ESSAYS ON HEALTH AND ABUSE

A Dissertation

by

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ABSTRACT

This dissertation covers the topics of substance abuse, crime, health insurance, and child maltreatment.

Demand-side approaches should be strongly considered when attempting to combat America's illegal drug problems. Implementing an identification strategy that leverages variation driven by substance-abuse-treatment facility openings and closings measured at the county level, estimations show that substance-abuse-treatment facilities reduce both violent and financially motivated crimes in an area. These effects are particularly pronounced for relatively serious crimes.

There is a role the employment relationship plays in determining the provision of health benefits at the establishment level. Using restricted data from the Medical Expenditure Panel Survey-Insurance Component (MEPS-IC) and the Coarsened Exact Matching technique, the analysis extends previous studies by testing the relationships between premium costs, employment relationships, and the provision of health benefits between 1999 and 2012. Both establishment- and state-level union densities increase the likelihood of employers providing health plans, while right-to-work legislation depresses the provision. Furthermore, state-level union density reduces the adverse impact of premium costs. These results indicate that the declining provision of health benefits is in part driven by the transformation of the employment relationship in the United States and that labor unions remain a critical force in sustaining employment sponsored healthcare coverage over the past two decades.

Child maltreatment is vastly underreported. School attendance takes children out of the home and places them under the supervision of educators trained to notice symptoms of abuse and neglect. Using state-age level enrollment data, estimations show that school attendance increases maltreatment reporting by 77%. This increase stems solely from reports initiated by educators with no evidence that these reports crowd out reporting from other entities. The effect is not caused by seasonal maltreatment nor hypersensitivity in reporting from educators.

DEDICATION

To Hannah

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NOMENCLATURE

ACS	American Community Survey
CBP	County Business Patterns
CM	Child Maltreatment
CPS	Child Protective Services
ECS	Education Commission of the States
HCUP	Healthcare Cost and Utilization Project
ICD-9-CM	International Classification of Diseases, Ninth Revision, Clinical Modification
IPUMS	Integrated Public Use Microdata Series
KID	Kids' Inpatient Database
N-SSATS	National Survey of Substance Abuse Treatment Services
NCANDS	Nationa Child Abuse and Neglect Data System
NCHS	National Center for Health Statistics
RCT	Randomized Control Trials
SAT	Substance Abuse Treatment
SEER	Surveillance Epidemiology and End Results
UCR	Uniform Crime Reports

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1. INTRODUCTION

Traditionally the branch of health economics studies aspects of the healthcare system or the behaviors of individuals that affect their health. Recently, using the broadest possible definition, health economics has covered a multitude of different topics including human capital development, child welfare, moral hazard, violent crimes and more. Even with the vast subject areas under a loosely defined scope of health economics, all studies investigate societal problems and bring forth a meaningful and actionable solutions. The three chapters in this dissertation consist of three distinct studies under a broad definition of health economics.

In Chapter 2 of this dissertation the problem presented deals with America's drug epidemic and how we should go about alleviating the many known issues that come along with it. Over the past few decades, the U.S. government's drug control policy has been focused on supply-side interventions such as interdiction, eradication, source-country control, and law enforcement. This chapter looks into a demand-side intervention coming in the form of substance abuse treatment, more specifically substance abuse treatment facilities at the county level. We link these facilities to agency level crime data from the FBIs Uniform Crime Report. We show that treatment facilities built in the previous year reduce violent crimes as well as financially motivated crimes in the current year. We further show that the benefit of the facilities far outweigh the costs associated with building and maintaining them.

With the current national attention being placed on how we should go about implementing an all-inclusive health insurance system, it is vital that we understand the main components that make up the deciding factors of where and how individuals receive their health insurance plans. Chapter 3 looks into the employment relationship as a determining factor for employer sponsored health insurance using the Medical Expenditures Panel Survey Insurance Component provided by the U.S. Census Bureau. With nearly half of Americans being covered by employment-based health insurance and a large portion of the workforce being under union contracts, it is critical that policy makers take into account the role employment relationships play in determining employer sponsored health insurance.

The last chapter investigates the problem of underreporting for child maltreatment incidents. Official rates of maltreatment only capture around 1 in 10 victims. The reason for this vast level of underreporting is that children can only be reported for maltreatment if they are visible to potential reporters. School attendance places children out of the home and under the supervision of educators that are mandatory reporters of child maltreatment and are trained to notice symptoms of abuse and neglect. Do to this large increase in public visibility I show that maltreatment reporting greatly increases whenever children begin to attend school. This chapter shows that if a society wants to increase their level of maltreatment reporting they need to focus on policies that will increase maltreated children's visibility to potential reports.

2. SUBSTANCE ABUSE TREATMENT CENTERS AND LOCAL CRIME*

2.1 Introduction

Drug-induced deaths in the United States have increased 280 percent since 1999 and now represent the largest major category of external causes of death by a wide margin: there were 47,055 deaths due to drug overdoses in 2014 compared to 32,675 due to motor vehicle accidents.[1] These facts underscore a growing need to understand how to reduce drug-related harms. Towards this end, a large body of work has shown that policies targeting the supply of illicit drugs are rarely effective.[2] In contrast, recent work indicates that expanding access to substance-abuse-treatment (SAT) facilities significantly reduces severe drug abuse, as measured by drug-induced mortality [11]. While this evidence highlights that investments in SAT can improve outcomes for some individuals, it does not necessarily reflect a broad-based benefit for communities that might be considering making such investments. In this paper we fill this important gap in the literature by estimating the effects of SAT facilities on homicide rates, which are especially high in urban areas, other violent crimes, and property crimes.[3]

There are several mechanisms through which SAT facilities may affect local crime. As outlined in Goldstein's [12] influential tripartite conceptual framework for the drugs-violence nexus, drugs may affect violence through psychopharmacological effects, economically compulsive effects, and systemic effects. In these terms, SAT could be expected to reduce violence by: (i) reducing the use of drugs that lead to aggressive behavior (though there may be some offsetting effects caused by withdrawal), (ii) by reducing conflicts associated with financially motivated crimes committed by addicts seeking funds to buy drugs, and (iii) by reducing violence among and against those

*Reprinted with permission from "Substance Abuse Treatment Centers and Local Crime" by Samuel R. Bondurant, Jason M. Lindo, and Isaac D. Swensen. Journal of Urban Economics, Volume 104, Pages 124–133, March 2018 by Elsevier.

[1]See [1] and [2]

[2]See for instance [3], [4], [5], [6], [7], [8], [9], and [10].

[3]In 2012, the homicide rate was 7.4 per 100,000 in central metropolitan counties compared to 4.1 per 100,000 in other counties. These statistics are based on the Uniform Crime Reports data described in detail in Section 2.3.

associated with the drug trade.[4] Moreover, drug-abuse treatment may reduce gun carrying through all three of these mechanisms, which could serve to reduce the amount—and intensity—of violence in communities. It is also important to keep in mind that a relatively large share of drug users have mental health problems that contribute to their addiction and to violent behaviors [17, 18]. As such, we could expect SAT to reduce violence because it can itself include—or can direct patients towards—treatment for underlying mental health problems that contribute to violence [17, 19]. Finally, SAT treatment may reduce criminal activity through positive spillover effects on friends and family members of those receiving treatment.

Although these mechanisms highlight how SAT facilities can reduce crime through their effect on drug abuse, there are other mechanisms through which we might expect SAT facilities to *increase* local crime. Featuring prominently in not-in-my-backyard arguments against SAT facilities is the notion that such facilities pose risks by drawing into the area individuals who have relatively high rates of crime perpetration (drug users). Going beyond the idea of shifting crime perpetration from one place to another, SAT facilities could increase crime by altering the social and environmental context faced by drug users. That is, by altering the types of people and places that they encounter and with which they interact.

In this study we contribute to this policy debate by quantifying the effects of SAT facilities on crime. Specifically, we use annual county-level data on the number of SAT facilities to evaluate the degree to which crime rates change when SAT facilities open and close. We consider various crime outcomes measured over time at the county and law-enforcement agency level, based on data from the National Center for Health Statistics and the FBI's Uniform Crime Reporting Program. These panel data allow us to include a rich set of fixed effects (county/agency and state-by-year) and control variables (demographics, various measures of economic conditions, and law enforcement presence) in our models, so the estimates are identified based on plausibly exogenous variation. Several ancillary analyses support the validity of this research design, including analy-

[4] Prior studies have documented causal effects of drug activity on community violence by exploiting variation in drug use induced by price shocks [13, 14] and by exploiting variation in the timing with which specific drugs became available across different cities [15, 16].

ses that demonstrate that outcomes in an area change after but not before the number of facilities change.

Our approach shifts the focus from the effects of SAT on those who receive treatment to the effects of SAT facilities on the communities they serve. This allows us to make several contributions. First, we consider outcomes that tend to be beyond the scope of randomized control trials (RCTs), which are limited by small samples, short follow-up periods, and the potential for false reporting. In particular, our approach allows us to consider severe-but-infrequent outcomes (e.g., homicide) and behaviors that individuals are likely to conceal (e.g., sexual assault). Second, our estimates reflect the effects of SAT on patients and the spillover effects onto the broader community, inclusive of any spillover effects on nearby friends and family and on the market for illegal drugs. In so doing, our estimates will allow for more comprehensive cost-benefit considerations. Third, whereas the nature of RCTs tends to require the use of small localized samples, which may have limited external validity, our use of administrative data allows us to obtain estimates that reflect the effects of SAT facilities across the United States.

Our analysis reveals significant and robust evidence that expanding access to SAT through additional treatment facilities reduces local crime. The effects appear to be particularly pronounced for relatively serious violent and financially motivated crimes: homicides, aggravated assaults, robbery, and motor vehicle theft. We do not find significant effects on more frequent but less serious crimes (simple assault, burglary, and larceny), nor do we find a significant effect on sexual assault. We show that the estimated effect on homicides is present across two different sources of homicide data and that they are concentrated in highly populated areas.[5]

Despite the various contributions of our research described above, there are some limitations that bear noting. First, our empirical approach, which focuses on county- and law-enforcement-agency-level aggregates, implies that we cannot separate the effects of SAT facilities on those who receive treatment from the effects of SAT facilities on the broader community. Our use of

[5] In an earlier version of this study [20], we updated Swensen [11] analysis and showed that the impacts on drug abuse—as measured by drug-induced mortality—are readily apparent in an analysis that uses the same years of data as our analysis of crime. These results indicate a 0.50 percent decline in drug-induced mortality rates associated with an additional SAT facility in a county, a bit larger than the estimated effect of 0.42 percent reported in [11].

aggregate data also implies that we cannot separately identify effects for areas in a county that are nearer versus farther from a SAT facility. That said, we view these as a reasonable tradeoffs in order to be able to speak to the effects on the community as a whole. Second, while there is significant variation across SAT facilities in the types of treatment that they offer and in the number of patients they can treat, our estimates will reflect an average of the effects of these facilities. Finally, openings and closings of SAT facilities are not random. While this has the potential to compromise our ability to identify causal effects, our ancillary analyses, which are discussed in detail in subsequent sections, demonstrate that it is unlikely in light of our empirical strategy.

2.2 Background

2.2.1 Substance Abuse and Treatment

According to the National Survey of Drug Use and Health over 21.5 million people in the U.S. are classified as having a substance-use disorder [21].[6] A high incidence of substance abuse is also apparent in crime perpetration, with 40 percent of convicted violent criminals being under the influence of alcohol and nearly 60 percent of all arrestees testing positive for some illicit substance at the time of arrest.[7] The annual societal costs of drug abuse solely in terms of drug-related crime are estimated at over 56 billion dollars.[8]

Though substance-abuse treatment is a promising avenue to reduce these costs, treatment rates for those in need remain very low. In 2014, 85 percent of those abusing or dependent on an illicit substance did not receive treatment and 91 percent of those abusing or dependent upon alcohol did not receive treatment. Moreover, despite the prevalence of alcohol and drugs among arrestees, 70 percent of arrestees have never been in any form of drug or alcohol treatment [22]. Notably, recent changes brought about by the Affordable Care Act are expected to increase coverage and take-up of treatment [23, 24].

In this context, the number of substance-abuse treatment facilities may be a particularly rel-

[6] Based on criteria specified in the Diagnostic and Statistical Manual of Mental Disorders, 4th edition (DSM-IV).

[7] See https://ncadd.org/about-addiction/alcohol-drugs-and-crime.

[8] Estimates based on the 2011 National Drug Threat Assessment counducted by the National Drug Intelligence Center.

evant policy parameter. In the United States, over 14,500 stand-alone treatment facilities are the primary setting for delivery of substance-abuse treatment, offering a wide range of drug-treatment programs and related services [25]. Local treatment centers most commonly offer outpatient care to deliver treatment programs such as detoxification, methadone maintenance, regular outpatient, adolescent outpatient, and drug-court programs [25]. For more serious substance-abuse problems, facilities provide residential treatment in which clients temporarily live at the treatment site (e.g. inpatient detoxification, chemical dependency programs, therapeutic communities). While treatment programs vary substantially and often target particular demographic groups or specific drug addictions, all treatment approaches share similar goals to mitigate the consequences of drug abuse and encourage healthier lifestyles. According to the National Survey of Drug Use and Health (2015), 62 percent of individuals undergoing treatment reported receiving treatment for alcohol, 21 percent reported receiving treatment for marijuana, 18 percent reported receiving treatment for pain relievers, 14 percent reported receiving treatment for cocaine, 13 percent reported receiving treatment for heroin, and 11 percent reported receiving treatment for stimulants such as methamphetamines.

More broadly, the substance-abuse treatment industry includes profit, non-profit, and public providers, the bulk of which (87 percent) are privately-owned facilities.[9] Though the objective functions of facilities may differ somewhat by ownership status and treatment focus, the decision to open or close a treatment facility likely depends crucially on (i) a perceived need for treatment providers or opportunities to improve upon currently offered treatment services and (ii) the ability to secure funding for treatment services from either public or private third-party payers [26]. Given the high need for addiction treatment and existing evidence of binding treatment capacity constraints and long wait lists, the availability of funds is particularly relevant when considering the predictors of facility openings and closings.[10]

[9] According to the 2013 National Survey of Substance Abuse Treatment Services, 60 percent of facilities are non-profit, 30 percent are for profit, and 10 percent are public.

[10] Evidence suggests that capacity concerns and being put on a wait list are important barriers to treatment enrollment [27, 28, 29]. Relatedly, [30] analyze the effect of state legislation that reduces out-of pocket costs for mental health and substance-abuse treatment and find a relatively small effect on treatment admissions. They argue that the effect on admissions is muted, in part, because of treatment capacity constraints suggested by limited growth in the number of treatment facilities and increasing treatment waiting periods.

Unlike general health care, which relies on funding through insurance mechanisms, substance-abuse treatment relies primarily on public funding in the form of federal block grants and state subsidies. That said, recent mental health parity legislation and the rise of managed-care contracts have increased the importance of public and private insurance revenue to providers [31, 32]. Assuming these sources of financing generally increase with drug abuse and related problems, analyses of the effect of treatment provision on drug-related outcomes may understate the actual effect of treatment.

2.2.2 Related Literature on SAT and Crime

An extensive literature has evaluated the relationship between substance-abuse treatment programs, drug related outcomes, and criminal activities, including some that use "the gold standard" for empirical research, randomized control trials (RCTs). In a widely-cited meta analysis, Prendergast et al. [33] reviewed 78 studies of SAT, 60 percent of which used random or quasi-random assignment to treatment and 25 of which examined crime outcomes. They report that "drug abuse treatment has both a statistically significant and a clinically meaningful effect in reducing drug use and crime, and that these effects are unlikely to be due to publication bias." The estimates indicate an average 13 percent decline in criminal involvement as a result of treatment.[11] More recent reviews of specific treatment approaches provide consistent evidence that criminal involvement declines during treatment and mixed evidence when considering longer-run crime outcomes [34, 35].

The existing literature also adds insight into the efficacy of specific treatment settings in reducing drug-related crime. Some of the more convincing and consistent evidence comes from studies evaluating prison-based drug treatment. This is partly due to the relative ease of employing a randomized treatment design and the ability to consider recidivism rates rather than relying on self-reported criminal activity.[12] Summarizing the literature, Mitchel et al. [38] review 74 studies of

[11]Crime outcomes included self-reported crimes and official records on arrest, conviction and incarceration. As such, this review includes evidence from crime outcomes during and after treatment.

[12]Treatment rates increased by 34 percent among state inmates and 90 percent among federal inmates from 1997-2004 [36]. Core funding for these increases has come from the federal government through the Residential Substance Abuse Treatment (RSAT) initiative and funding for drug courts through the Bureau of Justice Administration [37].

prison-based treatment programs and conclude that substance-abuse treatment for inmates reduces recidivism by 15 percent. Existing evidence also suggests that court-mandated treatment programs, which account for a third of all treatment admissions, can be effective in reducing crime.[13] For instance, Wilson, Mitchell, and Mackenzie [39] identify and review 55 quasi-experimental and experimental evaluations of drug courts. They concluded that court-referred treatment does lower re-arrest rates though the estimated effects were notably smaller and less precise among evaluations that employed randomization. They also find consistent evidence of declines in re-offending both during and following court-referred treatment programs, however the estimated effects do decay over time.

Together, this literature provides consistent evidence that treatment programs can reduce crime. While these studies have made significant contributions to our knowledge, the merit of our study is predicated on the notion that some of the most important questions about the effects of SAT are only likely to be answered using alternative methods applied to observational data. In particular, our study shifts the focus from the effects of SAT on those who receive treatment to the effects of SAT facilities on the communities they serve and uses data that allow us to obtain estimates that reflect the effects of SAT facilities on local-area crime across the United States.

To our knowledge only one other recent working paper attempts to consider the effects of SAT on crime in such a comprehensive fashion. Wen, Hockenberry, and Cummings [40] consider the effects of changes in SAT rates on property and violent crimes using data collected by the FBI that span the United States. Their instrumental variables approach relies on the assumption that state health insurance expansions (made possible through Health Insurance Flexibility and Accountability waivers) only relate to changes in crime through their impacts on SAT.[14] This assumption could be violated if, for example, expanding access to health insurance affects crime through its impact on treatment for mental health problems or through its impacts on overall health and well

[13] See [25] for a breakdown of admissions by treatment referral source.

[14] They also use as an instrumental variable state-level mandates requiring private group health plans to provide benefits for substance-use disorder treatment that are no more restrictive than the benefits for medical insurance parity mandates; however, it is always used in conjunction with the waiver expansion instrument, presumably due to a lack of independent power.

being. As all observational studies rely on fundamentally untestable assumptions, and as any body of evidence is more compelling when similar results are documented using approaches that rely on different assumptions, we view our work as an important contribution that complements this prior study, which reports that increases in substance-use-disorder treatment significantly reduces robbery, aggravated assault, and larceny.

2.3 Data

Following Swensen [11], we identify county-level changes in the number of substance-abuse treatment facilities using data from the U.S. Census Bureau's County Business Patterns (CBP). The CBP data reports the annual number of substance-abuse treatment clinics (a single physical location) in each U.S. county for both outpatient and residential facilities from 1999-2012.[15] Although classified separately in the CBP data, residential and outpatient establishments often offer both residential and outpatient treatment services with 90 percent of all admissions occurring in an outpatient setting [25]. Therefore, estimating the effects separately for outpatient and residential facilities would not be informative as residential and outpatient services are not distinctly identified. As such, we combine outpatient and residential classifications using the total count of establishments as an indicator for county-level provision of substance-abuse treatment.

To estimate the effect of treatment facilities on local-area crime we merge CBP data with several independent data sources for criminal activity. We use two datasets to investigate impacts on homicides, one of which we also use to investigate a wide variety of crimes. First, we use annual county-level mortality data from the National Center for Health Statistics (NCHS) Multiple Cause of Death Data to analyze homicides.[16] We combine these data with county-year population counts from the National Cancer Institutes's Surveillance Epidemiology and End Results (Cancer-SEER) program to construct mortality *rates*. We also use these population data to create county-by-year controls for demographic characteristics.[17]

[15] The following six-digit NAICS codes identify treatment establishments: 621420 —"Outpatient mental health and substance abuse centers" and 623220—"Residential mental health and substance abuse facilities."

[16] NCHS homicides include deaths by another person with the intent to injure or kill. They do not include homicides due to legal intervention, operations of war, or homicides from the Sept. 11, 2001 attacks.

[17] As reported by [41], the Cancer-SEER population data are more accurate than data interpolated from the Census

Our second source of crime data is based on the Uniform Crime Reports (UCR), which are a compilation of crime statistics reported by local law-enforcement agencies across the United States to the FBI. Specifically, we use the offenses known data from the Offenses Known and Cleared by Arrests UCR segment. These data, which we will refer to as UCR Offenses Known, include the most commonly reported violent and property crimes including criminal homicide, sexual assault, robbery, assault, burglary, larceny theft, and motor vehicle theft. We focus on known offenses in order to capture crimes that come to the attention of law enforcement, as opposed to alternative data sets that are available but are restricted to crimes that have been cleared by arrest. We use these data in conjunction with the UCR's estimates of the population covered by an agency in a given year to construct annual *agency*-level crime rates. We restrict our UCR sample to agencies that cover a single county and agency-years in which agencies are reporting the full 12 months of crime to the UCR program. We link the UCR agency-level data with county-level CBP data using the primary county in which each municipality resides and calculate crime rates using the annual reported population covered by each municipal agency.[18]

We restrict our analysis to U.S. counties with at least one treatment facility over the 1999-2012 time period and counties with available identifiers in the 48 contiguous states.[19] The resulting data include treatment facility, mortality, and crime data in 48 states, spanning 14 years. In Table C.2 we present summary statistics for our sample, weighted by the relevant populations. CBP data indicate that counties have a population-weighted average of 49.5 SAT facilities. Importantly, there is substantial variation in the number of facilities with the average county experiencing 5.8 net facility openings and 3.7 net closings from 1999 to 2012, where a net opening is an observed increase in the number of facilities from one year to the next and a net closing is defined similarly.

