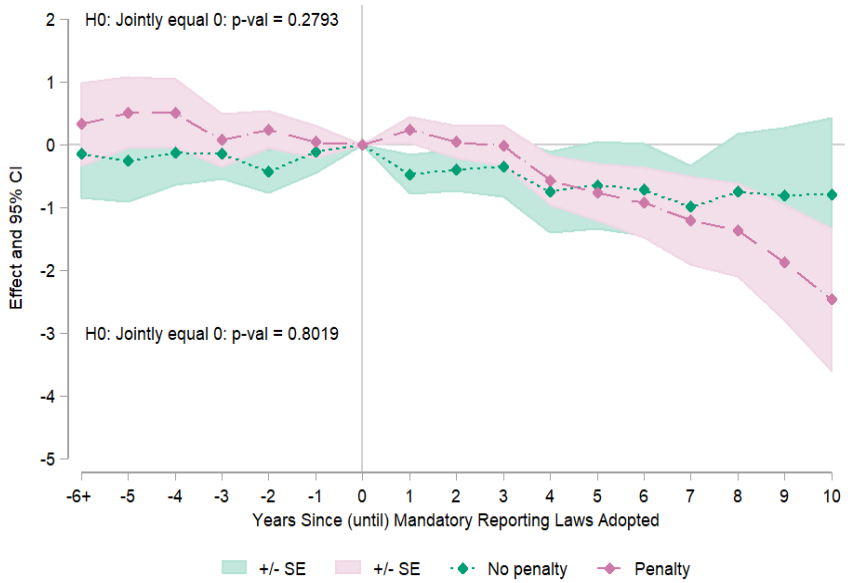
Notes: This figure presents trends in infant (< 1 year old) mortality rates by state. Trends are presented as a function of the years since (until) the mandatory reporting law was adopted in each state. States are grouped based on the year they first adopted a mandatory reporting law. See table A.1 for specific adoption dates.

