Persistent Effects of Temporary Policies: Long-Term Impacts of Forced Child Care Center Closures on Parental Labor Market Outcomes

Lauren Russell* Chuxuan Sun†

December 15, 2022

Abstract

Forced child care center closures, mandated in 16 states, were temporary stop-gates designed to curb the spread of COVID-19. Although these policies remained in effect for, at most, three months, states that forced centers to close inadvertently caused a persistent negative supply shock in their child care sectors. Estimations of double-difference and triple-differences models using two years of CPS data reveal some evidence that this persistent shock to child care availability has decreased labor force participation rates and increased unemployment rates for parents of young children. Our results highlight the importance of child care availability in promoting equitable labor market outcomes.

JEL Codes: J2; J6.

Keywords: COVID-19; coronavirus; pandemic; child care availability; women's labor supply; women's employment

*Corresponding Author. Fels Institute of Government, University of Pennsylvania, 3814 Walnut Street, Philadelphia, PA. USA. Email: lrus@sas.upenn.edu.

†No affiliation. USA. Email: chuxuan@sas.upenn.edu

Declarations

Funding

No funding was received for conducting this study.

Conflicts of interest/Competing interests

The authors have no conflicts of interest to declare that are relevant to the content of this article.

Previous Work

An earlier version of the analysis focusing on shorter-run policy effects was published in Russell and Sun (2020). This publication was a pre-print meaning that we retained the copyright and reserved the right to later submit the article to established reviews. This 2022 version of the analysis is significantly updated relative to the initial 2020 version.

1 Introduction

During the early months of the COVID-19 pandemic, 42 states implemented stay-at-home orders and other forms of forced closures designed to curb the community spread of COVID-19 (Moreland et al., 2020). Sixteen of those states issued orders that forced child care centers to close in March or April of 2020. In this paper, we document that these child care closure policies which remained in effect for, at most, three months, have led to long-lasting differences in child care availability across states and had some downstream effects on parental labor market outcomes.

Using the U.S. Database of Child Care Closures which tracks year-over-year changes in in-person visits to more than 80,000 child care centers throughout the U.S. using mobile cell phone data, we find that 62% of child care centers were closed in April 2020. Closure rates in April, May, and June were only slightly greater in in states that mandated closures. However, by June, a much larger gap in closure rates opened up, despite the fact that child care centers were officially allowed to reopen in all states. Nearly two years after the pandemic began, states that implemented these temporary closure policies still have significantly higher closure rates, a fact we attribute to a persistent supply side shock and slow rates of recovery relative to other industries after a recession (Brown and Herbst, 2021). Our findings using center closure rates are consistent with other work that shows the childcare sector has still not recovered, and early childhood education job postings remain 4% lower than the pre-pandemic period (Ali, Herbst and Makridis, 2021).

We use this large and unprecedented shock to the U.S. child care sector to shed light on the role that a well-functioning child care system plays in promoting equitable labor market outcomes for parents of young children. Prior to the pandemic, 24% of children aged 5 and younger received center-based care from a day care center, preschool, prekindergarten or other early childhood program, and 60% participated at least one weekly in some type of non-parental care arrangement including home-based day cares or care with a relative (NCES 2016). Nine percent of the U.S. workforce has a child under age 6 (Dingel, Patterson and Vavra, 2020). Despite the potential importance of child care in the lives of many workers, relatively little recent research has directly investigated the causal link between child care and parental labor market outcomes.[1] The limited studies that do exist have focused on child care subsidies for low-income women or universal pre-K or kindergarten expansion. Most research indicates that child care subsidies increase employment among single mothers (Meyers, Heintz and Wolf, 2002; Tekin, 2007*a*,*b*; Herbst, 2010). In contrast, Cascio (2009) and Fitzpatrick (2010) find that universal pre-K or kindergarten has not had a major impact on the labor supply of most women in the United States, but policies under investigation in these studies led to only modest changes in overall child care usage as many parents substituted away from private child care.[2] Our study is unique in its ability to look at a major shock to overall child care availability that affects parents of all income levels.

Using Current Population Survey data, we show that the persistent shock to the child care industry in some states has had noticeable downstream effects on parents of children aged 5 and younger. We estimate difference-in-differences and triple-differences models that assess the impacts of the forced closure and class size limit policies themselves. In our study, we focus mainly on mothers in light of recent research showing that childcare challenges tend to matter more for mothers, though we also present results for fathers (Heggeness, 2020; Prados and Zamarro, 2021; Beauregard et al., 2021).