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because they "are based on an algorithm that incorporates information from Vital statistics, IRS migration files, and the Social Security database."

[18]The UCR Offenses Known data used in this study were collected and compiled by the Inter-University Consortium for Political and Social Research (ICPSR).

[19]Specifically, we drop all counties in HI and AK and combine counties that experience boundary changes over time. This involves combining Adams, Broomfield, Boulder, Jefferson, and Weld in Colorado; Prince George's and Montgomery in Maryland; Gallatin and Yellowstone National Park in Montana; Craven and Carteret in North Carolina; Alleghany and Clifton Forge in Virginia; Augusta and Waynesboro in Virginia; Bedford and Bedfort City in Virginia; Halifax and South Boston City in Virginia; Prince William and Manassas Park in Virginia; Southampton and Franklin in Virginia; and York and Newport News in Virginia.

For reference, Table C.2 also shows summary statistics for each mortality and crime outcome used in our analysis. Summary statistics for the control variables we use in our analysis are shown in Table A.7.

2.4 Empirical Approach

We identify the effects of SAT facilities using year-to-year variation within counties driven by facility openings and closings, controlling for state-by-year shocks common to areas within a state in addition to time-varying county characteristics. As we analyze both county and agency-level outcomes, we operationalize this strategy using a regression model that includes either county or agency fixed effects in addition to state-by-year fixed effects and county-year covariates:

$$y_{ast} = \theta Facilities_{cs,t-1} + \alpha_{as} + \alpha_{st} + \beta X_{cst} + \epsilon_{ast},$$

where y_{ast} represents outcomes in area a (either county or agency) in state s in year t. We use log rates to measure crime outcomes. We add one to all outcome counts before constructing log rates to avoid dropping area-year observations for which the outcome would otherwise be undefined, but we show that results of all of our analyses are similar if we instead simply focus on areas that always have a positive count, with the sample being defined separately for each outcome considered. We also show that results are similar using an inverse hyperbolic sine transformation instead of adding one before taking the log of counts. In support of using a log transformation, we have verified that Poisson models (where computationally feasible) yield very similar estimates. $Facilities_{cs,t-1}$ represents the number of SAT facilities in county c in state s in year t-1, α_{as} are area fixed effects, α_{st} are state-by-year fixed effects, and X_{cst} includes county unemployment rates, the number of firm births, number of law enforcement officers per 100,000, and the fraction of the county population that is: white, black, male, less than 10 years old, 10-19 years old, ...

, 60-69 years old.[20,21] Finally, ϵ_{ast} is a random error term that we allow to be correlated within a county across years, and across all counties in any given year by estimating two-way standard errors following Cameron et al. (2011).[22] To be clear, our measure of facilities is a county-level measure even when we are considering crimes at the agency level. We also note that our main results are based on regressions that weight by the relevant population size in order to improve efficiency though we subsequently explore unweighted estimates.

Our focus on within-area variation accounts for fixed characteristics of areas (both observable and unobservable) that may be correlated with the number of SAT facilities in a county and with our outcomes of interest. For example, this approach will address the fact that there are inherent differences between urban and rural counties. The inclusion of state-by-year fixed effects account for aggregate time-varying shocks, such as aggregate economic conditions or changes in the national drug-control strategy.[23] They also control for state-specific shocks such as changes in state funding for law enforcement services. The controls for unemployment rates and firm births account for the possibility that our outcomes of interest and treatment facilities may both be related to local economic conditions. The controls for demographics account for the possibility that compositional changes in a county's population may affect outcomes and investments in SAT facilities.

Our empirical approach closely follows Swensen [11], who also conducts several ancillary

[20]County unemployment rates are from the BLS Local Area Unemployment Statistics. Firm births are the number of firms reporting positive employment for the first time, as reported by the U.S. Census Statistics of U.S. Businesses. The number of law-enforcement officers per 100,000 residents are calculated using the UCR agency-specific employment reports available in the Law Enforcement Officers Killed and Assaulted (LEOKA) database. For our county level analysis, we use aggregated agency-level data.

[21]Our choice to use the number of facilities in the prior year as opposed to the number per capita is supported by an ancillary analysis of drug-induced mortality. Drug-induced mortality clearly responds to the number of facilities (as previously shown in [11]), less so to facilities per capita. This is likely to be in part explained by larger areas tending to have larger facilities. In any case, this finding supports the idea that the number of facilities is a stronger predictor of utilization than a per-capita measure. We have also investigated models that include the number of facilities squared but never find its corresponding parameter estimate to be statistically significant, whether evaluating drug-induced mortality or crime outcomes. This suggests that any diminishing returns that may exist are not large enough to be detected using our data and identification strategy.

[22]That is, we estimate two-way standard errors clustered on counties and years. This approach yields more conservative estimates than estimates that solely cluster on counties, reflecting that there are unobserved shocks to outcomes that span counties.

[23]For instance, state-by-year fixed effects control for nationwide effects of the substantial increases in federal funding for substance-abuse treatment services for inmates through the Residential Substance Abuse Treatment (RSAT) initiative and funding for drug courts through the Bureau of Justice Administration.

analyses in support of the validity of the research design for estimating effects on drug-induced mortality. In particular, Swensen demonstrates that additional facilities lead to increases in treatment admissions and that the effects of additional facilities are greatest for causes of death that are most closely related to drug abuse.[24] To address concerns regarding reverse causality, Swensen plots drug-induced mortality rates leading up to and following changes in the number of facilities and finds no visual evidence of changes in drug-related mortality prior to changes in the number of facilities. Furthermore, his estimates from models that consider additional lags and leads of treatment facilities show that the previous- and current-year changes in the number of facilities is significantly related to drug-induced mortality, but that drug-induced mortality is not related to the number of facilities in future periods.[25] In a similar fashion, we estimate a version of Eq. (1) that also considers the effect of the number of facilities in the current, previous and subsequent years on the outcomes that are the focus of this paper. The results of this analysis, discussed in more detail below, indicate that changes in the number of treatment facilities are also not driven by recent changes in crime.

We note that a third of all treatment admissions are court-ordered, often as an alternative to incarceration. This is potentially important because links from increased crime to increased incarceration to increased SAT facilities could cause our empirical strategy to understate the reductions in crime generated by SAT facilities. Alternatively, links from increased incarceration to reduced crime (through incapacitation effects) and to SAT facilities could cause our empirical strategy to overstate the reductions in crime generated by increased SAT facilities. While we cannot rule out either of these possibilities, we note that any such changes would have to be happening differentially across counties within the same states to generate bias (because we control for state-by-year

[24] Swensen uses data on admissions into facilities receiving public funding to offer "proof of concept" that increases in treatment facilities leads to a change in an underlying factor associated with treatment. Notably, other mechanisms—including perceptions toward treatment or factors influencing the quality and accessibility treatment—may also contribute to declines in substance abuse as treatment services expand.

[25] Swensen also estimates models using demand-side characteristics to predict treatment facility openings in order to offer insight into the degree to which treatment provision responds to changes in the demand for addictive substances. His results suggest that the number of treatment facilities varies directly with measures that proxy for the demand for addictive substances, he argues that not adequately accounting for these correlations would understate the effect of an additional treatment facility on drug-related mortality.

fixed effects). We also note that persistent shocks generating these sorts of relationships would be expected to generate significant links between current crime rates and future levels of SAT. Reassuringly, we do not find evidence of any such links.

2.5 Estimated Effects on Crime

2.5.1 Homicides

Before turning to estimates that are based on Uniform Crime Reports data, we begin with an analysis of homicide deaths recorded in NCHS mortality data. Though these also include justified homicides, 94 percent are unjustified criminal homicides and, as such, they can shed light on the degree to which treatment interventions affect the most serious and costly form of criminal activity.[26] The results of this analysis, shown in the first panel of Table A.2, provide causal evidence that county-level homicide rates are reduced by SAT facilities. Specifically, the estimates indicate a 0.25 percent decline in intentional homicide death rates associated with an additional SAT facility. These estimates are similar across specifications with limited control variables and richer sets of control variables.

In the second panel of Table A.2 we investigate the effects on homicide rates using law-enforcement-agency-level data from the UCR's Offenses Known database. We estimate similar models when using these data, just modifying them to reflect that they are agency-year data by using agency fixed effects instead of county fixed effects and using agency covered population as the denominator to construct homicide rates. Analyses of these data continue to indicate that SAT facilities significantly reduce homicides in areas covered by municipal law-enforcement agencies, though the estimates are somewhat smaller, indicating a 0.16 percent decline in intentional homicide death rates associated with an additional SAT facility.[27]

[26]For a breakdown of justified and unjustified homicides in 2013, see https://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2013/crime-in-the-u.s.-2013/offenses-known-to-law-enforcement/expanded-homicide.

[27]In an earlier version of this study Bondurant et al. [20], we also investigated the impacts on homicides using the UCR's Supplementary Homicide Reports (SHR) database, an incident-level dataset that includes detailed information on each homicide as voluntarily reported by agencies participating in the UCR program. The results of this the analysis indicated that effects of SAT facilities on homicides are concentrated among homicide incidents in which the relationship to the offender was unknown or in which the offender was a friend.

2.5.2 Violent Crimes More Broadly

Having established that SAT facilities reduce the most costly of crimes (homicides), we next consider the degree to which treatment facilities affect other types of violent crime. In Table A.3 we show a detailed breakdown of the effects of SAT facilities on violent crimes based on analyses of the UCR Offenses Known data.[28] While we focus our discussion below on the point estimates from models with the richest set of controls (Column 5), we note that the estimated effects are similar across specifications once state-by-year fixed effects and demographic controls are included as covariates. The estimates are not sensitive to the inclusion of other county-year control variables.

Across the first four panels of Table A.3, we sequentially report the estimated effects on violent crimes of decreasing severity according to social cost estimates reported in McCollister et al. [43]: homicides ($9,881,198 per incident), sexual assault ($264,854), aggravated assault ($117,722), and simple assault.[29] We defer our consideration of robbery until the next section where we focus on financially motivated crimes. As mentioned above, the estimated effect on homicides indicates a significant reduction caused by SAT facilities. While the point estimate for the effect on sexual assault is also negative, suggesting that SAT facilities reduce sexual assault as well, it is not close to being statistically significant at conventional levels. The estimated effect on aggravated assaults also suggests a reduction in crime associated with SAT facilities, though this estimate is only marginally statistically significant. Finally, the estimates suggest no effect on simple assaults.

2.5.3 Financially Motivated Crimes

Table A.4 shows the estimated effects on financially motivated crimes. We again sequentially report the estimated effects on crimes of decreasing severity according to social cost estimates: robbery ($46,541), motor vehicle theft ($11,849), burglary ($7,108), and larceny ($3,885). As with the estimated effects on violent crimes, these estimates suggest more pronounced effects of

[28] In most cases each outcome represents a distinct incident as 85 percent of UCR incidents are single-offense incidents, where an incident is a distinct time, place, victim (for crimes against the person), and offender. In cases of multiple-offense incidents, agencies are instructed to report the most severe offense according to the UCR hierarchy rule [42].

[29] Note that we have adjusted the cost estimates for inflation to put the amounts in 2016 dollars. MCollister et al. [43] do not include estimates for simple assault.

SAT facilities on relatively serious crimes. The point estimates indicate that a SAT facility reduces robbery by 0.11 percent, motor vehicle theft by 0.12 percent, burglary by 0.05 percent, and larceny by 0.06 percent. The estimated effects larceny are not statistically significant at conventional levels.

2.5.4 Assessing Endogeneity and Lag Structure

As discussed in Section 2.4, the main threat to the validity of our empirical strategy is the possibility that changes in the number of facilities in an area might be driven by trends in the outcomes we consider (or the correlates thereof) and/or recent shocks to the outcomes we consider (or the correlates thereof). To the degree to which such trends and/or shocks occur at the state level or relate to changing demographics, economic conditions, or the size of police forces, they should be captured by state-year fixed effects and the control variables included in our analysis. As this is fundamentally untestable, we propose a test of the validity of our identification strategy based on examining the lead and lag structure of the estimated effects. Specifically, we estimate versions of Eq. (1) that consider the link between our outcome variables and the number of SAT facilities in a county *in a future year*.

We also expand on Eq. (1) to consider contemporaneous versus lagged measures of SAT facilities. We do so in order to evaluate our choice to focus on the number of facilities in the prior year as our main variable of interest, a choice we made to avoid attenuation bias that would likely be caused by the fact that newly opened (or closed) facilities would only affect counties for some fraction of the year.

Table A.5 shows estimates of this type for all of the outcomes considered across tables 2 through 4. Specifically, it shows estimates based on our richest model while additionally considering the number of facilities in the current year and in the future year. Across the 9 outcomes we consider, the estimated effects of the number of facilities one year in the future are *never* statistically significant, even at the ten-percent level. We interpret these results as evidence that reverse causality, or the possibility that changes in the number of SAT facilities may be driven by recent changes in drug abuse and related outcomes, is not a major concern. As such, these results provide support for a causal interpretation of our main results.

These results also provide support for our focus on the lagged measure of facilities. In particular, where we see significant effects on outcomes, the number of treatment facilities in the prior year has a stronger effect than the number of treatment facilities in a given year in all but one instance. Moreover, the estimated effect of the number of treatment facilities in the current year is usually not statistically significant, suggesting that the effects are more likely to be driven by successful treatment as opposed to incapacitation effects.[30]

2.5.5 Alternative Empirical Approaches

As an additional test of the robustness of our estimates, in Panel A of Table A.6 we show the estimated effects for each outcome based on the subset of areas for which the log outcome rate can be defined in each year without adding one.[31] For nearly all of the outcomes we consider, these estimates are virtually the same as our main results in both statistical and economic significance.

Panel B of Table A.6 shows estimates that transform crime counts using the inverse hyperbolic sine function as an alternative to adding one before taking the log.[32] For all of the outcomes we consider, this approach yields estimates that are very similar or larger in magnitude to our main results.

Panel C of Table A.6 shows estimates that do not use population weights. Notably, the estimated effects on homicide are smaller in magnitude and no longer statistically significant at the five-percent level in these unweighted results. As discussed in Solon et al. [44], differences between weighted and unweighted estimates can reflect heterogeneity in the effects across high-weight and low-weight observations (or high-population and low-population areas in our case). To gain greater insight into such heterogeneity, we examine how our (weighted) estimates vary as we exclude the largest 5 areas, the largest 10 areas, and so on.[33] The results of this analysis are

[30] Further results along these lines are presented in Tables A.8 through A.10. In these tables, we explore models that consider alternative lag and lead specifications including an additional lead indicator. The results of these analyses lead to the same conclusions as in Table A.5.

[31] As such, the set of areas contributing to the estimates varies across outcomes, with fewer areas contributing to the estimates focusing on rarer outcomes such as homicides.

[32] Specifically, the outcome variable we use here is $ln(\frac{count+\sqrt{count^2+1}}{population})$.

[33] Recall that we are focusing on counties in our analysis based on death records and police agencies in our analyses based on offenses known to police.

shown in Table A.11. Consistent with there being significant effects on homicides for populous areas but not for smaller areas, the estimates do eventually shrink to zero as we exclude more and more large areas. However, the estimates using agency-level homicide reports, which offer a lot more precision than the estimates based on county-level death records, continue to be statistically significant even when we omit the largest 200 agencies. Along similar lines, in Table A.12, we show the estimated effects on homicides if we *only* use the largest locations. Though the statistical significance varies depending on how many agencies are included (ranging from 20 to 500) and on whether the estimates are weighted, they routinely indicate that SAT facilities reduce homicides for populous areas.

2.6 Discussion and Conclusion

In the preceding sections, we document statistically and economically significant effects of SAT facilities on several categories of crime. Our estimates of the effects on agency-level crime indicate that an additional facility in a county reduces municipal rates of homicide, aggravated assault, robbery, motor vehicle theft, and burglary; with homicide rates reduced by an average of 0.16 percent annually.[34] In conjunction with social-cost-of-crime estimates from [43], our estimates indicate that an additional SAT facility in a county reduces municipal crime costs by approximately \$475,642 per municipality.[35] Given an average of six municipal governments in each county, this suggests a decline in annual costs of county-level crime by approximately 2.85 million dollars for

[34] In an interesting contrast, Dobkin and Nicosia [8] evaluate a crackdown reducing methamphetamine consumption over an 18-month period and do not find statistically significant effects on property or violent crime. Noting that 62 percent of SAT is for alcohol—and alcohol has been causally linked to crime by a number of studies including Carpenter and Dobkin [45], Lindo et al. [46], and Anderson et al. [47], among many others—it could be that the effects that we find on crime are driven by reductions in alcohol abuse. Moreover, Dobkin and Nicosia [8] find that the intervention they evaluate substantially increased alcohol abuse, which could attenuate the effects they find on crime. Another potential explanation for why we find statistically significant effects of expanding access to SAT on crime whereas they find no statistically significant effects of methamphetamine use on crime could relate to power. In particular, the effects of methamphetamine use on crime may be too small to be detected by their identification strategy. The 95 percent confidence interval for their estimated elasticity of homicides with respect to methamphetamine consumption (as measured by hospital admissions) includes 0.078. This is not directly comparable to our estimates, but does not seem at odds with our point estimate which indicates that an additional SAT facility in the county reduces municipal homicide rates by 0.16 percent.

[35] Municipal cost calculations are based on a weighted average population of 315,030 and cost-of-crime estimates in 2016 dollars for homicides (\$9,881,198 per incident), aggravated assault (\$117,722), robbery (\$46,541), motor vehicle theft (\$11,849), and burglary (\$7,108).

each additional facility.

These estimates suggest that reductions in crime account for a sizable share of the benefits of SAT facilities. Updated estimates of the effects on county-level drug-related mortality reported in Bondurant et al. [20] indicate that an additional SAT facility reduces drug-related mortality by 0.50 percent annually. Based on a value of 7 to 8 million dollars per expected life saved, the estimate implies a decline in a county's annual drug-related mortality costs by 4.2 to 4.8 million dollars.[36,37] In total, these calculations suggest that the county-level benefits of an additional facility—in terms of drug-related mortality and criminal activity—are between 7.05 and 7.65 million dollars. Reductions in crime account for approximately 40 percent of these benefits.

To compare these benefits to the annual costs of treatment at each facility, we can consider the average number of annual treatment admissions (255) from the National Survey of Substance Abuse Treatment Services (N-SSATS), and treatment modality-specific cost estimates from French et al. [50].[38] A back-of-the-envelope calculation indicates that the annual costs of treatment for a SAT facility are approximately 1.1 million dollars.[39] These calculations suggest that the benefits of expanding treatment facilities far outweigh the associated treatment costs.

While our data do not allow us to establish a direct link between substance-abuse treatment and incidents, the results of our analyses provide support for the idea that there are broad-based benefits of SAT facilities in terms of public safety. This evidence is in contrast to not-in-my-backyard arguments that have been used to hinder attempts to expand access to SAT through additional facilities. That said, an important limitation of our research design is that it identifies effects of having an additional SAT facility *in the county*, which could mask heterogeneous effects for areas

[36] This is based on 10.9 drug-related deaths per 100,000 and an average weighted county population of 1.09 million.

[37] Kniesner [48] suggest a 7 to 8 million dollar value of a statistical life (VSL) for health and safety regulation cost-benefit analyses, which is consistent with median VSL estimates from meta analysis of existing VSL research [49].

[38] Estimates from French et al. [50] include all treatment delivery costs related to personnel, supplies and materials, contracted services, buildings and facilities, equipment, and miscellaneous items.

[39] We use the annual number of treatment admissions reported in Swensen [11] based on the 2002-2008 N-SSATS data. More recent N-SSATS data do not include treatment admissions information. To calculate the total cost of treatment at a SAT facility, we use the median of the cost bands reported for each modality in French et al. [50] weighted by the proportion of total admissions accounted for by each modality as reported in the 2013 N-SSATS reports.

in a county that are nearer versus farther from such a facility. Assessing whether such heterogeneity exists would seem to be an important avenue for future research.

3. UNION, PREMIUM COST, AND THE PROVISION OF HEALTH BENEFITS

3.1 Introduction

Employment-based health plans have been the main channel through which most Americans acquire their health care, but its coverage has been in decline in the past few decades. Between 1987 and 2017, the proportion of Americans who were covered by employment-based health plans declined from 62.1% to 49%, a difference of 13.1 percentage points consisting of over 41 million men, women, and their dependents who have access to health insurance through their own or a family members employment [51]. While the Affordable Care Act has significantly reduced the uninsured population since 2013, particularly among low-income households [52], the employment-based health plan remains the central pillar in the U.S. healthcare system. As such, the shrinkage of employer-sponsored health plans has drawn significant attention.

A main driver of the decline in employment-based health insurance is that fewer workplaces offer any health plans to their employees. Figure B.1 presents the percentage of private establishments that provided health insurance between 1999 and 2014. It shows that at the turn of the century close to 60 percent of all U.S. private workplaces provided at least one health plan. The number dwindled in the early 2000s and again in the aftermath of the Great Recession. In 2014, only 47.5 percent of establishments provided any insurance. Many of these losses were concentrated in small workplaces which experienced the greatest relative declines in offers, whereas larger workplaces tended to remain stable [53]. Furthermore, the downward trend underestimates the deterioration of employment-based health plans, as many providing employers adopt plans with more restrictive healthcare networks and higher deductibles.

The predominant explanation of this downward trend is the growth of premium costs. A recent report from the Kaiser Family Foundation [54] indicates that the cost of health insurance has been on the rise. Between 2003 and 2016, the average annual health insurance premium for family coverage doubled from $9,068 to $18,142. Furthermore, costs have often been cited as the most important reason why firms do not provide health insurance. Several other studies also indicate that a significant portion of the decline in insurance coverage could be attributed to the rise of premium costs and the tendency to not offer health plans is particularly salient among small, low-wage employers [55, 56, 57]. [1]

While these studies agree that financial incentives would increase the prevalence of employment-based insurance, field and experimental studies report that the provision of a health plan is not solely a financial matter. In the late 1980s, the Robert Wood Johnson Foundation sponsored a series of programs in nine cities to subsidize health insurance for small firms that did not offer this benefit. A mere five percent of the eligible firms decided to enroll, contradicting the prediction that lowering premium costs would lead to higher provision [59]. A similar voluntary program was conducted in two cities in New York state [60] and a randomized trial in San Diego [61], both suggesting that employers who do not offer health insurance are reluctant to do so even when 50 percent of the cost would be subsidized.