Figure 4.Infant Mortality (<1 year old) and Adoption of Mandatory Reporting Laws



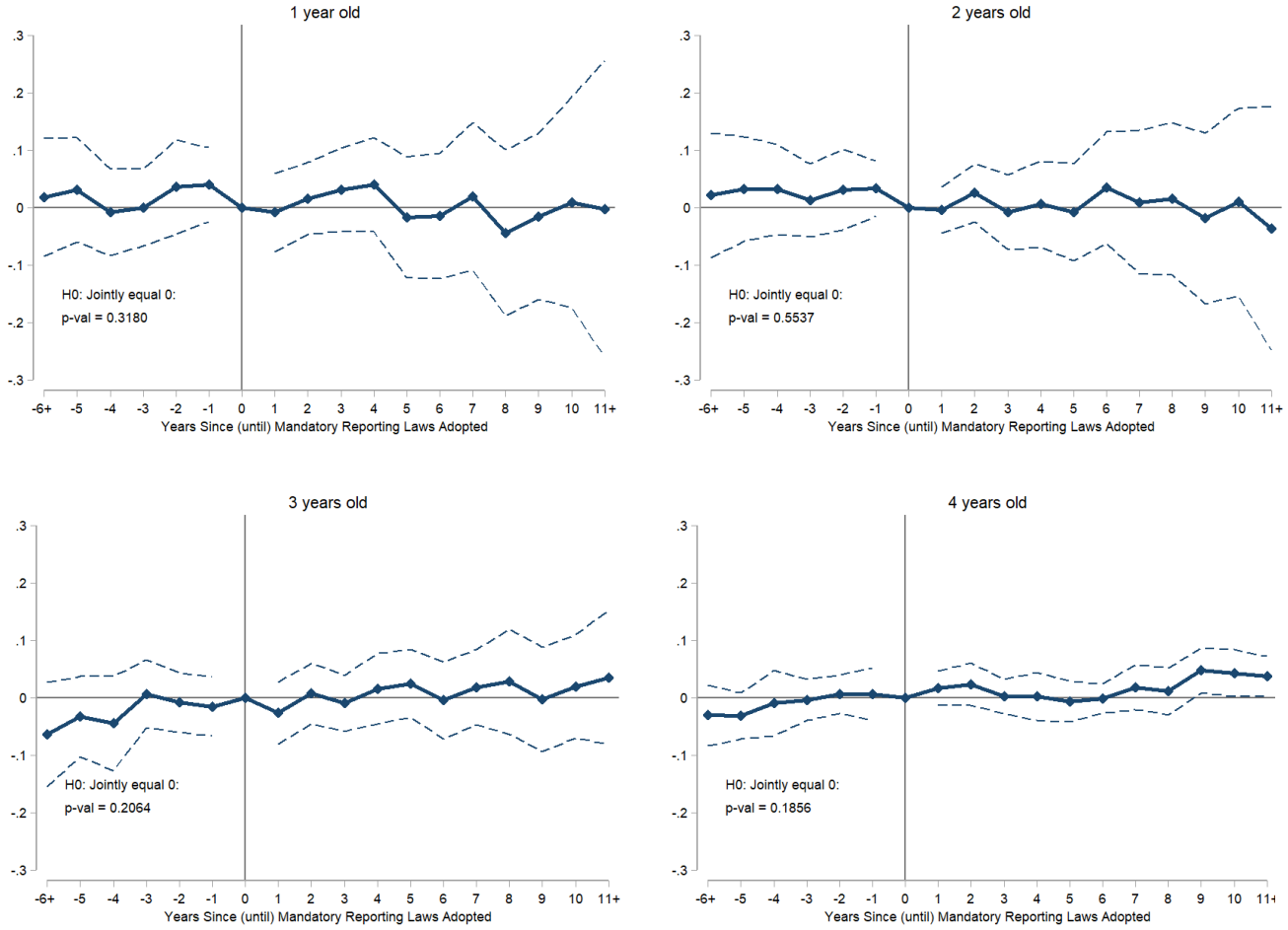
Notes: The dependent variable is infant mortality rate in state s for the cohort c. The average dependent variable in the year before the adoption of mandatory reporting is 22.85 infants deaths per 1,000 infants. The figure plots event-study estimates from equation (1) with no state times time varying controls. Dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered at the state level. The sample includes 49 states for 26 time periods measure relative to the treatment date. 6+ states for6 to 10 years until the law was adopted.11+ states for 11 to 15 years since the law was adopted. Detailed estimates are presented in table 1.

Figure 5.Event Study Infant Mortality (<1 year old) by penalty adoption.



Notes: The dependent variable is infant mortality rate in state s for the cohort c.The figure plots event-study estimates from equation (1) with no state times time varying controls, by the status of penalty adoption. Estimates are from separated regressions. Point estimates are presented in Table 5. 6+ states for6 to 10 years until the law was adopted.11+ states for 11 to 15 years since the law was adopted. Detailed estimates are presented in table 4.

Figure 6. Mortality Rate by Age (1 to 4 years old) and Adoption of Mandatory Reporting Laws



Notes: The dependent variable is the mortality rate at the age indicated in the top of each graph in state s for the cohort c. The average dependent variable in the year before the introduction of mandatory reporting is: 1 year old 1.493 (0.469) ; 2 years old 0.958 (0.236); 3 years old 0.772 (0.252); 4 years old 0.610 (0.180).The figure plots event-study estimates from equation (1) with no state times time varying controls. Dashed lines are pointwise 95 percent confidence intervals based on standard errors clustered at the state level. The sample includes 49 states. 6+ states for6 to 10 years until the law was adopted.11+ states for 11 to 15 years since the law was adopted. Detailed estimates are presented in table 5.

Table 1.Effects of Mandatory Reporting on Infant Mortality.