We find that these policies adversely affect mothers' labor market participation and employment; effects are larger in magnitude for non-married mothers and low-income mothers, though effects are estimated much more imprecisely more these smaller subgroups. We estimate that among all mothers closure policies increase unemployment rates by 1-4 percentage points during the months when they are in effect. Notably, adverse effects are detectable after forced closure policies themselves have been discontinued, which we attribute to the permanent contraction in childcare supply. Even once childcare centers are officially allowed to reopen, mothers in states that previously forced centers to close experience unemployment rates 1-3 percentage points higher than mothers in states that did not. We also find similar

[1]Recent research on the U.S. child care sector has mostly focused on the effects of care on children (Herbst, 2013; Herbst and Tekin, 2016; Felfe and Lalive, 2018), estimating parents' demand for quality (Gordon and Tekin, 2021), and costs (Herbst, 2018).

[2]Baker, Gruber and Milligan (2008) show that universal child care in Quebec did increase women's labor supply.

adverse impacts for fathers of young children.

2 Prior Literature

Our work contributes to two separate but related literatures: the more recent contemporary literature on the differential gender impacts of the COVID-19 pandemic and a somewhat older literature investigating the effects of childcare availability on women's labor supply more generally.

The economic downturn ushered in by the COVID-19 pandemic stands in stark contrast to previous recessions because it has disproportionately affected women (Alon et al., 2020*b*). Many have hypothesized that two primary factors are responsible for the dramatic effects on women's employment rates in the US: the concentration of women in sectors and occupations disproportionately impacted by the pandemic and changes in school and child care availability (Alon et al., 2020*b*; Dingel, Patterson and Vavra, 2020; Collins et al., 2020). Because mothers spend more time on childcare than fathers in two-parent households, and single-mother households are much more common than single father households, if school and child care center closures matter, they are likely to have a more substantial impact on mother's labor market outcomes (Alon et al., 2020*a*).

A number of papers have estimated the extent to which school and child care closures have affected women's employment rates during the pandemic with the evidence somewhat mixed. Couch, Fairlie and Xu (2022) estimate triple-differences models and attribute reductions in employment-to-population ratios and hours worked among mother's of school-age children to additional childcare responsibilities. By looking at average employment rates by sex-age-education-parental status cells, Furman, Kearney and Powell (2021) attempt to quantify what share of the overall decline in the female unemployment rate is due to childcare challenges specifically. In contrast to Couch, Fairlie and Xu (2022), they conclude that childcare challenges can explain very little of the aggregate decline in women's unemployment.

Studies that have directly investigated the impact of closure policies have focused mostly on public school closures for children elementary school aged or older. For instance, using a differences-in-differences approach, Heggeness (2020) estimates effects of early public school closures and stay-at-home orders and finds that mothers in early closure states were significantly more likely to have a job but not be working as a result of early shutdowns. She finds no immediate impact on labor market detachment or unemployment. A longer-term analysis by Prados and Zamarro (2021) uses double-difference and triple-differences models to assess the impacts of school closures and reopenings on parental labor market outcomes in the US. They conclude that the lack of school reopenings in some areas made it harder for parental employment to recover in Fall of 2020 and that "transitions out of employment for mothers who were working before the onset of the pandemic seem to be more persistent than for fathers in similar conditions." Beauregard et al. (2021) estimate impacts of school reopenings on labor market outcomes in parents in Canada and find that school reopenings increased employment rates of parents, especially single mothers and jobs that could not easily be done at home. Garcia and Cowan (2022) assess the impacts of both public school closures and child care center closures in the U.S. They conclude that most of the labor market impacts from K-12 closures were on reductions in hours worked per week and that both mothers and fathers' work was impacted. The estimated effects of childcare closures are negative for women but not statistically significant.