The main criticism of these findings is that the temporary nature of these programs was unattractive to many employers who did not want to offer insurance and then discontinue it when the subsidies ended. This very criticism points out the social nature of employment-based insurance. Employment-based insurance is not merely a form of compensation but signals a social commitment between employers and their employees. In addition to the rising cost, the decline of employment-based insurance may be in part driven by the transformation of the employment relationship in the United States [62, 63, 64].

This article expands the focus on financial constraints and investigates how workers collective bargaining power may shape the provision of health benefits and moderate the impact of premium costs in recent years. Instead of focusing on financial factors, we examine how the provision of employment-based health benefits is codetermined by both economic and social concerns. In the next section, we trace the history of health benefits and review existing literature on the links

[1] In addition to premium cost, there has been an extensive investigation viewing the issue through a financial lens. Abraham, Feldman, and Graven [58] find that since compensation in the form of insurance premium is either exempt or taxed at a lower rate, the preferential treatment helps to stimulate the provision of health plans for those earning higher income or residing in the states with higher tax rates.

between employment relationship and the provision of health insurance.

3.2 The Changing Landscape of Employment

Health benefits emerged as a form of compensation during the Second World War, after the Roosevelt administration instituted wage controls to curb potential inflation. This policy faced strong opposition from trade and labor unions, which had gained a strong foothold in national politics in the 1930s and threatened to organize a general strike in response. To compromise, the War Labor Board excluded health benefits from wage controls and the Internal Revenue Service granted employer-sponsored health benefits exempt status from income tax at federal, state, and city levels.

The popularity of employment-based health benefits soared as unions expanded. By 1960s, nearly all employers provided some form of health insurance. There are several reasons why the provision of health benefits rose alongside the expansion of organized labor even in the absence of wage control. First, union members tend to be older, more educated, and higher paid, suggesting that they have greater demand for health care and benefit more from the tax deduction [65]. Second, unions increase workers bargaining capacities through the expression of collective preferences and the threat of strike, both ensuring the employer will be sensitive and responsive to workers demand [66, 67].

Several studies have indicated that unions may play a direct role in supporting employment-based health benefits. By organizing union hall meetings and training programs, union officers inform the members about positive health practices and the rights to healthcare [68]. Unions also provide a channel through which workers voice their concerns about health and related issues, rather than leaving employment to seek healthcare elsewhere [69, 70]. Furthermore, even though unionized workers tend to be better off on average, unionization provides the most benefit when it reaches formerly marginalized workers such as women, minorities, and less-educated workers, who tend to have less individual bargaining power and therefore gain the most when unionized [71]. This point becomes particularly salient as de-unionization falls upon marginalized workers first and thus limits the union gains associated with employer sponsored healthcare to more advantaged workers.

Importantly, unions may foster long-term commitment between employees and their employers. Studies have often found that union members tend to have lower turnover rates than non-unionized workforce when reporting similar or lower levels of job satisfaction [72, 73, 74]. Unionized workers also tend to participate more in workplace governance [75], and are more likely to have high levels of commitment and loyalty to their company during periods of organizational restructuring [76, 77]. With long-term commitments from union employees, an investment in the health of the workforce could be mutually beneficial. Additionally, employers anticipating long term employment relationships may find it useful to offer healthcare to attract high quality employees.

Some studies have shown that the presence of labor unions in the local labor market affects both unionized and non-unionized establishments. To compete for workers and prevent unionization in highly unionized states, non-unionized establishments are under pressure to provide similar compensation [78, 79]. Even when nonunionized establishments do not compete directly with unionized establishments, labor unions tend to set the social norms regarding employment conditions [80].

While organized labor played an important role in setting the compensation standards in the post-war era, its influence began to decline in the 1980s and the 1990s as it was challenged and undermined at multiple fronts. As unions declined and the flexible employment model became the norm, employment-based insurance, essentially an investment in workers most portable human capital, began to provide less return to the employers. Health benefits became the privilege of workers with the most collective bargaining power. Using a survey of employers conducted by the Robert Wood Johnson Foundation in 1993, an early study estimate that declining unionization could account for 20-35 percent of the decrease in the offering of health plans between 1983 and 1997 [81]. A similar conclusion is reached in a separate study using the Employer Costs for Employee Compensation survey conducted in 2004 [67].

However, recent studies have casted doubt on whether labor unions still play an important role in promoting employment conditions. New evidence suggests that strike activity by unions is no longer associated with rising wages or distribution of income towards labor [82, 83]. In addition, scholars have found zero or negative benefits associated with union certification elections and union bargaining in the United States [84, 85]. Furthermore, as unions declines, they may no longer be able to shape the employment conditions of non-unionized workplaces. The inability of unions to expand into new, non-unionized industries or states has stopped premiums from reaching the wider labor market, despite attempts by unions to diversify and adapt to new conditions. This pattern has been compounded by political weakness of unions, meaning unions are increasingly unable to sway elections or mobilize private sector workers who might organize for more generous labor market policies [71, 86]. In healthcare, the question then becomes whether unions can still influence general norms about health benefits.

This study advances the study on the provision of health benefits in two main ways. First, we update previous employer-level studies [67, 81] by testing whether labor unions remain a critical force in sustaining employment-based health insurance in the 21st century. Second, we integrate literature focusing on the rising healthcare costs with the literature on changing employment relationships. We hypothesize that, independent of the premium costs, the establishments in which workers have greater collective bargaining capacity are more likely to provide health plans. Furthermore, we hypothesize that workers collective bargaining power could moderate the adverse effects of premium costs, meaning that the employers would be less cost sensitive when their workforce is more organized.

3.3 Study Data and Method

3.3.1 Data

Our primary data source is the restrict-use Medical Expenditures Panel SurveyInsurance Component (MEPS-IC) at the Federal Research Data Centers for the years 1999–2012.[2] The MEPS-IC provides information regarding employer sponsored health insurance as well as financial and de-

[2]In 2008, the MEPS-IC switched from a retrospective to a current-year survey. Therefore, our sample does not include data for 2007.

mographic characteristics for a nationally representative sample of private establishments. Specifically, it asks whether the establishment provides any health insurance and, if so, how much the employer contributes to the premium cost.

We augment the MEPS-IC by matching the establishments to the Longitudinal Business Database (LBD) and the Business Registrar (BR) also provided by the Census Bureau. This allows us to gain additional establishment characteristics, as well as linking individual establishments to their parent firms. To assess the impacts of state-level factors, we also construct variables using the March Current Population Survey (CPS) provided by the Integrated Public Use Microdata Series [87].

3.3.2 Measures

Our outcome of interest is whether the establishment provides any health plan, including single, plus-one, or family coverage. We test the importance of workers collective bargaining capacity at both the establishment and state level. Establishment-level union density is measured as the percentage of employees that are union members. State-level union density is measured as the percentage of workers that are union members or covered by union contracts. In addition, we include a dichotomous variable indicating whether the state has right-to-work legislation.

A main challenge of our analysis is that the potential costs of provision is unobserved among workplaces that do not provide any health benefits. If unionization reduces the cost of purchasing health insurance [**?**], we would see a spurious association between unionization and the provision of health benefits if the premium cost is unaccounted for in our analysis. We address this challenge by matching providing and non-providing establishments with the Coarsened Exact Matching technique (CEM) [88, 89]. Unlike propensity score matching that groups observations with similar likelihood of receiving treatment, CEM is a nonparametric technique of preprocessing data that accounts for confounding factors but does not make linear assumptions regarding the underlying functional forms. We match the establishments with characteristics that would influence insurance costs, including total number of employees, percentage of female employees, the percentage of workers 50 years old or older, whether the parent firm has multiple establishments, year, and state.

Based on these criteria, observations are placed into the various cells in which all establishment share similar characteristics. After the matching we drop the observations for the bins that do not have at least one offering establishment and at least one non-offering establishment. This leaves around 68% of the original sample. We then impute the average health insurance cost per worker for firms that do not offer any plans using the average cost of health insurance from the matched establishments that offer health plans

In addition, our regression analysis accounts for a series of characteristics that are associated with the provision of health benefits. At the firm level, we account for the founding period of the firm, firm age, whether the firm has a multi-unit operation, non-profit status, as well as employment size. At the establishment level, we control for the average pay of employees, percentages of workers that are part-time, female, 50 years old or older, or receive low wages.[3] At the state level, we control for unemployment rates and the percentage of population living under the poverty line to account for the state-wide demand for labor. Table B.1 presents the summary statistics and description of the variables used in our analysis.

3.3.3 Analytical Strategy

We estimate the effects of employment relationship and premium cost on the provision of health insurance with a series of logistic regression models. Our main model is specified as:

$$Log\left(\frac{P(Y=1)}{1-P(Y=1)}\right) = \alpha_s + \alpha_y + \alpha_n + \beta_1 U_{i,s,y} + \beta_2 C_{i,s,y} + \beta_3 UN_{s,y} + \beta_4 R_{s,y} + X_p\beta + \epsilon_{i,s,y}$$

where Y indicates the provision of any health plan. We absorb the effects of time-constant, unobserved state characteristics with α_s and industry fixed effects with α_n. Additionally we absorb year-specific shocks such as recession with α_y. $U_{i,s,y}$ denotes the percentage of workers that are unionized and $C_{i,s,y}$ denotes the employers contribution to premium cost per worker for establishment, i, in state, s, for year, y. At the state-level, $UN_{s,y}$ denotes the union density and $R_{s,y}$ indicates

[3]The MEPS-IC adjusts the definition of low-wage workers across different survey years. In general, employees who receive at or below the 25^{th} percentile for all hourly wages in the US are classified as low-wage workers. In 1999 the cutoff was set at $6.50 and increased to $11.50 for 2012.

whether there is right-to-work legislation in state, s, and year, y. X_p includes all the control variables described above. The coefficients of interest are β_1, β_2, β_3, and β_4. We expect establishment and state-level union density to be positively associated with the provision of health plans, while premium cost and right-to-work legislation have adverse effects.

To test how employment relationships could moderate the adverse effect of premium cost, we sequentially add an interaction term between the premium cost and the three variables associated with employment relationship. We expect establishment- and state-level union densities to reduce the adverse effect of premium cost, and the right-to-work legislation to intensify employers sensitivity to cost. All our estimates below are weighted using sample weights provided by the MEPS-IC which are adjusted for non-response and post stratification.

3.4 Study Results

Table B.2 presents the coefficients and standard errors from our main models. Model (1) includes all variables except the cost per worker. It shows that establishments with higher-levels of unionized workers are more likely to provide health plans. Furthermore, the results indicate that the decision to provide is embedded in a wider context. The union density at the state level is positively associated, while right-to-work legislation is negatively associated with the provision of health plans. These results support our hypothesis that organized labor remains an important force in sustaining the provision of health plans.

Other coefficients behave in their expected manner. At the establishment level–higher compensation, a greater proportion of female and older workers, and fewer part-time and low-wage workers are associated with greater likelihoods of providing any health plan. At the firm level, more established firms, a larger workforce, and non-profit status are all associated with higher likelihood of providing health benefits. When comparing firms of the same employment size, we see that firms with multi-unit operation are less likely to provide health insurance, which may be driven by the fact that workers are more dispersed and therefore have less collective bargaining power. At the state level, we do not see the demand for labor, measured by both unemployment and poverty rates, to have a significant impact on the decision of provision, though the coefficients are both negative.

In Model (2) we include the cost per worker to provide health plans as a determinant of provision. As expected, the higher the cost, the less likely the employer will provide the benefit. Furthermore, the inclusion of premium cost does not attenuate the association between workers collective bargaining power and health benefits, suggesting that the effects of union and related legislation are robust even when the potential cost is considered.

Since the actual cost of providing health insurance could be systematically higher for non-providing establishments than for providing establishments due to unobserved characteristics, we re-estimate Model (2) two more times with a different assumption for each model. In Model (3) and (4), we impute the cost for non-providing establishments to be 10 percent or 20 percent higher than for providing establishments, conditional on observed characteristics. The results suggest that the impact of employment relationship remains substantial even with alternative cost measures.

To test whether employment relationship moderate the adverse effect of premium cost, we interact premium cost with the three indicators of employment relationship and include them within the model specified in Table B.2 Model 1. We present these estimates in Table B.3 showing that both the union densities at the establishment- and the state-level reduce the adverse effect of premium cost, while the right-to-work legislation intensifies the consequence. While we only have conclusive evidence indicating that state-level union density significantly reduces the impact of rising premium ($P<0.05$), these results suggest that the adverse of premium costs could be exacerbated by the deterioration of organized labor.

3.5 Discussion

This article examines how rising premium cost and employment relationship jointly shape the provision of health insurance in workplaces. We find that, while premium cost is a clear deterrent for the offering of health plans, workers collective bargaining power remains an important determinant for the provision of health benefits in the 21^{st} century. Evidence suggests that both establishment- and state-level union densities increase the likelihood of employers providing health plans, while right-to-work legislation depresses the provision. Furthermore, state-level union density reduces the adverse impact of premium costs, which indicates that employment relationship could either exacerbate or moderate the consequence of rising premium costs.

Much of the current discussion on employer-sponsored health care has been concentrated on the effect of the Affordable Care Act (ACA) on employer offers. New evidence indicates that coverage has not declined because of the ACA, and that coverage may have modestly risen in advance of the employer mandate [90]. In the meantime, less attention is paid to how the prevalence of employer-sponsored insurance may affect the success of the ACA. Nationally, the ACAs exchanges are less likely to provide sufficient coverage in states where organized labor is weak and right-to-work laws are instituted **??**. This suggests that a more tenuous employment relationship could offload the burden of health expanse from employers to employees and indirectly undermine the exchanges.

Two policy recommendations could be made to strength labor bargaining power. If unions in small, low wage workplaces are the carriers of increased healthcare coverage, it will be key to support legal frameworks which facilitate these campaigns. Sponsorship of the Employee Free Choice Act, which would enable unions to certify elections with signatures and increase responsibilities and penalties for not following through on arbitration, would be a powerful step in low-wage workplaces. Next, it will be important to uphold the 2015 Browning-Ferris decision of the National Labor Relations Board, recently repealed and then reinstated, which establishes joint-employer status between contractors, franchises and larger employers. Such a legal framework is crucial in large, franchised or subcontracted workplaces to enable divided, precarious workers to organize across work units for healthcare.

3.6 Conclusion

While the rising premium cost has been a main deterrent for employers to offer health plans, this study indicates that the decision to provide is also embedded in a wider social context. Employers are more likely to provide health plans when their workers are organized and when the establishment locates in a more labor-friendly state. Our results suggest that, in addition to the employer mandate provision, policies that strengthen organized labor could promote the access to health care and lessen the burden of the ACA and its associated Medicaid expansion. Future research should consider organized labor as an important determinant of the provision of employment-based health insurance.

4. THE ROLE OF EDUCATORS IN THE REPORTING OF CHILD MALTREATMENT: CAUSAL EFFECTS OF SCHOOL ATTENDANCE

4.1 Introduction

The U.S. Department of Health & Human Services [91] reports that over 3.2 million children received an investigation or alternative response in suspicion of child maltreatment in 2014 with 702,000 of those children being proven victims of child maltreatment. In the same year they estimated that 1,580 children died from abuse or neglect.

The long run outcomes for maltreatment victims have been well-documented. Children exposed to abuse have an increased risk for delinquency and criminal involvement as adults as well as an increased risk for violent behavior [92, 93]. These children are also more likely to suffer from depression [94, 95], behavioral problems [96], post-traumatic stress disorder [97, 98], and are at an increased risk for chronic diseases [99, 100]. Currie and Widom [101] report that adults with documented histories of childhood abuse and/or neglect have lower levels of educational attainment, earnings, and fewer assets, with more pronounced effects for women. The societal economic burden created by child maltreatment for 2008 was estimated to be as large as $585 billion [102]. With such a high cost for the victims and society as a whole, efforts to prevent future child maltreatment are of great importance.

One first step towards combating maltreatment is reporting the incidents whenever they occur. Unfortunately, it is well documented that reports sent to state CPS agencies grossly underestimate the actual severity of child maltreatment [103, 104, 105, 106, 107]. It is estimated that official rates for substantiated child maltreatment represent less than 10% of actual abuse and neglect [108]. One could surmise that the reason for this large number of unreported incidents is that child maltreatment can only be reported if the victims are visible to potential reporters. Most abuse and neglect is committed by parents or caretakers whereas the vast majority of reports stem from referrals made by individuals outside of the home environment [91]. Throughout the last decade, numerous states across the US have signed into law over one hundred legislative acts involving the reporting of child maltreatment.[1] A multitude of occupations have been deemed mandatory reporters of child maltreatment; social workers, teachers, counselors, law enforcement officers, and physicians in nearly every state are all required to report to child protective services if they suspect abuse or neglect [91]. All of these efforts to limit the amount of underreporting do little if children are not visible to potential reporters. Attending school is one of the biggest ways victims of child maltreatment can increase their visibility to potential reporters.

The main contribution of this paper empirically measures the magnitude of the effect school attendance has on maltreatment reporting. I estimate the effect by exploiting the seasonal patterns in school attendance using two methodological approaches. The first approach is a simple difference-in-differences design leveraging variation in attendance across different age groups. Children below the age of five are less likely to be enrolled in school, therefore, maltreatment reports for this age group are less likely to be affected by the academic calendar compared to children six or older. Reports for children below the age of five gives us information on the expected change in reporting from the school year to the summer months not driven by school attendance. The second approach is similar to the first except it incorporates enrollment data at the state-age level to better capture the actual level of school attendance at varyings ages for different states. This is especially important as there is considerable variation across states in participation in pre-K and Headstart programs [109].

Another important contribution shows that reports initiated by educators do not simply replace reports that would have been initiated by other reporting entities. One might expect non-educators to have a sharp increase in reporting whenever children begin their summer break, potentially picking up cases that would have been filed by educators, but this does not appear to happen. This suggests a lack of substitutability in reporting between educators and non-educators. It also implies that a substantial number of reports made by teachers would otherwise go unreported. This highlights the importance of states requiring educators to report any incident of suspected child

[1] Education Commission of the States (ECS) State Policy Database, https://b5.caspio.com/dp.asp?AppKey=b7f93000695b3d0d5abb4b68bd14&id=a0y70000000CboyAAC

maltreatment.[2] Unfortunately, even with this mandate, some teachers still fail to file reports even when they suspect abuse might have occurred [110]. A back of the envelope calculation suggests that over 28,000 reports of substantiated abuse go unreported each year when school-aged children are on summer break.

With such a large number of unreported cases of child maltreatment it is clear that maltreatment reports do not accurately reflect the true level of actual maltreatment. For my identification to be valid actual maltreatment cannot drive the downward trend in summer reporting. If it were the case that actual maltreatment falls during the summer months then it would mechanically follow that reporting would drop as well. Using proxy data for actual child maltreatment in the form of inpatient hospitalization records and mortality data, I find no evidence that actual maltreatment falls during the summer months. A secondary threat to identification comes from educators potentially over-reporting less severe or marginal cases of maltreatment that would have not been deemed serious enough to be reported by other entities. This is not the cases as the effect of school attendance on maltreatment reporting still remains even after restricting the sample to more severe cases of abuse an neglect.

The rest of this paper will go as follows: Section 4.2 will give a description of the maltreatment and enrollment data. Section 4.3 will describe the methodologies used in this paper. Section 4.4 will go over the results. Section 4.5 will address identification concerns. Section 4.6 will discuss policy implications and conclude.

4.2 Data

4.2.1 Data on Maltreatment

Data for child maltreatment reporting comes from the National Child Abuse and Neglect Data System (NCANDS) Child File, a federally sponsored effort that annually collects maltreatment data from state CPS agencies.[3] The data represents a census of child maltreatment reports known to CPS agencies from nearly every state across the nation for the years 2002 to 2015. The data

[2] https://www.childwelfare.gov/topics/systemwide/laws-policies/statutes/manda/

[3] This data is provided by the National Data Archive on Child Abuse and Neglect from the College of Human Ecology at Cornell University[111].

includes general information about each incident, including the date the report was filed, what reporting entity initially referred the incident to CPS, and the state where the report was filed. Due to the sensitive nature of the data all reporting dates are suppressed to two reporting dates per month. Any incident that occurred between the 1^{st} and 15^{th} get recorded as an "8" and any incident that occurred after the 15^{th} was recorded as a "23". This gives a total of 24 distinct reporting periods, or dates, throughout each year.

Each report also lists the reporting entity that initiated the referral of alleged maltreatment to a CPS agency. The different reporting entities are listed in Table C.1 along with the portion of total maltreatment reports each entity is responsible for at various ages. Educational personnel show the largest change in the proportion of total reports as the age group increases. The proportion doubles in size from 9.17% at age 3 to 19.02% at age 5 with a plateau of around 24% from age 7 to 12. No other reporting entity has such drastic change in their relative proportion of total reports. For this paper, "reports from non-educators" will consist of all reporting sources other than reports filed by education personnel. Unknown or missing reporting sources were dropped from the analysis. Results were not sensitive to this omission.

The data is structured in a way that each observation consists of a unique child-report pairing. A benefit of this unique pairing is that it eliminates the concern for potential double counting of maltreatment reports. For example, if an educator and another entity initiate maltreatment reports for the same incident then an increase in the number of reports during the school year does not necessarily reflect an increase in the number of children being reported. A limitation of the data however is that multiple children can be included on the same report even though the initial referral was for a single child. This implies that it is not clear if the reporting entity actually referred a case of potential maltreatment for a certain child included in a report with multiple children. Many states conduct investigations for all children within a family whenever any child is the subject of an investigation [91]. I will refer to the uncertain inclusion of siblings on a maltreatment report as the "sibling effect." I restrict a portion of the analysis to reports involving only one child to address this issue.