Infant Mortality	Coeff (1)	Effect size (2)	Coeff (3)	Effect Size (4)
1 year later	-0.039	-0.171	0.014	0.060
	(0.169)		(0.196)	
2 years later	-0.128	-0.560	-0.098	-0.430
	(0.189)		(0.189)	
3 years later	-0.104	-0.455	-0.092	-0.404
	(0.218)		(0.235)	
4 years later	-0.542*	-2.372	-0.503*	-2.201
	(0.297)		(0.288)	
5 years later	-0.609*	-2.665	-0.633*	-2.770
	(0.329)		(0.360)	
6 years later	-0.647*	-2.832	-0.528	-2.311
	(0.378)		(0.385)	
7 years later	-0.897**	-3.926	-0.655	-2.867
	(0.421)		(0.415)	
8 years later	-0.931*	-4.074	-0.790	-3.457
	(0.492)		(0.520)	
9 years later	-1.149*	-5.028	-0.736	-3.221
	(0.630)		(0.611)	
10 years later	-1.381*	-6.044	-1.037	-4.538
	(0.748)		(0.648)	
> 10 years later	-1.925*	-8.425	-1.623*	-7.103
	(1.100)		(0.840)	
N	1,273		1,273	
Mean MR	22.8500		22.8500	
SD mean MR	4.8177		4.8177	
F-test of joint significance (p-val)				
Pre-policy period	0.9402		0.8835	
Post-policy period	0.1977		0.0161	
State FE	Yes		Yes	
Cohort FE	Yes		Yes	
Other covariates	No		Yes	

Notes: The dependent variable is infant mortality rate in state s for the cohort c. The average dependent variable reported in the table corresponds to the year before the adoption of mandatory reporting. State and cohort fixed effects are included. The sample includes 49 states for 26 time periods measure relative to the treatment date.> 10 refer to 11 to 15 years since the law was adopted. Control variables included are: unemployment rate, share of white population, share of black population, share of population living in non-metropolitan areas, share of population bellow the (federal) poverty line, and share of babies born with low weight (less than 2,500 gr). Standard errors are clustered at the state-level. * p<0.10 ** p<0.05 *** p <0.01

Table 2. Effects of Mandatory Reporting on Infant Mortality – Semi parametric model

Infant Mortality	Coeff (1)	Effect Size (2)	Coeff (3)	Effect Size (4)
1 - 3 years later	-0.080	-0.350	-0.050	-0.214
	(0.161)		(0.155)	
4 - 6 years later	-0.584*	-2.556	-0.535*	-2.341
	(0.307)		(0.275)	
7 - 9 years later	-0.967**	-4.232	-0.705*	-3.085
	(0.469)		(0.406)	
10 - 12 years later	-1.576*	-6.897	-1.280**	-5.602
	(0.894)		(0.623)	
13 - 15 years later	-2.022	-8.849	-1.672*	-7.317
	(1.336)		(0.865)	
N	1,273		1,273	
Mean MR	22.8500		22.8500	
SD mean MR	4.8177		4.8177	
F-test of joint significance (p-val)				
Pre-policy period	0.8121		0.9376	
Post-policy period	0.2757		0.1777	
State FE	Yes		Yes	
Cohort FE	Yes		Yes	
Other covariates	No		Yes	

Notes: The dependent variable is infant mortality rate in state s for the cohort c. The average dependent variable reported in the table corresponds to the year before the adoption of mandatory reporting. State and cohort fixed effects are included. The sample includes 49 states for 26 time periods measure relative to the treatment date.> 10 refer to 11 to 15 years since the law was adopted. Control variables included are: unemployment rate, share of white population, share of black population, share of population living in non-metropolitan areas, share of population bellow the (federal) poverty line, and share of babies born with low weight (less than 2,500 gr). Standard errors are clustered at the state-level. * p<0.10 ** p<0.05 *** p <0.01

Table 3. Effects of Mandatory Reporting on Infant Mortality (<1 year old)