I aggregate the child-report pairings by age, reporting date, and year for my first analysis that relies on the variation in reporting across child age groups. For my analysis using variation across states, I re-aggregate the data to the age, state, reporting date, and year level.The aggregated counts are then divided by the relevant number of reporting days for each of the 24 reporting periods throughout each year to eliminate any mechanical fluctuations. Any reports that were unsubstantiated due to being intentionally false were dropped from the sample. Reporting rates are calculated using yearly population data provided by the National Cancer Institutes's Surveillance Epidemiology and End Results (Cancer-SEER) program.

4.2.2 Data on School Enrollment

School enrollment data are based on the American Community Survey (ACS) 1-year estimates collected annually by the U.S. Census Bureau.[4] The data consists of the enrollment percentage of each age group within each state for each year from 2002-2015. Table C.2 reports that nearly a third of all three-year-olds across the U.S. are enrolled in some form of schooling. This percentage rises with each successive age group. Almost all children are enrolled in school by the age of six. Since the data measures whether or not a child attended school within a certain year it cannot be used to determine when children are actually attending school.

It is important to note that the definition of enrollment within the survey also includes nursery or preschool. For children ages 3 or 4 it could very well be the case that they are enrolled in nursery or preschool that remains open throughout the year and thus would not have a summer break. This is troublesome in that I am making the assumption that all children enrolled in school will have the same academic calendar. However, I would expect this issue to only bias the estimates towards zero. The enrollment data contributes towards measuring the drop in aggregate school attendance across age groups during the summer. If there is no drop in attendance for 3 or 4-year-olds during the summer, then the level of enrollment should have no significant effect when considering the changes in maltreatment reporting that occurs from the school year to the summer months.

[4]Data is provided by the Integrated Public Use Microdata Series (IPUMS-USA).

4.3 Methods

4.3.1 Defining the Academic Calendar

I define the months from September to May as the academic year. This is a rough measure for the academic calendar, as many schools might still hold some classes during the summer months and all schools have multiple breaks within the school year. A more comprehensive academic calendar could take into account Thanksgiving, spring, and winter breaks as well as include a more precise cutoff for the summer break for each school district across the United States. Unfortunately it is not feasible for me to construct such as measure. During the months of June, August, and the second half of December schools are likely to be transitioning to or from break at different times. This leaves ambiguity as to what percent of children are actually attending school during these dates. This is not a major concern as it would only serve to bias the estimates downwards. A separate analysis dropping the troublesome dates showed an increase in the estimated effect.

4.3.2 Estimations Based on Variation in Child Age

This first methodology relies on variation driven by differences in seasonal reporting across different child age groups. For this estimation strategy maltreatment reports are aggregated for each age, reporting date, and year. The estimation model for this methodology is:

$$ln(CM_{awy}) = \alpha_a + \theta S_m + \beta T_a * S_m + \epsilon_{awy}, \tag{4.1}$$

where CM_{awy} are rates of maltreatment reports for children of age a in half month w for year y; α_a are age fixed effects; S_m is an indicator for the school year months of September to May; and T_a is an indicator if the child is of "schooling age," defined as either five or six.[5] The random error term, ϵ_{awy}, is allowed to be correlated across time within an age group and across all ages in any given year by estimating two-way standard errors following Cameron et al. [112]. This difference-in-differences model measures the increase in maltreatment reporting during the school year for children reaching "schooling age" compared to children not of "schooling age." The identifying

[5] Poisson models yield very similar estimates.

assumption is that younger children not of “schooling age" provide a comparison for how reports would have changed from the school-year to the summer months not driven by school attendance.

4.3.3 Estimations Based on Variation by State Using School Enrollment Data

Adding school enrollment data into the model described above incorporates and intensity level of actual school attendance at varying ages across different states. Maltreatment reports for this estimation are aggregated at the each age, state, reporting date, and year level. The following estimation model is implemented:

$$ln(CM_{aswy}) = \alpha_s + \theta S_m + \beta \text{Enroll}_{as} * S_m + \epsilon_{aswy}, \tag{4.2}$$

where CM_{aswy} are rates of maltreatment reports for children of age a in state s in half month w for year y; α_s are state fixed effects; S_m is an indicator for the school year months of September to May; and Enroll_{as} is the enrollment percentage for children of age a in state s averaged across all years.[6] The random error term, ϵ_{aswy}, is allowed to be correlated across time within a state and across all states in any given year by estimating two-way standard errors following Cameron et al. [112]. The identifying assumption underlying this model is that states with a higher school starting age (or lower enrollment for younger ages) offer as a comparison group for kids of the same age in other states with a lower school starting age (or higher enrollment for younger ages) telling us how reports would change from the school-year to summer not driven by school attendance.

4.4 Analysis

4.4.1 Maltreatment Reports from Educators

Looking at the reports initiated by educators, Figure C.1 displays the average reporting rates for each of the 24 reporting dates for different child ages for the years 2002-2015. The drop in reports during the summer months is apparent for any age group with the smallest drop occurring

[6] I add 0.0625 (the smallest reported unit) to all child maltreatment report counts before constructing log rates to avoid dropping observations for which the outcome would otherwise be undefined. This amount comes about from the bimonthly reporting nature of the data. The largest reporting period is 16 days so 1 report in 16 days corresponds to 1/16 or 0.0625 reports per day. Poisson models yield very similar estimates.

for children 2 and under and then increasing in relative magnitude with each older age group. The most drastic increase occurs from ages 4 to 5 with an additional large increase occurring from ages 5 to 6. This is expected since these age groups have the largest increases in school enrollment as reported in Table C.2. The increase in school enrollment from age 4 and age 5 is 25.5% and 12.8% from ages 5 to 6. After age six, enrollment remains constant with nearly all children enrolled in some form of schooling.

There is some seasonality in reports initiated by educators for the age group two and under. It is not expected for children at these ages to be enrolled in any sort of program that uses a traditional academic calendar. This is a visual representation of the previously mention "sibling effect" which will be addressed later on in Section 4.4.3.1.

A vast majority of states have their cutoff age for starting kindergarten to be 5 on or before a specified date in the fall.[7] Thus, a 5-year-old listed on a maltreatment report filed in September is more likely to have been enrolled in school than a 5-year-old on a maltreatment report that was filed in May. This scenario can be seen in the maltreatment data when differencing the reporting rates between certain age groups. Figure C.2 displays the difference in the reporting rates between children ages 6 and 5 as well as the difference in the reporting rates between children ages 7 and 6 as a comparison. Starting from September, the difference in reports between children aged 6 and aged 5 is almost zero. This difference increases as we move through the year peaking in May at 0.014 reports per 1,000 children before reducing back to zero during the summer months. There is no significant difference in reporting rates by educators across months between children aged 7 and aged 6 as nearly all children of age six are enrolled in school regardless of their birth month.

Looking at reports from non-educators in Figure C.3, reporting rates remain fairly consistent throughout the year for each age group. Reports for children under the age of 2 have the highest reporting rates with each successive age group decreasing in the rate of reports with six-year-olds having the lowest rates shown in the figure. For all age groups the reporting patterns throughout the year remain the same. There no strong changes during the summer months and, more importantly,

[7]National Center for Education Statistics, State Education Reforms
https://nces.ed.gov/programs/statereform/tab5_3.asp

no visible differences in reporting between age groups four, five, and six. If reports initiated by educators substituted for reports initiated by non-educators then there should be an uptick during the summer months for children of schooling-age. There could be heterogeneity among the different entities that comprise of the non-educator group but any effects do not show in the aggregate.

With the relative homogeneity across ages for reports initiated by non-educators and the heterogeneity in reports from educators, it is of no surprise that reports for school-age children drop dramatically during the summer months. Figure C.4 displays the total reporting rates for children ages three to four and the total reporting rates for children ages five to twelve. Children below the traditional schooling-age have lower levels of school enrollment offering a comparison as to how reports would change from the school year to the summer months not driven by school attendance. There is a small drop in reporting for children ages three to four but it pales in comparison to the drop experienced for the age group five to twelve. Using the methodology described in Section 4.3.2, I estimate the effect of school attendance on maltreatment reporting within this difference-in-differences framework in the following section.

4.4.2 Regression Results for Variation by Child Age

The results for estimating Equation 4.1 are reported in Table C.3 where each row and column are one regression. The first panel defines the cutoff age for starting school at age five and the second panel defines it at age six. Starting with column (1) the estimates indicate that attending school increases total maltreatment reports by 17.0% during the school year when defining the school starting age at five and lessens to 15.0% when changing the age cutoff from five to six. Columns (2) and (3) in Table C.3 add month and year fixed effects and then month-year interacted fixed effects, respectively. The estimates remain constant with the inclusion of these fixed effects showing that the estimated effects for both age cutoffs are not sensitive to monthly fluctuations in reporting or long term changes throughout the reporting period. The remaining columns (4) through (8) vary the sample size by different age groups to show which age groups are mainly responsible for driving the results. Restricting the sample to children ages four to six, the estimated effect of school attendance on maltreatment reports is a 10.0% increase when defining the starting age at five, 9.0% for age six. Adding successive age groups to the sample, moving from columns (5) to (7), serve to bolster the magnitude of the effect. This shows that maltreatment reports for the additional age (treatment) groups are more affected by the school year than those ages closure to, but above, the age cutoff. Finally, when restricting the age group to children ages three to six in column (8), the estimated effect of school attendance on maltreatment reporting is a 12.2% increase when defining the starting age at five, and 11.7% at age six. The magnitude of the effect is larger when comparing it to column (4) showing that maltreatment reports from children of age three are less likely to be affected by school year that those ages closure to, but below, the age cutoff.

When estimating the effect school attendance has on maltreatment reports, the estimates in Table C.3 should be considered conservative for three main reasons. First, school enrollment is defined by age. This strict definition assumes that all children above a certain age (five or six) are enrolled in school. When looking at Table C.2, no age group has complete enrollment, or non-enrollment, in school. The second methodology using state level variation described in Section 4.3.3 will better account for the differences in enrollment at varying ages across different states. Results for this analysis are located in **Section 4.4.3**.

The second reason why these estimates should be considered conservative is the issue of the previously mentioned "sibling effect." Since many states include all children within a family on a maltreatment report whenever any child becomes part of an investigation, it is not clear which child was initially referred to CPS. Thus, there could be numerous incidents in which a child under the typical schooling age is included on a report that was initiated by an educator for suspected abuse or neglect of an older sibling. A simply way to eliminate this issue is by restricting the analysis to only include single child reports. This analysis is located in Section 4.4.3.1.

Lastly, the school year is defined as the months of September through May. This rough definition for the academic calendar does not take into account the numerous breaks that occur throughout the school year. It also does not take into account the different beginnings and endings of the summer break across schools. In Section 4.4.3.2 I rerun the analysis dropping observations from the months of June, August, and the second half of December to address this measurement error and better capture whenever children are actually attending school. Dropping these dates of more questionable school attendance better segments the treatment period, the school year, from the control period, summer break.

4.4.3 Regression Results for Variation by School Enrollment

Variation in school enrollment across states mostly occurs for children aged three to five. Figure C.5 plots the density of enrollment for children ages 3 to 6 using state-year observations. There is considerable variation in school enrollment across states for ages 3, 4, and 5 with nearly all children enrolled in school at age 6. In Figure C.6 states are separated into enrollment quartiles for all children ages 3 to 5. The estimates are then demeaned to better show seasonality in reporting throughout the year. States in the highest quartile of enrollment show the largest relative drop in reporting during the summer months with mixed results for the lowest and middle quartiles. This provides evidence that even the younger age groups, not necessarily of typical schooling age, benefit from an increase in enrollment through greater maltreatment reporting.

Estimates exploiting variation in school enrollment using Equation 4.2 are reported in Table C.4 where each column is one regression. I first restrict the sample to children ages three to six. School enrollment increases the most for this age range and also has the greatest variation in enrollment across states. Starting with column (1), I start with age fixed effects as well as month interacted with year fixed effects so results would be comparable to column (8) in Table C.3. The interaction term between summer and reported school enrollment suggests, with complete enrollment, school attendance increases reports by 28.3%. This is over twice the magnitudes reported in column (8) in Table C.3 showing the importance of a more accurately defined school attendance variable. In column (2), with the inclusion of state fixed effects the estimate drops in magnitude to 25.2%. State fixed effects control for the relative differences in the reporting rates across states. The interaction term between the school year and enrollment no longer needs to explain the aggregate differences across states and instead focuses on the relative differences between states. Column (3) reports an increase back to 28.3% after the inclusion of state interacted with age fixed effects. These fixed effects account for the variation in the potential differences in reporting practices across age groups within each state. Further interacting age and year fixed effects, controlling for national-level changes in reporting patterns that might target specific age groups, does little to change the estimates as shown in column (4).[8]

Dropping three-year-olds from the sample in column (5) further increases the estimated effect of school attendance on maltreatment reporting to 34.6%, assuming complete enrollment. This suggests that a proportion of three-year-olds might be enrolled in some form of year-round schooling. If there is no change in attendance from the school year to the summer months then the interaction term defining school attendance should have no estimated effect. Thus, including them in the sample would serve to bias the estimates toward zero.

Adding additional older ages to the sample, reported in columns (6) to (8), serve to increase the magnitude of the estimated effect. This increase is expected because the proportion of the reports from educational personnel are larger for the older age groups as reported in Table C.1. Lastly, I include the full sample in column (9) which is my preferred specification. Previously, the estimated effect of attending school reported in Table C.3 was a 17.0% increase in maltreatment reports. Now using a more precise definition of school attendance this effect increases to 36.9%.

4.4.3.1 “Sibling Effects"

Maltreatment reports initiated by educators are not only important for the children attending school, they are also important for their siblings. Siblings of maltreated children are also at high-risk to suffer from maltreatment as well [113]. For this reason many states conduct investigations for all children within a family whenever any child is the subject of an investigation [91]. This gives younger siblings that are victims of child maltreatment a better chance of receiving help whenever their oldest sibling starts attending school.

The NCANDS data does not directly describe the relationships between children for reports that include more than one child. The data does however include child-perpetrator relationship

[8]Further including state interacted with year fixed effects to account for state policy changes in maltreatment reporting did little to nothing in changing the magnitude or precision of the estimates.

variables for substantiated or indicated cases of maltreatment.[9] If a report were to have at least two children with substantiated or indicated dispositions and non-missing child-perpetrator relationship variables, it is possible to make child to child relationship pairings within reports. For substantiated or indicated reports filed by educators that contained multiple children, 38.8% of these reports contain a child age four or under. Of the 38.8% of reports, 94.5% also include a child of five years or older. Lastly, of those 94.5% of reports, 79.7% of the younger (age four or under) children were matched with an older (five or older) sibling and 82.7% were matched with an older relative.[10]

The inclusion of all children within a family on a maltreatment report explains the considerable seasonality for children ages two and under shown in Figure C.1. Figure C.7 limits the focus to reports with only one child, around 30% of the original sample. The seasonality for children two and under goes completely away along with a comparatively large drop for children ages three and four. Since it is not possible to determine the relevant population for single child reports, the reporting rates for all ages drops significantly as the sample size is greatly reduced with the population denominator remaining unchanged. Figure C.8 and Figure C.9 represent the same graphs as Figure C.3 and Figure C.4, respectively, except the data is limited to reports with only one child. Comparing Figures C.3 and C.8, there is no discernible difference in yearly reporting patterns between the full sample and single-child report sample for reports initiated by non-educators. There is, however, a sizable difference between Figures C.4 and C.9 in the relative drop during the summer, meaning that the elimination of the "sibling effect" from the analysis should bring forth a greater estimated effect as shown in these two figures.

If we were to assume that the children on single-child maltreatment reports are no different in their school enrollment practices than those on reports with multiple children, then rerunning the analysis using only single-child reports should yield a more accurate estimate of the true effect of school attendance on maltreatment reporting. Implementing the same estimation strategy used in

[9]Some states include an a additional disposition of "indicated" meaning there is sufficient reason to believe the child was maltreatment or is at risk, but the case does not meet the level of evidence required for substantiation by state law. NCANDS considers children receiving a disposition of substantiated or indicated to be a victim of maltreatment [91].

[10]Due to the nature of the data, it is not possible to determine which child was referred to CPS first that initially started the investigation. It is possible that a child below the age of five was referred by an educator and the older sibling was included in the investigation, however, the reverse of the story seems more probable.

Table C.4, Table C.5 reports the estimated effect of school attendance on maltreatment reporting using only single child reports. For every estimated effect outside of columns (1), the magnitude increases by 80 to 90% with this alternative sample. Initially the effect of school attendance on maltreatment reporting, assuming complete enrollment, was estimated as 36.9% in column (9) of Table C.4. When only using reports involving only one child, thus eliminating the measurement error caused by the "sibling effect," the effect increases to 65.5%.

4.4.3.2 Adjusting the Academic Calendar

The school year in this paper is defined in a very strict manner. It does not account for the various beginnings and endings of summer break for schools across the US. One way to help reduce the measurement error induced from this crude measure of the school year is to restrict attention to observations more certain that children are either on break or in school. It is in this spirit that I rerun the main analysis omitting the observations from June, August, and the second half of December. At these dates many schools across and within states are transitioning to or from a break in schooling. Dropping these observations from the analysis better segments when children are attending school and when they are not. Table C.6 displays the results from the initial analysis dropping the observations of questionable school attendance. In all instances the magnitude of the estimated effect increased in magnitude. If it were possible to include the actual level of school attendance throughout the year within the model, we would suspect that these estimates would increase further.

4.4.4 Non Crowd-Out in Reporting

If it were the case that different child maltreatment reporting entities could fill in or fully substitute for each other in their reporting, then the large drop in reports from educators during the summer months would not be as problematic. However, a cursory glance at Figures C.3 and C.8 shows this not to be the case. There is no noticeable difference in reporting patterns during the summer months from non-educators between age groups. If reports initiated by educators replaced the reports that would have been filed by non-educators then we should see a spike in reporting during the summer months for school-age children from non-educators. Since this does not happen it gives strong descriptive evidence that reports filed by educators do not fully crowd-out the reports from other entities. This implies the role educators play in the reporting of maltreatment is extremely important in that if they do not report a potential incident of maltreatment, there is a good chance no one else will.

Previous to this section all estimations measured the effect of school attendance on total maltreatment reports. Using the same estimation strategy implemented in Table C.4, Table C.7 bifurcates maltreatment reports into those initiated by educators and those initiated by non-educators. From the table it is clear that the bulk of the increase in reports during the school is driven by educators as expected. Incorporating all the different fixed effects, column (4) reports that school attendance, assuming complete enrollment, increases maltreatment reports initiated by educators by 86.0% for the sample with the greatest variation in school enrollment. This effect increases to 88.9% for the full sample in column (9). These large increases in reporting are not surprising as we would expect reports initiated by educators to have the largest effect. What is surprising is the limited response from non-educators. The same columns report an estimated effect of 1.8% (statistically insignificant) and 6.7%, respectively. It is also surprising that these estimates are both positive. If reports initiated by educators crowded out the reporting from other entities then this estimate should be negative.

4.4.5 Adjusting for Measurement Error

As previously mentioned, Table C.3 suffers from measurement error in three main ways: 1. the way enrollment is defined 2. "sibling effect", and 3. the defined school year. Table C.8 reports the effect of school attendance on maltreatment reporting correcting for the various measurement error issues. Columns (1)–(4) use the specification listed in column (3) from Table C.3, and columns (5)–(8) use the specification listed in column (10) from Table C.4. Correcting for any of the measurement errors drastically increase the magnitude of the estimated effect. My preferred specification from this table is column (6) which uses school enrollment data and corrects for the sibling effect.

4.5 Threats to Identification

To assess the validity of the statement that school attendance increases maltreatment reporting, two main concerns need to be addressed. First, it needs to be shown that the actual rates of child maltreatment do not fall during the summer months. Second, it could be that teachers are over-reporting minor/marginal cases of maltreatment that other reporting entities might not consider severe enough to intervene.

4.5.1 Concern 1: Actual Maltreatment Might Fall When Children Are Out of School

A rhetorical argument can be made that the rate of maltreatment might actually increase. Psychologists have shown that an increase in temperature directly leads to an increase in aggression and violent crimes [114]. As the ambient temperature increases during the summer months it could be expected that perpetrators become more likely to commit some form of abuse. Additionally, [115] posit that a potential mechanism for child maltreatment could be attributed to changes in the time spent at home. Parents are listed as the perpetrator of maltreatment in 78% of substantiated reports in 2014 [91]; thus it is not unreasonable to assume that maltreatment would increase with a greater amount of time being spent at home with a potential perpetrator during the summer break from school. Both the increasing temperature and increasing time usage during the summer months could contribute to an overall increase in maltreatment.

If it is the case that maltreatment truly falls during the summer months we would expect to see drops in cases from the reporting sectors that are less affected by the academic calendar. Looking at Figures C.3 and C.8 there is no visible evidence to suggest that there is a decrease in reports from non-educators. It is actually the case that non-educators have the highest rates of reporting during the summer months, but for the most part the reporting rate remains relatively constant throughout the year. There could be both a drop in maltreatment during the summer and an increase in reporting from non-educators during the same time, although this scenario seems unlikely.

Lastly, data from the Healthcare Cost and Utilization Project (HCUP) Kids' Inpatient Database (KID) and the National Center for Health Statistics (NCHS) Multiple Cause of Death Mortality

Data provide a proxy for more serious cases of actual child maltreatment throughout the year. A benefit of these data is that they are less likely to suffer from seasonal underreporting issues that exist within the maltreatment reporting data.

The HCUP KIDs file represents the largest publicly-available all-payer pediatric inpatient care database in the U.S., covering around a weighted estimated 7 million hospitalizations each year. The KID is collected every three years and for this paper I use the years 2000, 2003, 2006, 2009 and 2012. Each hospitalization reports multiple diagnoses based on the International Classification of Diseases, Ninth Revision, Clinical Modification (ICD-9-CM). Also included in the reports are multiple ICD-9-CM external cause of injury codes (E codes). Using both the diagnoses codes and E codes I created variables counting total cases of various types of child maltreatment for each month throughout the data collection years.[11]

Figure C.10 displays the weighted total of monthly inpatient reports of child maltreatment for children ages three to twelve averaged across all reporting years. The figure includes the top three types of maltreatment–physical abuse, neglect, and sexual abuse–as well as all inpatient reports that had a diagnosis or external cause of injury from any form of child maltreatment. For the most part throughout the year maltreatment in any form remains fairly constant. There is a drop in inpatient reports for all cases during the month of July, but this drop is immediately followed by a large increase in August.[12]

The second proxy for actual child maltreatment comes from the NCHS mortality data. I create two categories of mortality–homicides and maltreatment. These categories were identified using the International Classification of Diseases (ICD).[13] Figure C.11 displays these two categories in terms of total counts for children ages three to twelve in each month averaged across the years 2002-2015. Homicide counts are located on top with the sub-category of child maltreatment related

[11]ICD-9-CM codes used for maltreatment consist of: 9955, 99550, 99551, 99552, 99553, 99554, 99555, and 99559. E codes used for maltreatment consist of: E967, E9670, E9671, E9672, E9673, E9674, E9675, E9676, E9677, E9678, and E9679.

[12]In a similar study using the same data, [116] estimated maltreatment for each season for those aged 21 or under. They document very marginal changes throughout the year with the largest differences being between the Spring months (25.8% of yearly cases) and Summer (24.7%).

[13]Using the ICD-10 358 recodes of selected causes of death the following group codes were used for homicides: 43200-44100 and maltreatment: 44000

homicides on bottom. Maltreatment related homicides are identified as those caused by neglect and abandonment or other forms of maltreatment such as mental cruelty, physical abuse, sexual abuse (excluding sexual assault by bodily force), and torture. In either case, whether it be homicides or the sub-category of child maltreatment related homicides, neither show any indication of a drop in counts during the summer months of June through August.[14]

4.5.2 Concern 2: Over-reporting of Maltreatment by Educators

A second concern stems from the potential for educators to over-report minor or marginal cases of child maltreatment. If educators are being over cautious in reporting what they perceive to be maltreatment bu which is not actual maltreatment, then the increase in reports during the school year would be unlikely to help actual victims of abuse and neglect.

In order to address this issue I reestimate Table C.4 limiting the outcome variables to reports with a higher degree of severity. Table C.9 reports the effect of school attendance on substantiated maltreatment reports, reports that lead to the removal of the child from the home, and reports that lead to a child placed into foster care. For all three outcomes the effect is similar to that of the full sample, however, the magnitude is somewhat smaller. Using the preferred specification, column (10), the effect of the school year on all reports was estimated at 36.9%, when limiting the analysis to only substantiated reports this effect decreases to 29.5%. Even further, when looking at arguably even more serious cases, reports that lead to the child being put into a foster care home and reports that resulted in a child being removed from the home, the estimated effect decreases to 21.6% and 21.8%, respectively.[15] The decrease in magnitude could potentially provide evidence of a general trend in over-cautiousness in maltreatment reporting coming from educators. However, with the general positive trend in more severe cases being consistent with the estimates reported in Table C.4, over reporting by educators is not of any concern when considering the validity of the main estimates.

[14] Laskey et al. [117] neglect to find a drop in child homicides during the summer months and go even further to say that there does not appear to be any seasonality in child homicides throughout the year.

[15] Reports that were substantiated account for 18.1% of all reports, foster care placement 5.7%, and removal from the home 5.9%.

4.6 Discussion and Conclusion

A big reason for the large number of unreported maltreatment incidents is that victims are not visible to the potential reporters. School attendance offers children their first big boost in public visibility as educators–many trained to spot indicators of maltreatment–begin to interact with children on a daily basis. Once children reach schooling age we see a large increase in maltreatment reporting driven by reports initiated by educators, picking up incidents that would have likely gone unreported. This effect is one that should not be surprising, as any increase in the monitoring of child abuse and neglect victims should being about more reports. What is potentially surprising is that reports from educators are not crowding out the reports filed by other reporting entities. This suggests a lack of over-lap in the monitoring of children from the various reporting entities. This makes it all the more important for educators to identify incidents and report them whenever possible as they might be the only entity with the possibility to do so. Even more so if the child has younger siblings below schooling age that are also likely to suffer from maltreatment as well.

The increase in reports during the school year shed light on the large number of maltreatment incidents that do not get reported during the summer months. In this case we should consider underreporting as a drop in reports that would have been known to CPS agencies had the children been in school. A back of the envelope calculation using the estimated effect of school attendance on substantiated reports listed in column (10) from Table C.9 estimates that 28,061 substantiated cases of abuse go unreported during the summer months when children are on break from school.[16] This calculation is contingent upon actual incidents of maltreatment remaining constant throughout the year. Evidence to suggest otherwise does not appear to be prevalent.

The large amount of underreporting during the summer months for school age children could illicit policy measures to extend the academic calendar. Economically speaking this would not seem to be a very efficient measure. A more reasonable approach to decrease underreporting can be

[16]The average number of substantiated reports per day per 100,000 children aged 3-12 is 2.59 reports. With a sample of 39,933,665 children and 92 reporting days during the months of June, July, and August this leads to around 95,123 total substantiated reports for all children aged 3-12 during the summer. Thus we have 95,123*29.5%=28,061.

applied to younger children through the increasing in enrollment for early childhood development programs like pre-K and Headstart. These programs have already shown to have beneficial impacts for children under the age of five [118]. Adding the increase in maltreatment reporting would only serve to benefit these age groups. Any measures to increase the visibility and monitoring of children vulnerable to maltreatment should only serve to increase reporting and increase the likely of maltreated children receiving the help and services they need.

5. CONCLUSION

With all great studies in economics, an importance needs to be placed on the usability of the information presented. What good is well-rounded paper if it is purely academic? A great study not only needs to be empirically sound but actionable as well.

From my work on substance abuse treatment in Chapter 2, it is clear that we as a society are not taking advantage of the large benefits we could receive when intervening in the illegal drug industry from the demand side of the market. The societal benefits of building a new treatment facility far outweigh its costs. This should encourage any community leader to consider increasing their efforts into substance abuse treatment, not only for the individuals that suffer from the addiction but also for the numerous residents living within their area. Drug addiction and abuse do not just affect the user, they affect the community as a whole.

The dynamic of the employer/employee relationship has changed throughout the past century. With the decline in collective bargaining control of the labor market has shifted favoring the employer. Additionally, coupled with rising health care costs, this shift in control has led to the decline in the offering of employer-sponsored health insurance. Understanding the major determinants relating to individual health care options is critical when deciding national-level health care policies.

Lastly, child maltreatment is an abhorrent part of our society. We must do all we can to limit the scope and frequency of incidents and the first step in alleviating this problem stems from reporting all possible incidents whenever they occur. The problem is that maltreatment children can only receive and intervention if they are visible to potential reporters. This is clear and evident when seeing the massive increase in reports that occurs whenever children begin to attend school. If a community wants to increase their efforts in finding and reporting incidents of maltreatment they will need to implement measures that increase the visibility of children to potential reporters.

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APPENDIX A

APPENDIX: SUBSTANCE ABUSE TREATMENT CENTERS AND LOCAL CRIME

A.1 Appendix Tables

Table A.1: Summary Statistics

	Mean	Std Dev
Substance Abuse Treatment Facilities (2,453 counties)		
Total	49.6	90.1
Net Openings	5.8	10.1
Net Closings	3.7	4.4
Facilities per 100,000	5.0	3.6
NCHS Mortality Files (2,453 counties)		
Homicides per 100,000	5.8	5.1
UCR Offenses Known Database (2,184 counties, 9,602 agencies)		
Homicides per 100,000	5.7	8.5
Sexual Assaults per 100,000	32.0	26.8
Aggravated Assaults per 100,000	309.2	288.0
Robbery per 100,000	164.8	180.0
Simple Assaults per 100,000	1118.2	878.7
Burglary per 100,000	762.1	523.2
Larceny per 100,000	2551.8	1479.1
Motor Vehicle Theft per 100,000	424.9	458.7

Notes: These data span 1999-2012. The means and standard deviations for the substance-abuse treatment facilities are derived from the NCHS Mortality sample. The reported facility statistics are similar when using the UCR Known Offenses sample. The means and standard deviations from the NCHS Restricted Mortality Files represent rates per 100,000 residents in each county and are weighted by county population. The means and standard deviations for the UCR Offenses Known Database rates per 100,000 residents covered by the municipal law enforcement agency and are weighted by agency population coverage.

Table A.2: Estimated Effects of SAT Facilities on Log Homicide Rates

	(1)	(2)	(3)	(4)	(5)
Homicide Data: NCHS Restricted Mortality Files					
Facilities Last Year	-0.0022***	-0.0028***	-0.0025***	-0.0025***	-0.0025***
	(0.0007)	(0.0005)	(0.0004)	(0.0004)	(0.0004)
Homicide Data: UCR Offenses Known Database					
Facilities Last Year	-0.0019***	-0.0020***	-0.0016***	-0.0016***	-0.0016***
	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0003)
County/Agency and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-by-year Fixed Effects	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes
Economic Controls	No	No	No	Yes	Yes
Officer Rate per 1,000	No	No	No	No	Yes

Notes: Estimates are based on 34,326 county-year observations for the NCHS Restricted Mortality Files and 106,965 agency-year observations for the UCR Offenses Known Database. Demographic control variables include the fraction of the population that are: white, black, male, ages 0–9, ages 10–19, ages 20–29, ages 30–39, ages 40–49, ages 50–59, and ages 60–69. Controls for economic conditions include the county unemployment rate and number of firm births. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by county population when using the NCHS Mortality data and are weighted by agency population coverage when using the UCR Offenses Known data.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.3: Estimated Effects of SAT Facilities on Log Violent Crime Rates

	(1)	(2)	(3)	(4)	(5)
Homicides					
Facilities Last Year	-0.0019***	-0.0020***	-0.0016***	-0.0016***	-0.0016***
	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0003)
Sexual Assaults					
Facilities Last Year	-0.0010**	-0.0004	-0.0004	-0.0004	-0.0005
	(0.0004)	(0.0003)	(0.0005)	(0.0005)	(0.0005)
Aggravated Assaults					
Facilities Last Year	-0.0034***	-0.0021***	-0.0011*	-0.0012*	-0.0012*
	(0.0009)	(0.0007)	(0.0006)	(0.0006)	(0.0006)
Simple Assaults					
Facilities Last Year	-0.0007	0.0004	0.0001	0.0001	0.0000
	(0.0006)	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Agency and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-by-year Fixed Effects	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes
Economic Controls	No	No	No	Yes	Yes
Officer Rate per 1,000	No	No	No	No	Yes

Notes: Estimates are based on 106,965 agency-year observations. Demographic control variables include the fraction of the population that are: white, black, male, ages 0–9, ages 10–19, ages 20–29, ages 30–39, ages 40–49, ages 50–59, and ages 60–69. Controls for economic conditions include the county unemployment rate and number of firm births. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by agency population coverage.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.4: Estimated Effects of SAT Facilities on Log Financially-Motivated Crime Rates

	(1)	(2)	(3)	(4)	(5)
Robbery Total					
Facilities Last Year	-0.0016***	-0.0019***	-0.0011***	-0.0011***	-0.0011***
	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
Motor Vehicle Theft					
Facilities Last Year	-0.0005	-0.0019***	-0.0012**	-0.0012**	-0.0012**
	(0.0011)	(0.0005)	(0.0004)	(0.0004)	(0.0004)
Burglary Total					
Facilities Last Year	-0.0010***	-0.0010***	-0.0005	-0.0005	-0.0005*
	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Larceny Theft					
Facilities Last Year	-0.0002	0.0001	-0.0006	-0.0006	-0.0006
	(0.0004)	(0.0004)	(0.0005)	(0.0005)	(0.0005)
Agency and Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
State-by-year Fixed Effects	No	Yes	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes	Yes
Economic Controls	No	No	No	Yes	Yes
Officer Rate per 1,000	No	No	No	No	Yes

Notes: Estimates are based on 106,965 agency-year observations. Demographic control variables include the fraction of the population that are: white, black, male, ages 0–9, ages 10–19, ages 20–29, ages 30–39, ages 40–49, ages 50–59, and ages 60–69. Controls for economic conditions include the county unemployment rate and number of firm births. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by agency population coverage.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.5: Expanding Model To Additionally Consider Contemporaneous and Future Facility Counts

	Homicide (NCHS Data)	Homicide (UCR Data)	Sexual Assault	Aggravated Assault	Simple Assault	Robbery	Motor Vehicle Theft	Burglary	Larceny Theft
Facilities Last Year	-0.0010*	-0.0016***	-0.0008	-0.0010	-0.0002	-0.0007**	-0.0006	-0.0003	-0.0003
	(0.0005)	(0.0005)	(0.0005)	(0.0008)	(0.0003)	(0.0003)	(0.0005)	(0.0003)	(0.0019)
Facilities This Year	-0.0014*	-0.0002	0.0001	0.0003	0.0001	-0.0002	-0.0004	-0.0002	-0.0011
	(0.0008)	(0.0007)	(0.0004)	(0.0009)	(0.0002)	(0.0003)	(0.0004)	(0.0003)	(0.0022)
Facilities Next Year	-0.0005	0.0002	0.0004	-0.0008	0.0003	-0.0003	-0.0005	-0.0001	0.0009
	(0.0008)	(0.0007)	(0.0004)	(0.0005)	(0.0003)	(0.0003)	(0.0004)	(0.0002)	(0.0010)

Notes: Estimates are based on 34,326 county-year observations for the NCHS Restricted Mortality Files in the first column and 106,965 agency-year observations for the UCR Offenses Known Database for the remaining columns. Outcomes are in log rates. All estimates control for county fixed effects, year fixed effects, state-by-year fixed effects, demographic controls, economic controls, and the size of the police force in the area. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by the population represented by each cell.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.6: Estimates Using Alternative Approaches

Panel A: Restricting Sample to Areas Reporting Positive Counts in All Years

	Homicide (NCHS Data)	Homicide (UCR Data)	Sexual Assault	Aggravated Assault	Simple Assault	Robbery	Motor Vehicle Theft	Burglary	Larceny Theft
Facilities Last Year	-0.0024**	-0.0018***	-0.0005	-0.0012**	0.0001	-0.0009***	-0.0011**	-0.0005	-0.0001
	(0.0010)	(0.0005)	(0.0005)	(0.0006)	(0.0003)	(0.0002)	(0.0004)	(0.0003)	(0.0002)
N	9145	4878	26772	67357	89722	39382	66040	93240	100647

Panel B: Inverse Hyperbolic Sine Estimates Using Full Sample

	Homicide (NCHS Data)	Homicide (UCR Data)	Sexual Assault	Aggravated Assault	Simple Assault	Robbery	Motor Vehicle Theft	Burglary	Larceny Theft
Facilities Last Year	-0.0026***	-0.0019***	-0.0006	-0.0012*	0.0000	-0.0011***	-0.0012**	-0.0005*	-0.0007
	(0.0004)	(0.0003)	(0.0005)	(0.0006)	(0.0004)	(0.0003)	(0.0004)	(0.0003)	(0.0006)
N	34326	106965	106965	106965	106965	106965	106965	106965	106965

Panel C: Unweighted

	Homicide (NCHS Data)	Homicide (UCR Data)	Sexual Assault	Aggravated Assault	Simple Assault	Robbery	Motor Vehicle Theft	Burglary	Larceny Theft
Facilities Last Year	-0.0017*	-0.0005	0.0002	-0.0002	0.0002	-0.0014***	-0.0016***	0.0000	0.0002
	(0.0008)	(0.0003)	(0.0004)	(0.0005)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0003)
N	34326	106965	106965	106965	106965	106965	106965	106965	106965

Notes: Outcomes are in log rates. Whereas the results in prior tables add one to counts to avoid dropping observations for which the outcome would otherwise be undefined, Panel A instead focuses on counties that never have a zero count for the specified outcome variable. Panel B transform counts using the inverse hyperbolic sine function as an alternative to adding one before taking the log such that the outcome variable is $ln(\frac{count+\sqrt{count^2+1}}{population})$. Unweighted estimates from the prior tables are in Panel C. All estimates control for county fixed effects, year fixed effects, state-by-year fixed effects, demographic controls, economic controls, and the size of the police force in the area. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions in Panel A and B are weighted by the population represented by each cell.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.7: Summary Statistics for Control Variables

	Mean	Std Dev
Demographic Controls		
% White Non-Hispanic	0.8	0.14
% Black Non-Hispanic	0.13	0.13
% Hispanic	0.17	0.17
% Male	0.49	0.01
% Ages 0-9	0.13	0.02
% Ages 10-19	0.14	0.02
% Ages 20-29	0.14	0.03
% Ages 30-39	0.14	0.02
% Ages 40-49	0.15	0.01
% Ages 50-59	0.13	0.02
% Ages 60-69	0.08	0.02
Economic Controls		
Unemployment Rate	0.06	0.03
Total Firm Births	3649	6070
Law Enforcement Presence		
Officers per 1,000	19.5	10.1

Notes: These summary statistics are from controls used in our agency-level analysis consisting of 106,965 observations. Demographic controls are based on the National Cancer Institutes's Surveillance Epidemiology and Ends Results (Cancer-SEER) program and are constructed at the county level. Unemployment rates are from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS) and are at the county level. Total firm births are based on the Census Bureau's Statistics of U.S. Businesses (SUSB) and are constructed at the county level. Officers per 1,000 are constructed using the FBI's Law Enforcement Officers Killed and Assaulted (LEOKA) at the agency level. All estimates are weighted by agency population coverage.

Table A.8: Estimated Effects on Log Homicide Rates, Lags and Lead

	(1)	(2)	(3)	(4)
NCHS Restricted Mortality Files				
Facilities 2 Years Ago		-0.0011*		
		(0.0005)		
Facilities Last Year	-0.0025***	-0.0017***	-0.0010*	-0.0010*
	(0.0004)	(0.0005)	(0.0005)	(0.0005)
Facilities This Year			-0.0019***	-0.0014*
			(0.0006)	(0.0008)
Facilities Next Year				-0.0005
				(0.0008)
N	34326	31875	34326	34326
UCR Offenses Known Database				
Facilities 2 Years Ago		-0.0004		
		(0.0004)		
Facilities Last Year	-0.0016***	-0.0013***	-0.0016***	-0.0016***
	(0.0003)	(0.0004)	(0.0005)	(0.0005)
Facilities This Year			-0.0000	-0.0002
			(0.0004)	(0.0007)
Facilities Next Year				0.0002
				(0.0007)
N	106965	100494	106965	106965

Notes: Column 1 reproduces the estimate shown in Column 5 of Table A.2. Columns 2–4 are based on the same model with the inclusion of the additional variables highlighted in the table. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by county population for the NCHS Mortality Files and are weighted by agency population coverage for the UCR Offenses Known Database.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.9: Estimated Effects on Log Violent Crime Rates, Lags and Lead

	(1)	(2)	(3)	(4)
Homicides				
Facilities 2 Years Ago		-0.0004		
		(0.0004)		
Facilities Last Year	-0.0016***	-0.0013***	-0.0016***	-0.0016***
	(0.0003)	(0.0004)	(0.0005)	(0.0005)
Facilities This Year			-0.0000	-0.0002
			(0.0004)	(0.0007)
Facilities Next Year				0.0002
				(0.0007)
Sexual Assaults				
Facilities 2 Years Ago		-0.0002		
		(0.0005)		
Facilities Last Year	-0.0005	-0.0004	-0.0008	-0.0008
	(0.0005)	(0.0006)	(0.0005)	(0.0005)
Facilities This Year			0.0005	0.0001
			(0.0004)	(0.0004)
Facilities Next Year				0.0004
				(0.0004)
Aggravated Assaults				
Facilities 2 Years Ago		-0.0017**		
		(0.0007)		
Facilities Last Year	-0.0012*	0.0003	-0.0009	-0.0010
	(0.0006)	(0.0004)	(0.0008)	(0.0008)
Facilities This Year			-0.0004	0.0003
			(0.0007)	(0.0009)
Facilities Next Year				-0.0008
				(0.0005)
Simple Assaults				
Facilities 2 Years Ago		0.0002		
		(0.0004)		
Facilities Last Year	0.0000	-0.0000	-0.0002	-0.0002
	(0.0004)	(0.0003)	(0.0003)	(0.0003)
Facilities This Year			0.0003	0.0001
			(0.0003)	(0.0002)
Facilities Next Year				0.0003
				(0.0003)
N	106965	100494	106965	106965

Notes: Column 1 reproduces the estimate shown in Column 5 of Table A.3. Columns 2–4 are based on the same model with the inclusion of the additional variables highlighted in the table. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by agency population coverage.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.10: Estimated Effects on Log Financially-Motivated Crime Rates, Lags and Lead

	(1)	(2)	(3)	(4)
Robbery Total				
Facilities 2 Years Ago		-0.0016***		
		(0.0003)		
Facilities Last Year	-0.0011***	0.0002	-0.0007**	-0.0007**
	(0.0002)	(0.0003)	(0.0003)	(0.0003)
Facilities This Year			-0.0005	-0.0002
			(0.0003)	(0.0003)
Facilities Next Year				-0.0003
				(0.0003)
Motor Vehicle Theft				
Facilities 2 Years Ago		-0.0005		
		(0.0005)		
Facilities Last Year	-0.0012**	-0.0007	-0.0005	-0.0006
	(0.0004)	(0.0004)	(0.0005)	(0.0005)
Facilities This Year			-0.0008	-0.0004
			(0.0005)	(0.0004)
Facilities Next Year				-0.0005
				(0.0004)
Burglary Total				
Facilities 2 Years Ago		-0.0006		
		(0.0004)		
Facilities Last Year	-0.0005*	-0.0001	-0.0003	-0.0003
	(0.0003)	(0.0004)	(0.0004)	(0.0003)
Facilities This Year			-0.0003	-0.0002
			(0.0003)	(0.0003)
Facilities Next Year				-0.0001
				(0.0002)
Larceny Theft (no MVT)				
Facilities 2 Years Ago		-0.0005		
		(0.0009)		
Facilities Last Year	-0.0006	-0.0001	-0.0005	-0.0003
	(0.0005)	(0.0011)	(0.0015)	(0.0019)
Facilities This Year			-0.0002	-0.0011
			(0.0016)	(0.0022)
Facilities Next Year				0.0009
				(0.0010)
N	106965	100494	106965	106965

Notes: Column 1 reproduces the estimate shown in Column 5 of Table A.4. Columns 2–4 are based on the same model with the inclusion of the additional variables highlighted in the table. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by agency population coverage.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.11: Estimated Effects of SAT Facilities on Log Homicide Rates Omitting the Largest Areas Based on Population

	Number of Counties/Agencies Omitted								
	0	1	5	10	20	50	100	200	500
Estimates Using NCHS Data, County-level Analysis									
Facilities Last Year	-0.0025***	-0.0021***	-0.0018**	-0.0025***	-0.0010	-0.0007	0.0010	0.0019	-0.0046
	(0.0004)	(0.0006)	(0.0006)	(0.0008)	(0.0010)	(0.0010)	(0.0015)	(0.0018)	(0.0033)
N	34326	34312	34256	34186	34049	33629	32929	31533	27335
Estimates Using UCR Data, Agency-level Analysis									
Facilities Last Year	-0.0016***	-0.0011***	-0.0011***	-0.0013***	-0.0011***	-0.0013***	-0.0013***	-0.0009**	-0.0000
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0003)
N	106965	106951	106898	106834	106698	106317	105692	104397	100444

Notes: Each column corresponds to the number of counties (NCHS data) or agencies (UCR data) dropped from the estimating sample based on population size. The first column drops no counties/agencies and presents the estimated effects of the full sample as reported in Column 5 of Table A.2. The second column shows estimated effects after dropping the largest county/agency from the sample. The third column drops the top five counties/agencies, and so on. Outcomes are in log rates. All estimates control for county fixed effects, year fixed effects, state-by-year fixed effects, demographic controls, economic controls, and the size of the police force in the area. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. The regressions are weighted by county population when using the NCHS Mortality data and are weighted by agency population coverage when using the UCR Offenses Known data.