Alternative model: no binning end points

Infant Mortality	Binned end points		No binned end points	
	(1)	(2)	(3)	(4)
1 year later	-0.039	-0.171	-0.038	-0.152
	(0.169)		(0.180)	
2 years later	-0.128	-0.560	-0.118	-0.478
	(0.189)		(0.214)	
3 years later	-0.104	-0.455	-0.111	-0.449
	(0.218)		(0.246)	
4 years later	-0.542*	-2.372	-0.569	-2.303
	(0.297)		(0.362)	
5 years later	-0.609*	-2.665	-0.641	-2.595
	(0.329)		(0.431)	
6 years later	-0.647*	-2.832	-0.723	-2.927
	(0.378)		(0.501)	
7 years later	-0.897**	-3.926	-1.003	-4.060
	(0.421)		(0.602)	
8 years later	-0.931*	-4.074	-1.023	-4.141
	(0.492)		(0.612)	
9 years later	-1.149*	-5.028	-1.259	-5.096
	(0.630)		(0.794)	
10 years later	-1.381*	-6.044	-1.468	-5.942
	(0.748)		(1.020)	
> 10 years later	-1.925*	-8.425	-2.112	-8.550
	(1.100)		(0.712)	
N	1,273		1,273	
Mean MR	22.8500		24.7036	
SD mean MR	4.8177		4.9464	
F-test of joint significance (p-val)				
Pre-policy period	0.9402		0.5203	
Post-policy period	0.1977		0.2368	
State FE	Yes		Yes	
Cohort FE	Yes		Yes	
Other covariates	No		No	

Notes: The dependent variable is infant mortality rate in state sfor the cohort c. Column 1 and 2 present estimates from equation 1, columns 3 and 4 follow Schmidheiny and Siegloch (2019) and present estimates without binned end points. The average dependent variable reported in the table corresponds to the year before the adoption of mandatory reporting in column 1 and a simple average of the omitted pre-policy period in column 2. Effect sizes (colums 2 and 4) are computed as the ratio of the estimated coefficient and the mean of the dependent variable and are expressed in percentage changes. The sample includes 49 states for 26 time periods measure relative to the treatment date.> 10 refer to 11 to 15 years since the law was adopted. See text for computation of this coefficient in the model without binned end points. Standard errors are clustered at the state-level. * p<0.10 ** p<0.05 *** p <0.01

Table 4. Effects of Mandatory Reporting on Infant Mortality – by penalty adoption

Infant Mortality	Penalty (1)	No Penalty (2)
1 year later	0.242	-0.472
	(0.208)	(0.309)
2 years later	0.044	-0.402
	(0.252)	(0.329)
3 years later	-0.015	-0.346
	(0.320)	(0.475)
4 years later	-0.566	-0.753
	(0.391)	(0.645)
5 years later	-0.760	-0.646
	(0.453)	(0.690)
6 years later	-0.926	-0.720
	(0.558)	(0.732)
7 years later	-1.206*	-0.988
	(0.704)	(0.658)
8 years later	-1.361*	-0.754
	(0.738)	(0.934)
9 years later	-1.874*	-0.814
	(0.927)	(1.083)
10 years later	-2.470**	-0.788
	(1.133)	(1.213)
> 10 years later	-3.383**	-0.809
	(1.552)	(1.631)
N	675	595
Mean MR	21.5592	24.2581
SD mean MR	4.6000	4.7425
F-test of joint significance (p-val)		
Pre-policy period	0.2793	0.8019
Post-policy period	0.0845	0.0241
State FE	Yes	Yes
Cohort FE	Yes	Yes
Other covariates	No	No

Notes: The dependent variable is infant mortality rate in state s for the cohort c. The average dependent variable reported in the table corresponds to the year before the adoption of mandatory reporting.The sample includes 23 states in column 1 and 26 states in column 2, each for 26 time periods measure relative to the treatment date.> 10 refer to 11 to 15 years since the law was adopted. Standard errors are clustered at the state-level. * p<0.10 ** p<0.05 *** p <0.01