*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

Table A.12: Estimated Effects of SAT Facilities on Log Homicide Rates Restricting the Sample to the Largest Areas Based on Population

	Number of Counties/Agencies Included				
	20	50	100	200	500
Panel A: Weighted Estimates					
Estimates Using NCHS Data, County-level Analysis					
Facilities Last Year	-0.0020**	-0.0019***	-0.0021***	-0.0022***	-0.0023***
	(0.0009)	(0.0004)	(0.0003)	(0.0004)	(0.0006)
N	168	504	1243	2679	6921
Estimates Using UCR Data, Agency-level Analysis					
Facilities Last Year	-0.0034	-0.0009	-0.0012	-0.0018**	-0.0022***
	(0.0030)	(0.0012)	(0.0010)	(0.0007)	(0.0006)
N	144	421	1165	2454	6429
Panel B: Unweighted Estimates					
Estimates Using NCHS Data, County-level Analysis					
Facilities Last Year	-0.0016	-0.0021***	-0.0022***	-0.0022***	-0.0012
	(0.0011)	(0.0005)	(0.0005)	(0.0006)	(0.0008)
N	168	504	1243	2679	6921
Estimates Using UCR Data, Agency-level Analysis					
Facilities Last Year	-0.0026	-0.0001	-0.0012	-0.0013	-0.0021***
	(0.0018)	(0.0014)	(0.0010)	(0.0011)	(0.0005)
N	144	421	1165	2454	6429

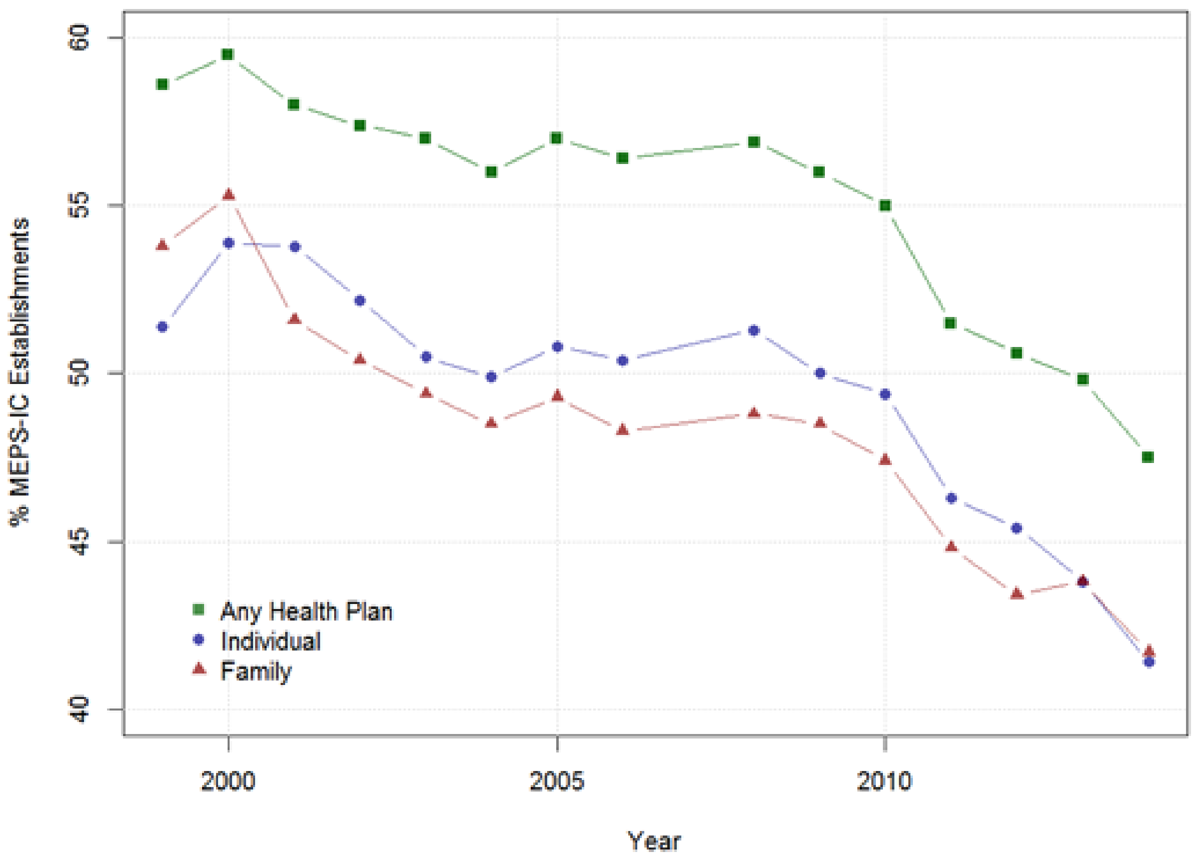
Notes: Outcomes are in log rates. All estimates control for county fixed effects, year fixed effects, state-by-year fixed effects, demographic controls, economic controls, and the size of the police force in the area. Each column corresponds to the number of counties (NCHS data) or agencies (UCR data) used within the estimating sample based on population. Note that the inclusion of state-by-year fixed effects requires multiple counties/agencies within each state. Thus, the actual number of counties/agencies is reduced further. For the NCHS data, the actual number of counties used were 12, 36, 89, 191, and 495, respectively. For the UCR data, the actual number of agencies used were 11, 33, 92, 193, and 496, respectively. Robust standard errors two-way clustered at the county and year levels are shown in parentheses. In Panel A the regressions are weighted by county population when using the NCHS Mortality data and are weighted by agency population coverage when using the UCR Offenses Known data.
*, **, and *** indicate statistical significance at the ten, five, and one percent levels, respectively.

APPENDIX B

APPENDIX: UNION, PREMIUM COST, AND THE PROVISION OF HEALTH BENEFITS

B.1 Appendix Figures

Figure B.1: The Declining Provision of Health Plan among US Private Establishments, 1999-2014



Notes: The MEPS-IC shifted from a retrospective survey that collected data about the previous year to a current survey that asked questions about current health plans. As such, we do not have any observations for 2007.

B.2 Appendix Tables

Table B.1: Summary Statistics of the Analytical Sample

	Mean	S.D.	Note	Source
Outcome Variable				
Health Plan	0.45	0.5	1= providing any health insurance; 0 otherwise	MEPS-IC
Worker's Bargaining Power				
% Union (Estab-level)	1.76	12.03	Percent of employees at the establishment that are union members	MEPS-IC
% Union (State-level)	0.14	0.07	Percent of the employees in the state that are members of a union or covered by a union	CPS
Right-to-work	0.38	0.49	1= having right-to-work laws; 0 otherwise	Public
Premium Cost				
Cost per Worker	6431	17160	Total establishment contribution to all health plans divided by the number of employees at each establishment	MEPS-IC, LBD
Firm Characteristics				
Founding Period			We use the earliest reported year for a firm between the MEPS-IC, LBD, and BR and difference that into the survey year	MEPS-IC, LBD, BR
(1980 or Before)	0.18	0.38		
1981-1990	0.2	0.4		
1991-2000	0.37	0.48		
2001 or After	0.25	0.43		
Firm Age	13.33	10.1		MEPS-IC, LBD, BR
Firm Employment Size	83.12	2641	Total workers for the firm, logged in the analysis	MEPS-IC
Multi-unit Firm	0.06	0.24	1= the firm has more than one establishment; 0 otherwise	MEPS-IC
Establishment Characteristics				
Pay per Worker	3363	79660	Total salaries paid divided by the number of workers	LBD
% Part-Time	27.91	35.62	Percent of employees working part-time	MEPS-IC
% Female	45.11	36.67	Percent of employees that are women	MEPS-IC
% Aged 50+	28.07	33.16	Percent of employees aged 50 or older	MEPS-IC
% Low-wage	31.53	39.24	Percent of employees earning at or below the 25th percentile	MEPS-IC
Non-profit	0.09	0.28	1= non-profit; 0 otherwise	
State Characteristics				
% Unemployment	0.07	0.02	Percent of the state population that is unemployed	CPS
% Below Poverty	0.13	0.03	Percent of the state population under the federal poverty lines	CPS

Notes: Statistics are based on 240,000 establishment-year observations for all variables except for the costs per worker which is based on 120,000 establishment-year observations. MEPS-IC: Medical Expenditure Panel Survey-Insurance Component; LBD: Longitudinal Business Database: BR: Business Registrar; CPS: Current Population Survey

Table B.2: Additive Logistic Regression Predicting the Provision of Health Insurance

	(1)		(2)		(3)		(4)	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
Worker's Bargaining Power								
% Union (Estab-level)	.0043***	0.0008	.0040***	0.0008	.0034***	0.0008	.0033***	0.0009
% Union (State-level)	.5885*	0.304	.6150**	0.3051	.6938**	0.322	.7619**	0.3655
Right-to-work	-.1563***	0.0483	-.1609***	0.0512	-.1757***	0.065	-.2027**	0.085
Financial Factors								
Cost per Worker*100%			-.0510***	0.0021				
Cost per Worker*110%					-.2031***	0.0046		
Cost per Worker*120%							-.3739***	0.0126
Firm Characteristics								
Founding Period								
1981-1990	.0804**	0.0399	.0823**	0.0407	.0884**	0.0431	.0998**	0.0451
1991-2000	.2123***	0.0649	.2152***	0.0664	.2251***	0.0704	.2430***	0.0735
2001 or After	.1679**	0.0758	.1706**	0.0772	.1801**	0.0807	.1992**	0.0836
Firm Age	.0403***	0.0041	.0406***	0.0041	.0416***	0.0041	.0428***	0.0042
Firm Age-squared	-.0004***	0.0001	-.0004***	0.0001	-.0004***	0.0001	-.0004***	0.0001
Ln(Firm Emp. Size)	-.0004***	0.0001	-.0004***	0.0001	-.0004***	0.0001	-.0004***	0.0001
Multi-unit Firm	-.5932***	0.0888	-.6236***	0.0897	-.7024***	0.0903	-.7620***	0.0919
Establishment Characteristics								
Ln(Pay per Worker)	.4086***	0.0164	.4159***	0.0163	.4380***	0.0159	.4607***	0.0156
% Part-Time	-.0160***	0.0005	-.0162***	0.0005	-.0167***	0.0005	-.0174***	0.0005
% Female	.0027***	0.0004	.0025***	0.0004	.0021***	0.0004	.0016***	0.0004
% Aged 50+	.0009***	0.0003	.0011***	0.0003	.0016***	0.0003	.0020***	0.0004
% Low-wage	-.0119***	0.0005	-.0120***	0.0005	-.0123***	0.0005	-.0128***	0.0005
Non-profit	.6464***	0.0364	.6567***	0.0364	.6865***	0.0371	.7197***	0.0384
State Characteristics								
% Unemployment	-0.7752	1.137	-0.6878	1.16	-0.4309	1.268	-0.1938	1.457
% Below Poverty	-1.052	0.8856	-1.019	0.8904	-0.8895	0.928	-0.7357	1.004
State Fixed-Effects	Yes		Yes		Yes		Yes	

Notes: Total observations for each regression is 240,000. Standard errors are clustered at the state level. The regressions are weighted using the sample weights provided by the MEPS-IC which are adjusted for non-response and post stratification. Stars indicate significance at the ten (*), five (**), and one (***) percent levels.

Table B.3: Multiplicative Logistic Regression Predicting the Provision of Health Insurance

	(1)		(2)		(3)	
	Coef.	S.E.	Coef.	S.E.	Coef.	S.E.
Worker's Bargaining Power						
% Union (Estab-level)	0.0025	0.0016	.0034***	0.0008	.0034***	0.0008
% Union (State-level)	.6940**	0.3221	-0.0863	0.4724	.6889**	0.3209
Right-to-work	-.1759***	0.065	-.1760***	0.0654	-0.1149	0.0978
Financial Factors						
Cost per Worker*110%	-.2034***	0.0049	-.2178***	0.0072	-.2000***	0.0062
Interaction Terms						
Cost*% Union (Estab-level)	0.0001	0.0002				
Cost*% Union (State-level)			.1018**	0.0479		
Cost*Right-to-work					-0.008	0.0074
State Fixed-Effects	Yes		Yes		Yes	

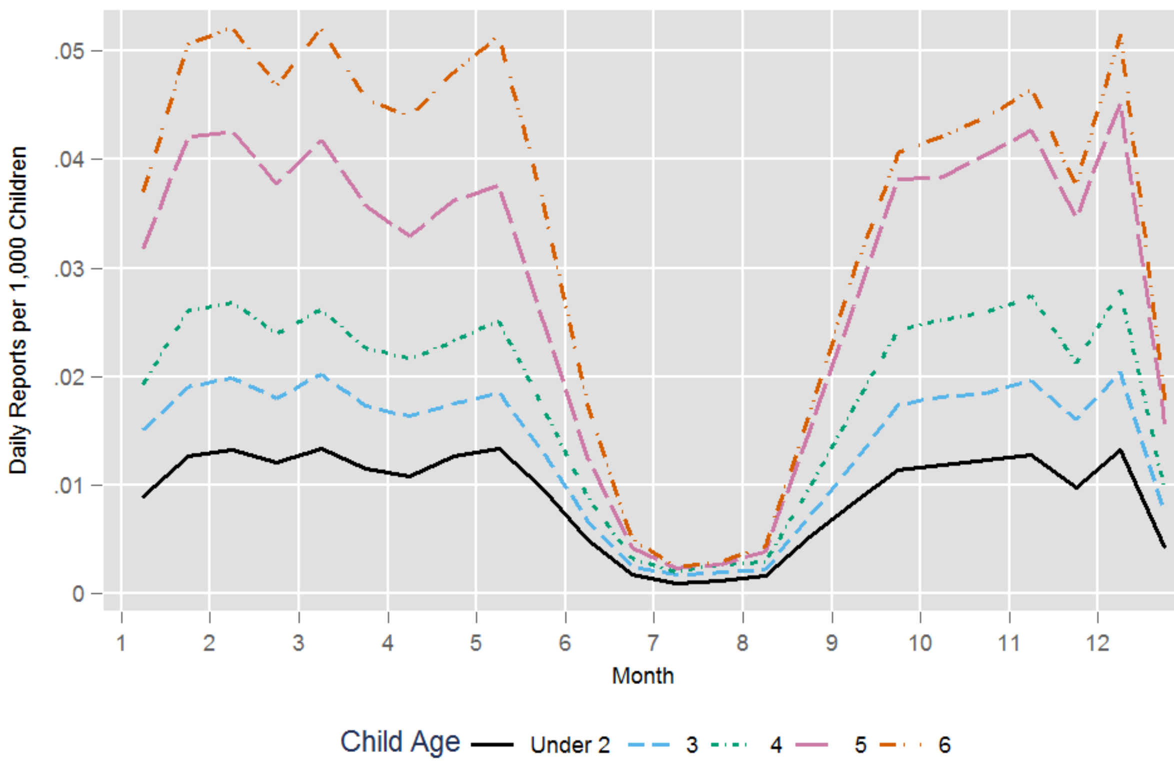
Notes: Total observations for each regression is 240,000. Robust standard errors are clustered at the state level. The regressions are weighted using the sample weights provided by the MEPS-IC which are adjusted for non-response and post stratification. Stars indicate significance at the ten (*), five (**), and one (***) percent levels. The model specification for each regression is that same as the previous table with additional cost variables included. Not all variable coefficient estimates are displayed.

APPENDIX C

APPENDIX: THE ROLE OF EDUCATORS IN THE REPORTING OF CHILD MALTREATMENT: CAUSAL EFFECTS OF SCHOOL ATTENDANCE

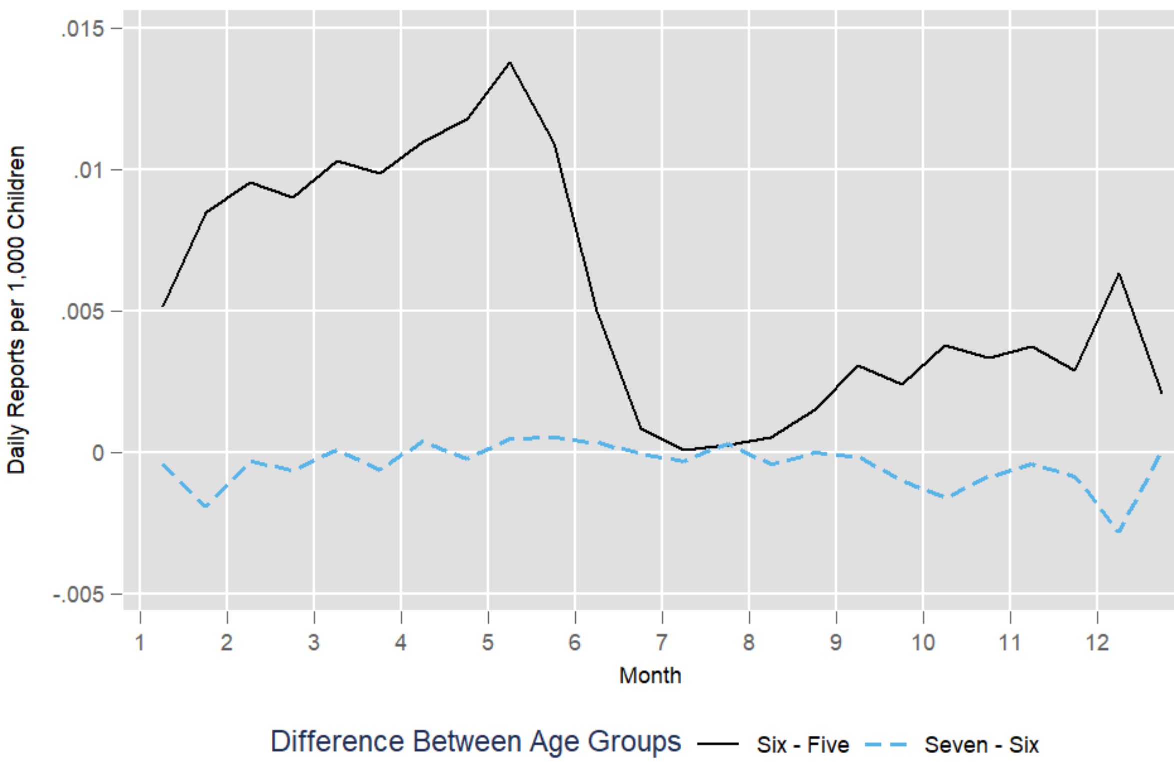
C.1 Appendix Figures

Figure C.1: Semimonthly Maltreatment Reports Initiated by Educators Averaged Across the Years 2002-2015



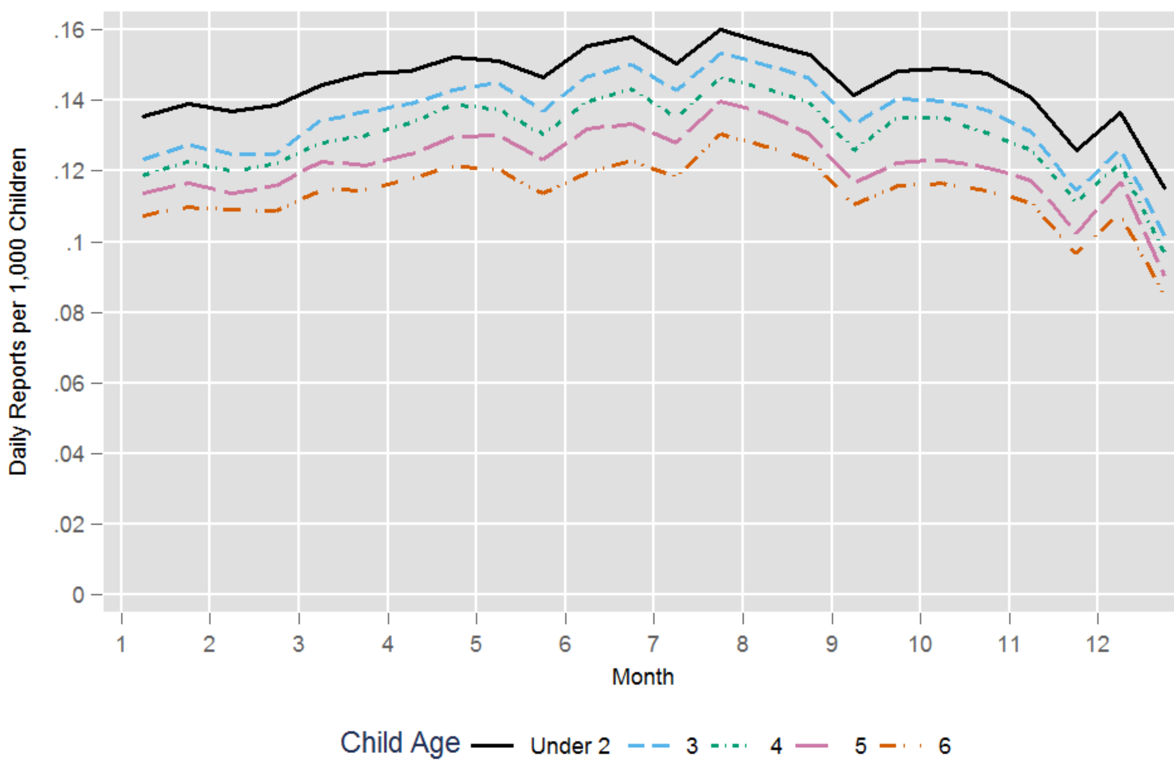
Notes: Maltreatment reports come from the National Child Abuse and Neglect Data System Child File, provided by the National Data Archieve on Child Abuse and Neglect at Cornell University. Child population counts come from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program. A child-report pairing counts as one report, thus, a report including two children counts as two reports.

Figure C.2: Semimonthly Differences in Reports Initiated by Educators Between Ages Averaged Across the Years 2002-2015



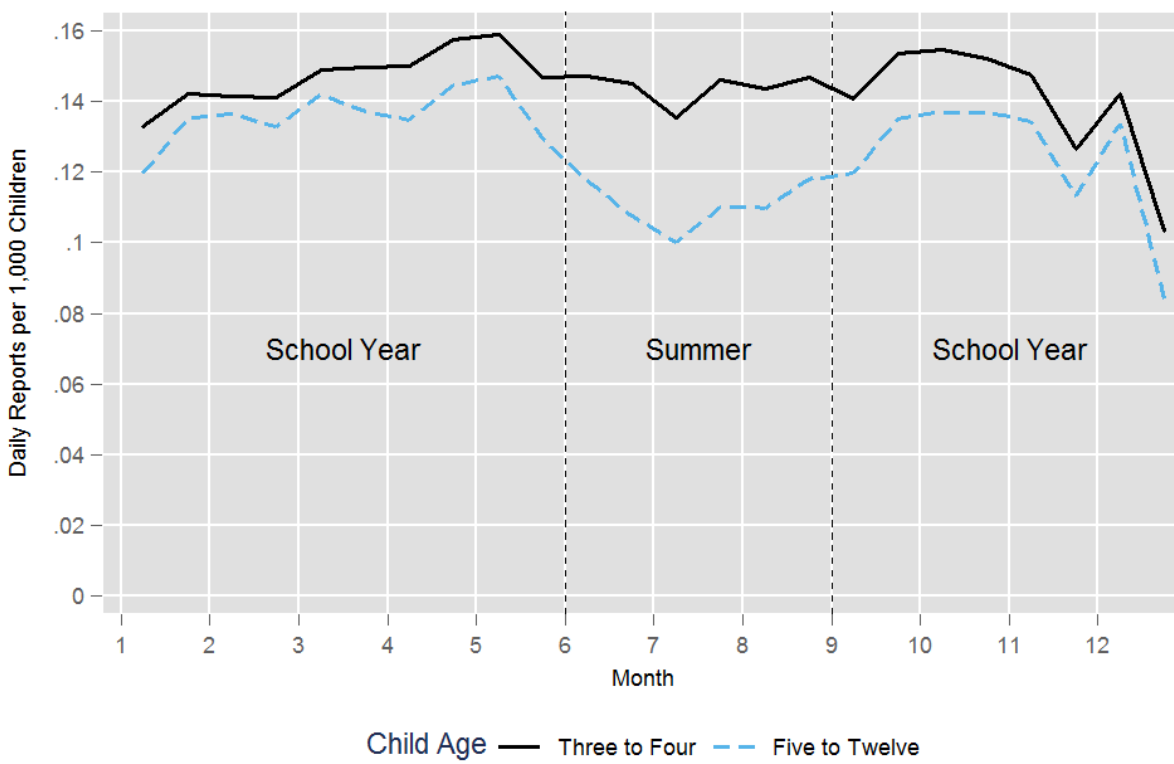
Notes: Six-Five shows the educator initiated reporting rates for children aged 6 differenced by the educator initiated reporting rates of children aged 5. Seven-Six is the same but for ages seven and six. Five-year-olds in September are more likely to be enrolled in school than five-year-olds in May. Maltreatment reports come from the National Child Abuse and Neglect Data System Child File, provided by the National Data Archieve on Child Abuse and Neglect at Cornell University. Child population counts come from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program. A child-report pairing counts as one report, thus, a report including two children counts as two reports.

Figure C.3: Semimonthly Maltreatment Reports Initiated by Non-Educators Averaged Across the Years 2002-2015



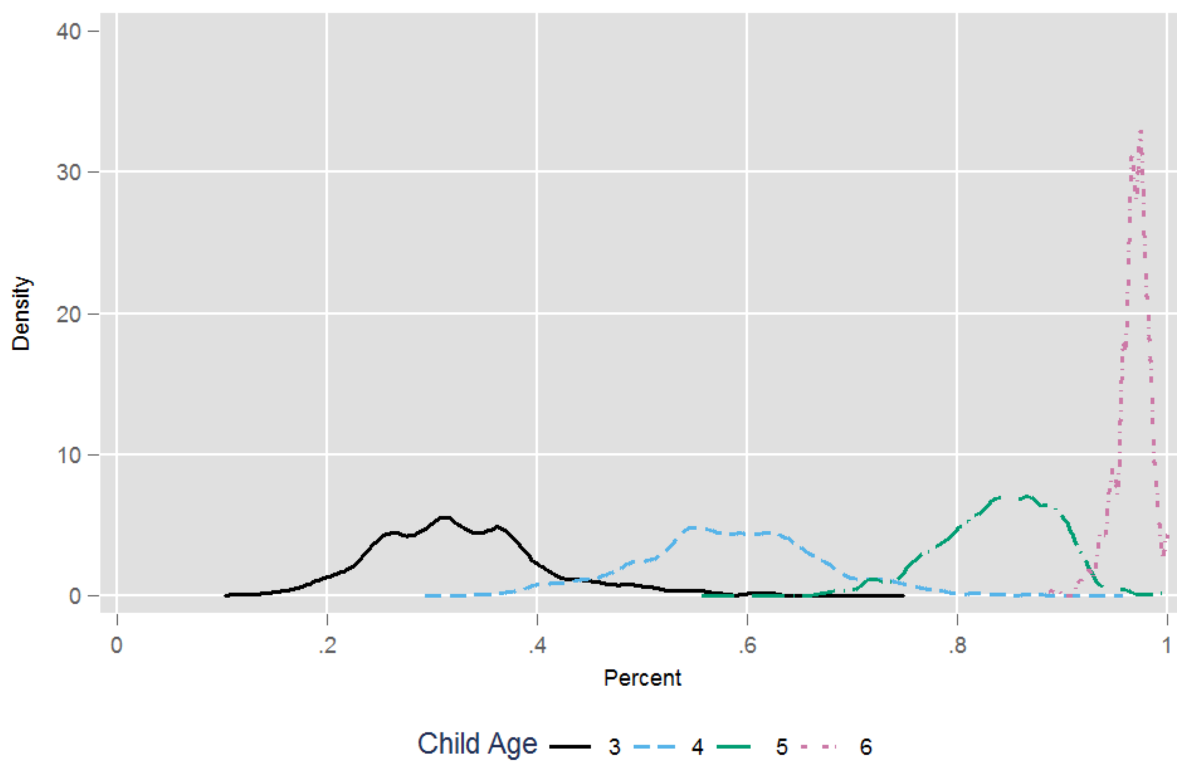
Notes: Maltreatment reports come from the National Child Abuse and Neglect Data System Child File, provided by the National Data Archieve on Child Abuse and Neglect at Cornell University. Child population counts come from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program. A child-report pairing counts as one report, thus, a report including two children counts as two reports.

Figure C.4: Semimonthly Maltreatment Reports Averaged Across the Years 2002-2015



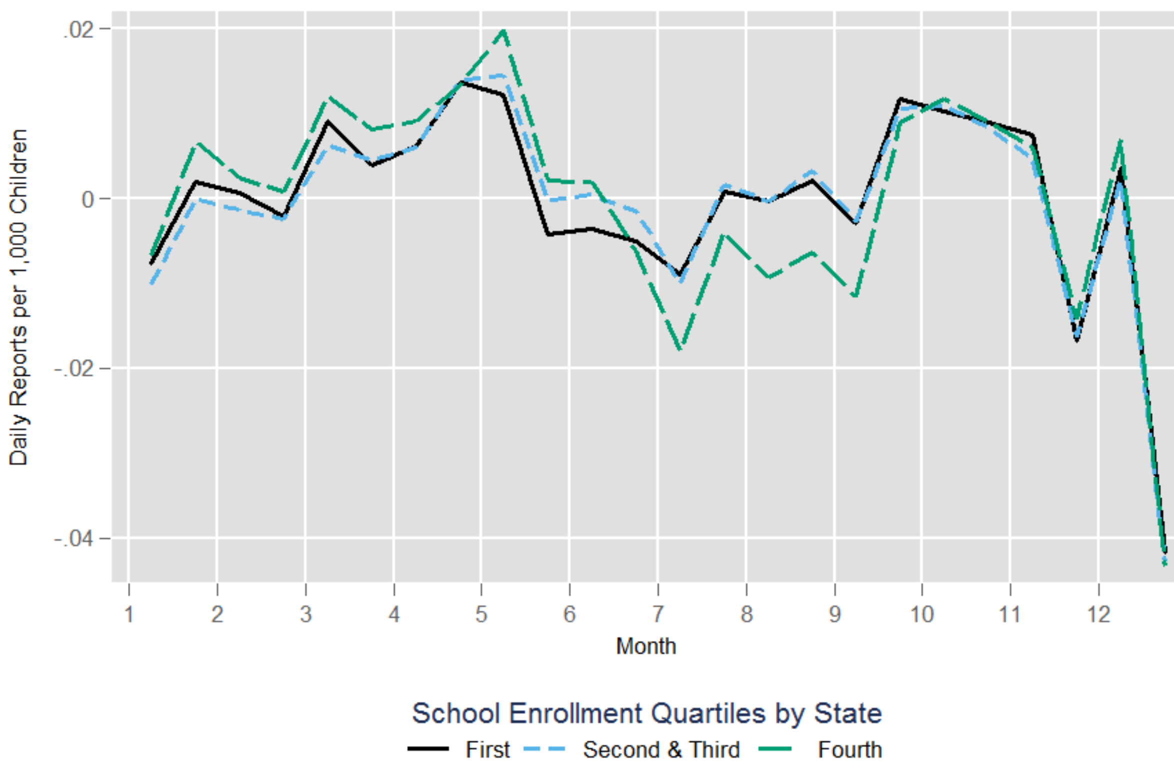
Notes: The dotted lines segment the summer months from the school year. Maltreatment reports come from the National Child Abuse and Neglect Data System Child File, provided by the National Data Archieve on Child Abuse and Neglect at Cornell University. Child population counts come from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program. A child-report pairing counts as one report, thus, a report including two children counts as two reports.

Figure C.5: School Enrollment Across the United States using State-Year Observations Averaged Across the Years 2002-2015



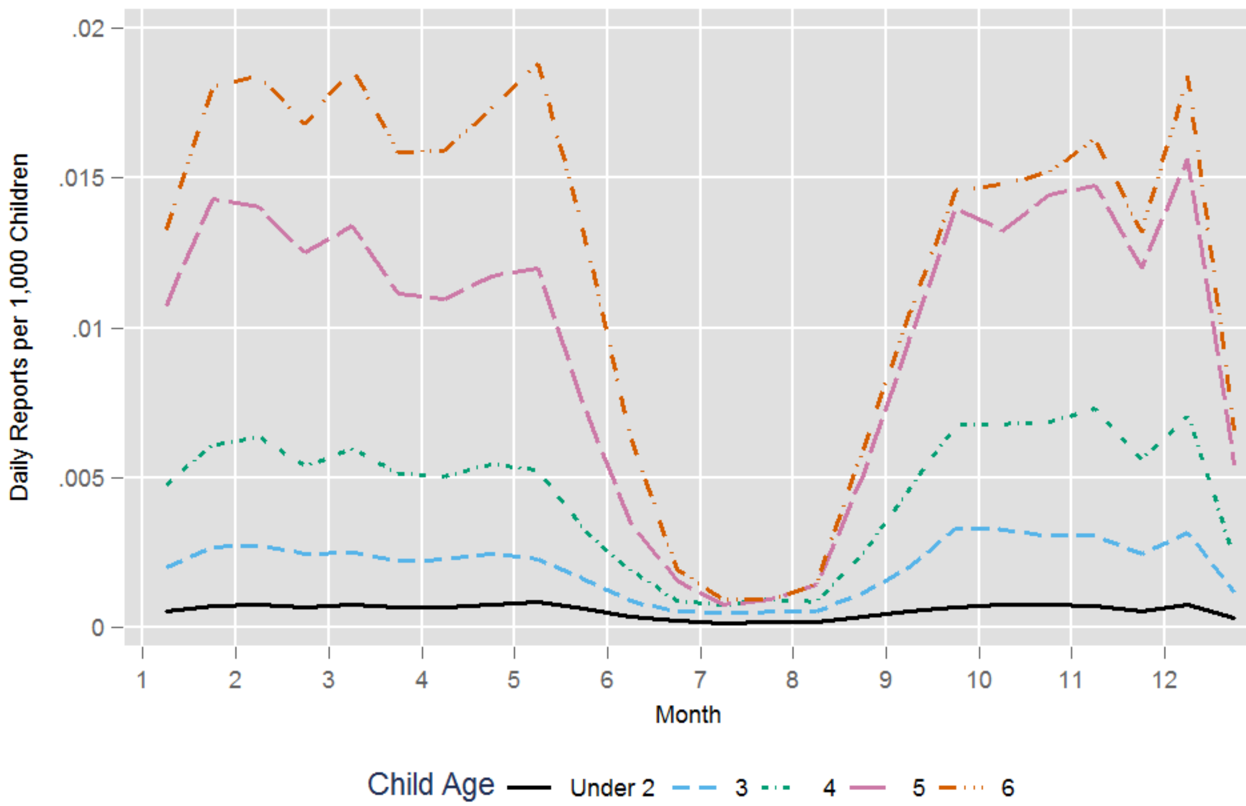
Notes: School enrollment data are based on the American Community Survey 1-year estimates provided by the Integrated Public Use Microdata Series (IPUMS-USA).

Figure C.6: Semimonthly Maltreatment Reports for Children 3-5 Averaged Across the Years 2002-2015 Demeaned



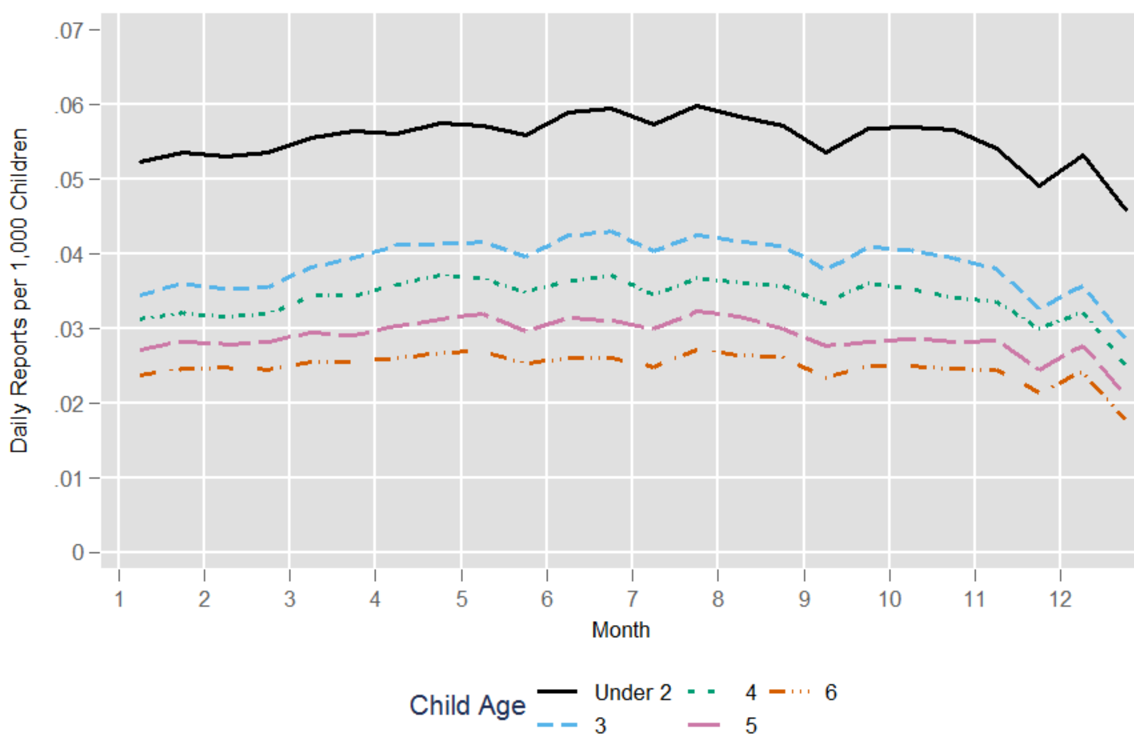
Notes: School enrollment data are based on the American Community Survey 1-year estimates provided by the Integrated Public Use Microdata Series (IPUMS-USA). Maltreatment reports come from the National Child Abuse and Neglect Data System Child File, provided by the National Data Archieve on Child Abuse and Neglect at Cornell University. Child population counts come from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program. A child-report pairing counts as one report, thus, a report including two children counts as two reports.

Figure C.7: Semimonthly Maltreatment Reports Initiated by Educators Averaged Across the Years 2002-2015 Using Only Single-Child Reports



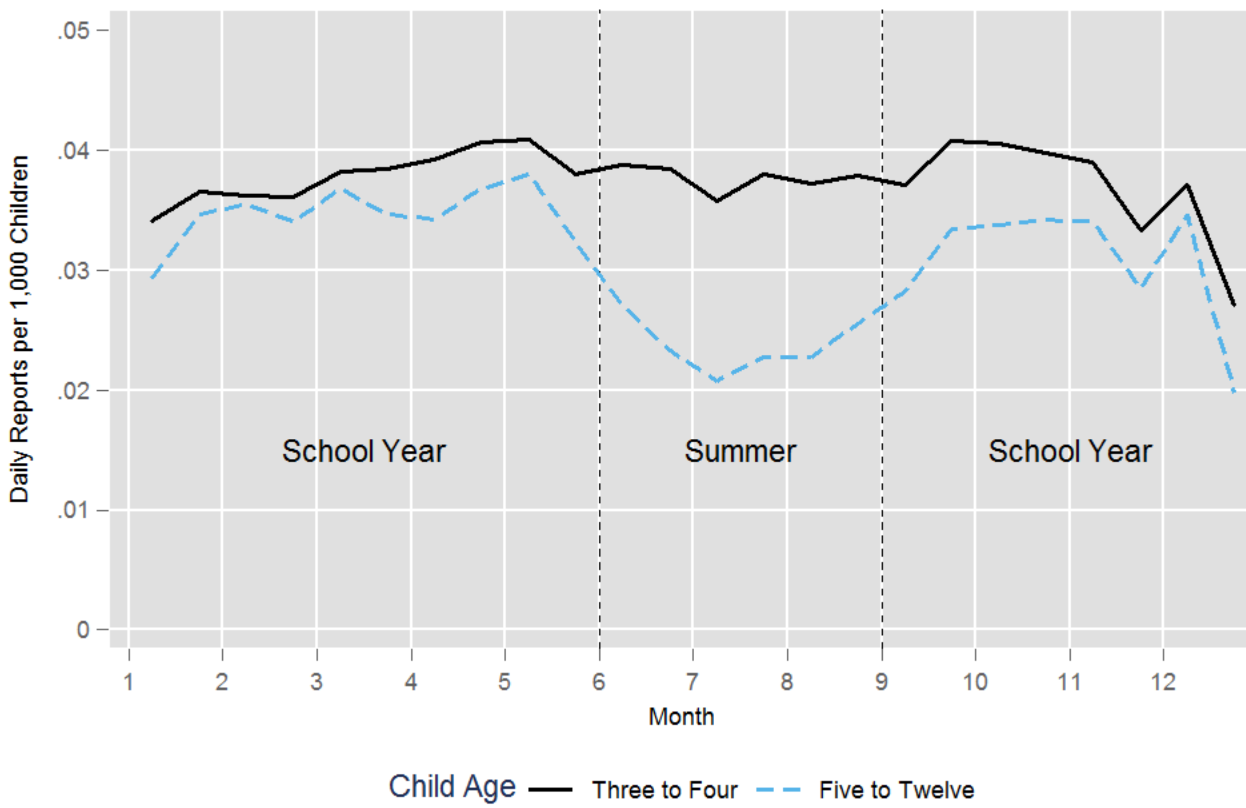
Notes: Maltreatment reports come from the National Child Abuse and Neglect Data System Child File, provided by the National Data Archieve on Child Abuse and Neglect at Cornell University. Child population counts come from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program. The population counts for this figure are the same as the previous figures as it is not possible to ascertain the relevant population for reports involving only one child. Single-child reports account for around 30% of the original sample.