Table 5. Effects of Mandatory Reporting on Mortality. Children aged 1 to 4 years old

Mortality rate	1 year old (1)	2 years old (2)	3 years old (3)	4 years old (4)
1 year later	-0.008	-0.004	-0.026	0.018
	(0.034)	(0.020)	(0.027)	(0.015)
2 years later	0.016	0.026	0.008	0.024
	(0.031)	(0.025)	(0.026)	(0.018)
3 years later	0.032	-0.008	-0.009	0.003
	(0.036)	(0.033)	(0.024)	(0.015)
4 years later	0.040	0.006	0.016	0.002
	(0.041)	(0.037)	(0.031)	(0.021)
5 years later	-0.016	-0.007	0.025	-0.006
	(0.052)	(0.042)	(0.029)	(0.018)
6 years later	-0.014	0.035	-0.004	-0.001
	(0.054)	(0.048)	(0.033)	(0.013)
7 years later	0.020	0.010	0.019	0.019
	(0.064)	(0.062)	(0.033)	(0.019)
8 years later	-0.044	0.016	0.028	0.011
	(0.072)	(0.066)	(0.045)	(0.020)
9 years later	-0.015	-0.018	-0.002	0.0477**
	(0.072)	(0.074)	(0.045)	(0.020)
10 years later	0.010	0.010	0.020	0.0434**
	(0.091)	(0.081)	(0.045)	(0.020)
> 10 years later	-0.002	-0.036	0.036	0.0379**
	(0.128)	(0.105)	(0.058)	(0.017)
N	1,273	1,222	1,171	1,122
Mean MR	1.4927	0.9582	0.7719	0.6102
SD mean MR	0.4688	0.2364	0.2519	0.1801
F-test of joint significance (p-val)				
Pre-policy period	0.318	0.5537	0.2064	0.1856
Post-policy period	0.0187	0.1094	0.2012	0.0119
State FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Other covariates	No	No	No	No

Notes: The dependent variable is mortality rate in state s for the cohort c, at the age stated in the top of each column. The average dependent variable reported in the table corresponds to the year before the adoption of mandatory reporting. State and cohort fixed effects are included. The sample includes 49 states for 26 time periods measure relative to the treatment date.> 10 refer to 11 to 15 years since the law was adopted. Standard errors are clustered at the state-level. * p<0.10 ** p<0.05 *** p <0.01

Table 6. Effects of Mandatory Reporting on Mortality. Children aged 5 to 9 years old

Mortality rate	5 years old (1)	6 years old (2)	7 to 9 years old (3)
1 year later	0.003	0.002	0.007
	(0.015)	(0.015)	(0.018)
2 years later	-0.006	-0.001	0.021
	(0.018)	(0.022)	(0.019)
3 years later	-0.029	-0.010	0.022
	(0.022)	(0.023)	(0.022)
4 years later	-0.012	0.032	0.011
	(0.029)	(0.028)	(0.024)
5 years later	-0.013	0.015	-0.004
	(0.028)	(0.025)	(0.025)
6 years later	-0.031	-0.008	-0.007
	(0.037)	(0.029)	(0.026)
7 years later	-0.024	0.020	0.025
	(0.039)	(0.036)	(0.029)
8 years later	0.003	0.015	0.031
	(0.048)	(0.033)	(0.035)
9 years later	-0.008	0.037	0.029
	(0.049)	(0.036)	(0.036)
10 years later	-0.016	0.042	0.021
	(0.057)	(0.043)	(0.040)
> 10 years later	-0.018	0.032	0.045
	(0.071)	(0.048)	(0.043)
N	1,113	1,110	1,059
Mean MR	0.4955	0.4392	0.4103
SD mean MR	0.1686	0.1671	0.1254
F-test of joint significance (p-val)			
Pre-policy period	0.7681	0.3230	0.5608
Post-policy period	0.2224	0.3431	0.3084
State FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
Other covariates	No	No	No