Figure C.8: Semimonthly Maltreatment Reports Initiated by Non-Educators Averaged Across the Years 2002-2015 Using Only Single-Child Reports



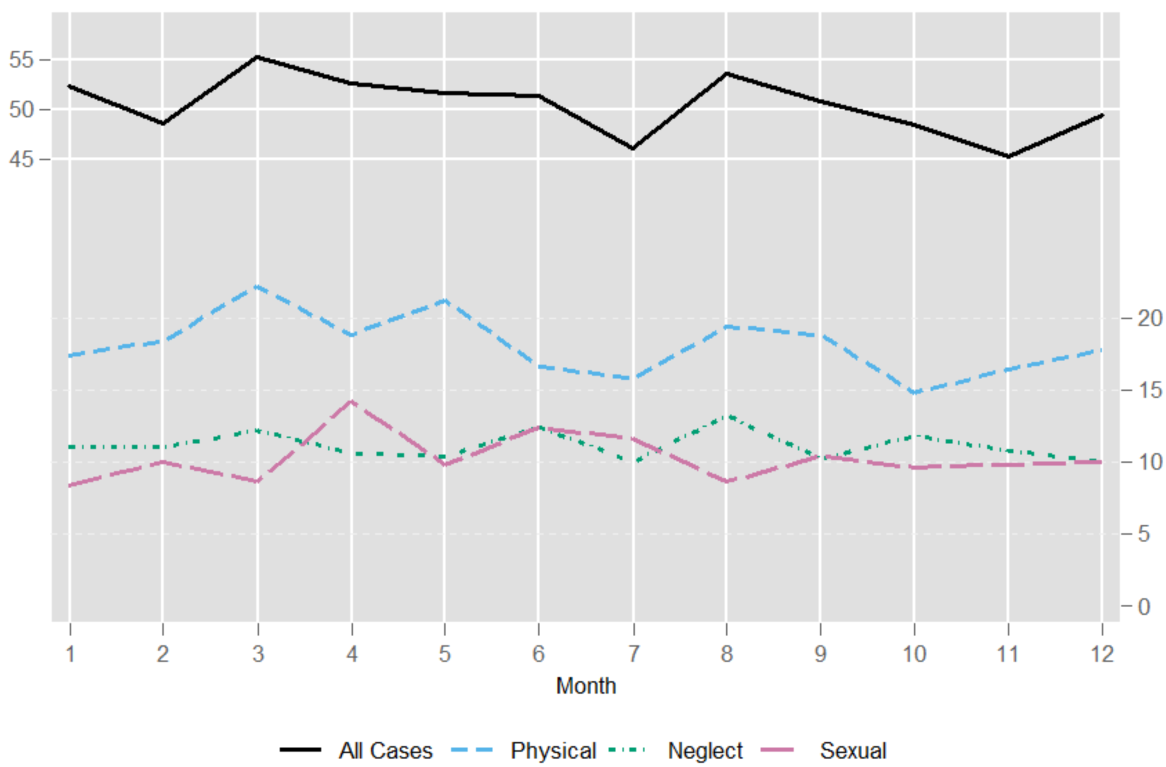
Notes: Maltreatment reports come from the National Child Abuse and Neglect Data System Child File, provided by the National Data Archieve on Child Abuse and Neglect at Cornell University. Child population counts come from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program. The population counts for this figure are the same as the previous figures as it is not possible to ascertain the relevant population for reports involving only one child. Single-child reports account for around 30% of the original sample.

Figure C.9: Semimonthly Maltreatment Reports Averaged Across the Years 2002-2015 Using Only Single-Child Reports



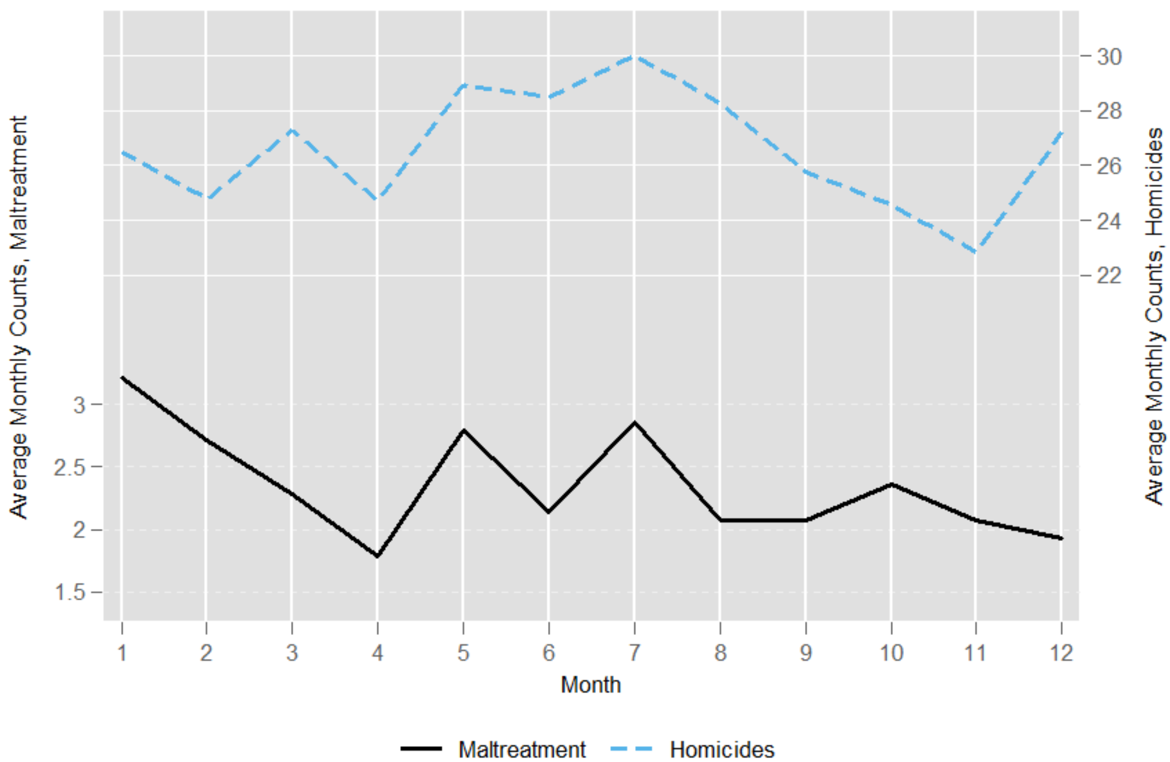
Notes: The dotted lines segment the summer months from the school year. Maltreatment reports come from the National Child Abuse and Neglect Data System Child File, provided by the National Data Archieve on Child Abuse and Neglect at Cornell University. Child population counts come from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program. The population counts for this figure are the same as the previous figures as it is not possible to ascertain the relevant population for reports involving only one child. Single-child reports account for around 30% of the original sample.

Figure C.10: Monthly Inpatient Reports of Maltreatment for Children Aged 3-12 Averaged Across the Years 2000, 2003, 2006, 2009 and 2012



Notes: All child maltreatment cases also include emotional abuse, cases marked as multiple types of abuse, and cases with external causes of injury codes marked as being caused by maltreatment. Inpatient reports come from the Healthcare Cost and Utilization Project (HCUP) Kids' Inpatient Database (KID).

Figure C.11: Monthly Mortalities for Children Aged 3-12 Averaged Across the Years 2002-2015, Homicides and Maltreatment Deaths



Note: Mortality data comes from the National Center for Health Statistics (NCHS) Multiple Cause of Death Data.

C.2 Appendix Tables

Table C.1: Summary Statistics, Percent of Maltreatment Report-Child Pairings by Reporter Type and Child Age

Child Age	3	4	5	6	7	8	9	10	11	12	3-12
Education Personnel	9.17	12.58	19.02	22.91	23.85	24.01	24.05	23.99	23.97	23.52	20.71
Social Services Personnel	10.44	10.26	9.83	9.78	9.83	9.93	10.04	10.19	10.39	10.77	10.15
Medical/Mental Health Personnel	11.20	10.72	10.29	10.15	10.26	10.43	10.61	10.78	11.11	11.74	10.73
Legal, Law Enforcement, Criminal Justice	20.20	18.60	16.75	15.84	15.50	15.43	15.54	15.65	15.98	16.53	16.60
Child Day/Substitute Care Provider	2.32	2.45	1.60	1.01	0.87	0.79	0.73	0.67	0.65	0.65	1.17
Alleged Victim	0.19	0.21	0.21	0.22	0.24	0.27	0.30	0.35	0.41	0.51	0.29
Parent	8.20	8.37	8.05	7.68	7.64	7.62	7.60	7.42	7.16	6.75	7.65
Other Relative, Friend, Neighbor	16.80	15.98	14.75	13.80	13.41	13.15	12.89	12.73	12.41	12.00	13.79
Alleged Perpetrator	0.08	0.08	0.08	0.07	0.07	0.07	0.07	0.08	0.08	0.08	0.08
Anonymous	12.05	11.64	10.97	10.53	10.36	10.30	10.20	10.12	9.81	9.33	10.53
Other	9.36	9.11	8.44	8.01	7.96	8.00	7.98	8.02	8.02	8.11	8.30
Unknown or Missing	7.50	7.44	7.25	7.16	7.10	7.09	7.07	7.11	7.09	7.05	7.18

Notes: Maltreatment reports come from the National Child Abuse and Neglect Data System Child File, provided by the National Data Archieve on Child Abuse and Neglect at Cornell University. Reports from non-educators consist of all reporting entities except education personnel and unknown or missing.

Table C.2: Summary Statistics, Child Maltreatment and Enrollment

	National Child Abuse and Negelct Data System			**American Community Survey**
	Daily child-report counts per 1,000 children			Percent of Yearly Enrollment
Child Age	All Reports	Reports from Educators	Reports From Non-Educators	
Three	0.161	0.014	0.137	0.329
	(0.081)	(0.016)	(0.073)	(0.088)
Four	0.160	0.018	0.131	0.584
	(0.080)	(0.019)	(0.071)	(0.092)
Five	0.162	0.029	0.123	0.839
	(0.082)	(0.026)	(0.068)	(0.057)
Six	0.159	0.035	0.116	0.967
	(0.084)	(0.030)	(0.067)	(0.016)
Seven	0.151	0.034	0.108	0.975
	(0.080)	(0.029)	(0.063)	(0.015)
Eight	0.142	0.032	0.102	0.978
	(0.077)	(0.028)	(0.061)	(0.0138)
Nine	0.134	0.030	0.095	0.980
	(0.075)	(0.027)	(0.060)	(0.013)
Ten	0.124	0.028	0.089	0.981
	(0.071)	(0.026)	(0.056)	(0.012)
Eleven	0.118	0.027	0.085	0.982
	(0.069)	(0.024)	(0.055)	(0.012)
Twelve	0.116	0.026	0.084	0.982
	(0.070)	(0.024)	(0.055)	(0.012)

Notes: Standard deviations are reported in parentheses. Maltreatment reports come from the National Child Abuse and Neglect Data System Child File, provided by the National Data Archieve on Child Abuse and Neglect at Cornell University. Child population counts come from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program. School enrollment data are based on the American Community Survey 1-year estimates provided by the Integrated Public Use Microdata Series (IPUMS-USA).

Table C.3: Estimated Effects of School Attendance on Maltreatment Reporting After Reaching Schooling Age

	(1) Ages 3-12	(2) Ages 3-12	(3) Ages 3-12	(4) Ages 4-6	(5) Ages 4-7	(6) Ages 4-8	(7) Ages 4-9	(8) Ages 3-6
School Attendance Starts at Age 5								
School Year*Age 5+	0.170***	0.170***	0.170***	0.100*	0.115**	0.124***	0.130***	0.122**
	(0.018)	(0.020)	(0.021)	(0.024)	(0.020)	(0.018)	(0.015)	(0.029)
School Attendance Starts at Age 6								
School Year*Age 6+	0.150***	0.150***	0.150***	0.090	0.099*	0.104**	0.108**	0.117**
	(0.029)	(0.030)	(0.031)	(0.032)	(0.031)	(0.030)	(0.029)	(0.032)
N	3300	3300	3300	990	1320	1650	1980	1320
Age FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month & Year FEs	No	Yes	-	-	-	-	-	-
Month*Year FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the age-year-semimonthly level. The outcome variable for each regression is total maltreatment reports. Robust standard errors are two-way clustered at the child age and quartly-by-year level. Significance levels at 1%, 5%, and 10% are indicated by ***, **, and *, respectively.

Table C.4: Estimated Effects of School Attendance on Maltreatment Reporting Using Enrollment

	(1) Ages 3-6	(2) Ages 3-6	(3) Ages 3-6	(4) Ages 3-6	(5) Ages 4-6	(6) Ages 4-7	(7) Ages 4-8	(8) Ages 4-9	(9) Age 3-12
All Reports									
School Year*Enroll%	0.283***	0.252***	0.283***	0.285***	0.346***	0.380***	0.400***	0.412***	0.369***
	(0.023)	(0.020)	(0.022)	(0.022)	(0.033)	(0.031)	(0.029)	(0.029)	(0.019)
Mean Enrollment %	68.0	68.0	68.0	68.0	79.7	84.2	86.9	88.7	86.0
N	64169	64169	64169	64169	48128	64172	80207	96244	160386
Age FEs	Yes	Yes	-	-	-	-	-	-	-
Month*Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	No	Yes	-	-	-	-	-	-	-
State*Age FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age*Year FEs	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the state-age-year-semimonthly level. The outcome variable for each regression is total maltreatment reports. Results are two-way clustered at the state and year level. Regressions on individual age groups were not significant. Significance levels at 1%, 5%, and 10% are indicated by ***, **, and *, respectively.

Table C.5: Estimated Effects of School Attendance Using Enrollment, Only Reports Involving One Child

	(1) Ages 3-6	(2) Ages 3-6	(3) Ages 3-6	(4) Ages 3-6	(5) Ages 4-6	(6) Ages 4-7	(7) Ages 4-8	(8) Ages 4-9	(9) Age 3-12
All Reports									
School Year*Enroll%	0.398***	0.494***	0.536***	0.541***	0.639***	0.696***	0.722***	0.735***	0.655***
	(0.034)	(0.033)	(0.030)	(0.029)	(0.042)	(0.043)	(0.040)	(0.039)	(0.027)
Mean Enrollment %	68.0	68.0	68.0	68.0	79.7	84.2	86.9	88.7	86.0
N	63530	63530	63530	63530	47618	63382	79109	94790	157866
Age FEs	Yes	Yes	-	-	-	-	-	-	-
Month*Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	No	Yes	-	-	-	-	-	-	-
State*Age FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age*Year FEs	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the state-age-year-semimonthly level. The outcome variable for each regression is total maltreatment reports. Results are clustered at the state-level. The population denominator for this table has not changed from the previous tables as it is not possible to ascertain the relevant population for reports involving only one child.

Table C.6: Estimated Effects of School Attendance Using Enrollment, Adjusted School Year

	(1) Ages 3-6	(2) Ages 3-6	(3) Ages 3-6	(4) Ages 3-6	(5) Ages 4-6	(6) Ages 4-7	(7) Ages 4-8	(8) Ages 4-9	(9) Age 3-12
All Reports									
Adjusted School Year*Enroll%	0.305***	0.248***	0.322***	0.325***	0.395***	0.440***	0.468***	0.484***	0.430***
	(0.044)	(0.029)	(0.024)	(0.024)	(0.038)	(0.032)	(0.032)	(0.034)	(0.022)
Mean Enrollment %	68.0	68.0	68.0	68.0	79.7	84.2	86.9	88.7	86.0
N	50230	50230	50230	50230	38046	50729	63406	76086	126786
Age FEs	Yes	Yes	-	-	-	-	-	-	-
Month*Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	No	Yes	-	-	-	-	-	-	-
State*Age FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age*Year FEs	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the state-age-year-semimonthly level. The outcome variable for each regression is total maltreatment reports. Results are clustered at the state-level. The population denominator for this table has not changed from the previous tables as it is not possible to ascertain the relevant population for reports involving only one child.

Table C.7: Estimated Effects of School Attendance on Maltreatment Reporting Using Enrollment

	(1) Ages 3-6	(2) Ages 3-6	(3) Ages 3-6	(4) Ages 3-6	(5) Ages 4-6	(6) Ages 4-7	(7) Ages 4-8	(8) Ages 4-9	(9) Age 3-12
Reports by Educators									
School Year*Enroll%	0.932***	0.843***	0.859***	0.860***	0.996***	1.023***	1.010***	1.003***	0.889***
	(0.023)	(0.068)	(0.076)	(0.072)	(0.098)	(0.096)	(0.089)	(0.085)	(0.061)
Reports by Non-Educators									
School Year*Enroll%	0.029	-0.003	0.016	0.018	0.016	0.030	0.042*	0.048**	0.067***
	(0.026)	(0.015)	(0.018)	(0.017)	(0.029)	(0.024)	(0.023)	(0.022)	(0.014)
Mean Enrollment %	68.0	68.0	68.0	68.0	79.7	84.2	86.9	88.7	86.0
N	64169	64169	64169	64169	48128	64172	80207	96244	160386
Age FEs	Yes	Yes	-	-	-	-	-	-	-
Month*Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	No	Yes	-	-	-	-	-	-	-
State*Age FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age*Year FEs	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the state-age-year-semimonthly level. The outcome variable for each regression is total maltreatment reports initiated by educators for the top panel and total maltreatment reports initiated by non-educators in the bottom panel. Results are two-way clustered at the state and year level. Regressions on individual age groups were not significant. Significance levels at 1%, 5%, and 10% are indicated by ***, **, and *, respectively.

Table C.8: Estimated Effects of School Attendance on Maltreatment Reporting Adjusting for Measurement Error, Children Ages 3-12

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	School Year*Age 5+				School Year*Enroll%			
Total Reports								
	0.170***	0.341***	0.205***	0.413***	0.369***	0.655***	0.430***	0.771***
	(0.021)	(0.042)	(0.026)	(0.050)	(0.019)	(0.027)	(0.022)	(0.033)
Reports by Educators								
	0.267***	0.551***	0.531***	1.088***	0.889***	2.569***	1.264***	4.040***
	(0.035)	(0.107)	(0.061)	(0.163)	-0.063	(0.281)	(0.093)	(0.376)
Reports by Non-Educators								
	0.028***	0.078***	0.029**	0.082***	0.067***	-0.029	0.067***	-0.051
	(0.010)	(0.017)	(0.012)	(0.021)	-0.014	(0.054)	(0.016)	(0.065)
N	3300	3300	2610	2610	160386	157866	124927	124927
Measurement Error Corrections								
Enrollment %	No	No	No	No	Yes	Yes	Yes	Yes
Single-Child Reports	No	Yes	No	Yes	No	Yes	No	Yes
Adjusted School Year	No	No	Yes	Yes	No	No	Yes	Yes

Notes: Observations are at the age-year-semimonthly level for the regressions in columns (1)–(4). These regressions estimate Equation 4.1 defining the schooling-age to be 5 and including age and month-by-year fixed effects. Observations are at the state-age-year-semimonthly level for the regressions in columns (5)–(8). These regressions estimate Equation 4.2 including month-by-year, state-by-age, and age-by-year fixed effects. Results are two-way clustered at the age and quarter-by-year level for columns (1)–(4). Results are two-way clustered at the state and year level for columns (5)–(8). Outcome variables for each panel from top to bottom are total maltreatment reports, total reports initiated by educators, and total reports initiated by non-educators. Significance levels at 1%, 5%, and 10% are indicated by ***, **, and *, respectively.

Table C.9: Estimated Effects of School Attendance on Maltreatment Reporting Using Enrollment

	(1) Ages 3-6	(2) Ages 3-6	(3) Ages 3-6	(4) Ages 3-6	(5) Ages 4-6	(6) Ages 4-7	(7) Ages 4-8	(8) Ages 4-9	(9) Age 3-12
Substantiated Reports									
School Year*Enroll%	0.386***	0.155***	0.203***	0.204***	0.267***	0.300***	0.317***	0.334***	0.295***
	(0.032)	(0.024)	(0.026)	(0.025)	(0.042)	(0.039)	(0.034)	(0.036)	(0.022)
Child Put Into Foster Care									
School Year*Enroll%	-0.355***	0.142***	0.175***	0.176***	0.227***	0.244***	0.237***	0.251***	0.216***
	(0.029)	(0.023)	(0.020)	(0.021)	(0.037)	(0.030)	(0.026)	(0.028)	(0.016)
Child Removed From Home									
School Year*Enroll%	-0.368***	0.143***	0.174***	0.175***	0.221***	0.246***	0.239***	0.254***	0.218***
	(0.032)	(0.024)	(0.021)	(0.021)	(0.037)	(0.028)	(0.025)	(0.028)	(0.016)
Mean Enrollment %	68.0	68.0	68.0	68.0	79.7	84.2	86.9	88.7	86.0
N	64169	64169	64169	64169	48128	64172	80207	96244	160386
Age FEs	Yes	Yes	-	-	-	-	-	-	-
Month*Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	No	Yes	-	-	-	-	-	-	-
State*Age FEs	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age*Year FEs	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Observations are at the state-age-year-semimonthly level. Outcome variables for each panel from top to bottom are total substantiated maltreatment reports, total reports that led to the child being placed into foster care, and total reports that led to the child being removed from the home. Results are two-way clustered at the state and year level. Regressions on individual age groups were not significant. Significance levels at 1%, 5%, and 10% are indicated by ***, **, and *, respectively.