Notes: The dependent variable is mortality rate in state s for the cohort c, at the age stated in the top of each column. The average dependent variable reported in the table corresponds to the year before the adoption of mandatory reporting. State and cohort fixed effects are included. The sample includes 49 states for 26 time periods measure relative to the treatment date.> 10 refer to 11 to 15 years since the law was adopted. Standard errors are clustered at the state-level. * p<0.10 ** p<0.05 *** p <0.01

Appendix A.1.Additional tables and Figures.

Table A.1. Dates of adoption of mandatory reporting laws by state.

State Name	Year	State Name	Year
California	1962	Utah	1965
Colorado	1963	Vermont	1965
Florida	1963	Washington	1965
Idaho	1963	West Virginia	1965
Arizona	1964	Wisconsin	1965
Kentucky	1964	District of Columbia	1966
Louisiana	1964	Mississippi	1966
New Jersey	1964	Virginia	1966
Rhode Island	1964	Michigan	1967
South Dakota	1964	Minnesota	1967
Alabama	1965	Pennsylvania	1967
Arkansas	1965	Ohio	1970
Connecticut	1965	Delaware	1971
Georgia	1965	North Carolina	1971
Illinois	1965	Oregon	1971
Indiana	1965	Wyoming	1971
Iowa	1965	South Carolina	1972
Kansas	1965	Maryland	1973
Maine	1965	Massachusetts	1973
Missouri	1965	Nebraska	1973
Montana	1965	New Mexico	1973
Nevada	1965	New York	1973
New Hampshire	1965	Tennessee	1973
North Dakota	1965	Texas	1973
Oklahoma	1965		

Notes: This table presents the year in which a given state has adopted a mandatory reporting law with physicians as the main reporting agents. Information about each law has been self-collected from States Session laws. States are sorted by the year in which physicians are mandated to report. We include the state abbreviation used in figure 1 in the second and fifth column. Statutory codes are available upon request to the authors.

Table A.2. Dates of adoption of mandatory reporting laws by state and reporter.

State Name	Physicians	Teachers	State Name	Physicians	Teachers
California	1962	1966	Utah	1965	1965
Colorado	1963	1969	Vermont	1982	1982
Florida	1963	1971	Washington	1969	1969
Idaho	1963	1973	West Virginia	1965	1965
Arizona	1964	1978	Wisconsin	1965	1965
Kentucky	1964	1964	District of Columbia	1975	1975
Louisiana	1964	1972	Mississippi	1973	1973
New Jersey	1964	1972	Virginia	1973	1973
Rhode Island	1964	1971	Michigan	1967	1967
South Dakota	1964	1973	Minnesota	1975	1975
Alabama	1965	1965	Pennsylvania	1970	1970
Arkansas	1965	1975	Ohio	1970	1970
Connecticut	1965	1967	Delaware	1971	1971
Georgia	1965	1974	North Carolina	1971	1971
Illinois	1965	1973	Oregon	1971	1971
Indiana	1965	1971	Wyoming	1971	1971
Iowa	1965	1965	South Carolina	1972	1972
Kansas	1965	1972	Maryland	1973	1973
Maine	1965	1975	Massachusetts	1973	1973
Missouri	1965	1969	Nebraska	1973	1973
Montana	1965	1965	New Mexico	1973	1973
Nevada	1965	1965	New York	1973	1973
New Hampshire	1965	1971	Tennessee	1973	1973
North Dakota	1965	1975	Texas	1973	1973
Oklahoma	1965	1972			

Notes: This table presents the year in which a given state has adopted a mandatory reporting law with physicians and teachers as the main reporting agents. Information about each law has been self-collected from States Session laws. States are sorted by the year in which physicians are mandated to report. Statutory codes are available upon request to the authors.