Other research has investigated whether pre-K and kindergarten availability makes women more likely to work. Exploiting birthday-based eligibility for universal pre-K, Fitzpatrick (2010) shows that universal pre-K availability has little effect on the labor supply of most women. Cascio (2009) finds that the introduction of kindergarten in the 1960s had no effect on labor supply of married mothers, but single mothers with no younger children were induced to enter the labor force. Gelbach (2002) adopts a quarter of birth instrumental variables strategy and concludes that kindergarten enrollment of the youngest child in the 1980s increased labor supply for both single and married mothers. None of these papers look at the effect of a sudden and unanticipated cutoff in access to care as even without public pre-K or kindergarten, families could still enroll children in private child care arrangements.

Mandatory child care center closures and increased regulations of centers implemented during the pandemic provide a unique opportunity to investigate the importance of childcare availability on parental labor force participation and employment when care is suddenly, and then permanently, disrupted.

3 Child Care Center Closure Policies

In March-April 2020, 16 states issued orders that forced childcare businesses to close, though most included an exemption which allowed centers to stay open if they served the children of essential workers. The other 34 states (plus DC) allowed childcare businesses to stay open. Among these 34 states, 15 imposed class size limits designed to increase social distancing and reduce the risk of COVID transmission within a classroom. For the purposes of our analysis, we classify a state as imposing class size limits if it required classes to consist of 15 or fewer students. Though mandates to close childcare centers were sometimes part of a more general stay-at-home order, state-imposed childcare center closures are not perfectly correlated with other types of closures such as public school closures (Heggeness, 2020); some states that closed public schools explicitly allowed child care centers to remain open (Hunt Institute, 2020; Food Industry Association, 2020; Child Care Aware of America, 2020).

Figure 1 identifies the states that ordered the closure of childcare businesses, states that allowed childcare centers to remain open without class size limits, and states that allowed childcare centers to remain open but imposed class size limits. Even though Alabama initially ordered childcare centers to close, this closure remained in effect only for one week between March 19, 2020 and March 27, 2020 at which point the state allowed centers to reopen with a class size limit of 11. Therefore, in our analysis we classify Alabama as a class size limit state rather than a mandated closure state.

Even in states that did not officially mandate stay-at-home orders or class size limits, childcare centers were deeply affected. Many centers voluntarily closed their doors due to health concerns, and others voluntarily decreased class sizes to allow for more social distancing. Some parents decided not to send children to childcare centers even if centers were open in their area (Quinton, 2020). Therefore, even in the states where childcare businesses technically had the ability to operate as normal during the early months of the pandemic, parents likely experienced decreased child care access, a fact we establish empirically in our analysis that follows.

4 Data Description

We use three data sources for our analysis: state-level information on child care center closure policies, the U.S. Database of Child Care Closures during COVID-19, and the Current Population Survey. Our data on childcare center closure policies, including dates of announcement/implementation and dates of reopenings, come primarily from government press releases, but we also used information from the Hunt Institute (2020), Food Industry Association (2020), and Child Care Aware of America (2020) to cross-reference this information. The Online Data Appendix reports specific language from these orders and a complete list of sources for each state.

The U.S. Database of Child Care Closures during COVID-19 tracks year-over-year changes in in-person visits to more than 80,000 childcare centers throughout the COVID-19 pandemic (Lee and Parolin, 2021). To account for potential seasonality in visits, Lee and Parolin (2021) calculate the year-over-year change in 2020-2022 by comparing the monthly total visits with the total visits during the same month in 2019. The data are based on anonymized, monthly tracking of 40 million mobile phone users provided by SafeGraph and include 85,328 childcare centers in 2,228 counties. Childcare centers were identified based on online information: either a maps-based application identified them as such or they had some other online presence that identified them as a childcare provider. Lee and Parolin (2021) estimate that the data cover about 78% of all licensed childcare centers in the U.S. Although they are unable to directly validate their childcare closure data, their validations of the SafeGraph data for K-12 public schools indicates that those data closely reflect patterns of school closures in the

U.S.

For labor market outcomes, we rely on the basic monthly files from the Current Population Survey, a monthly survey of about 60,000 households sponsored by the U.S. Census Bureau and the U.S. Bureau of Labor Statistics (Flood, Sarah and King, Miriam and Rodgers, Renae and Ruggles, Steven and Warren, J. Robert, 2020). Sampled households are in the survey for four consecutive months, are out for eight months, and then return for another four consecutive months before leaving the sample permanently. A new group of respondents starts in each calendar month at the same time another group completes its rotation. The survey is conducted on the 19th of each month and asks respondents questions about the previous week. Since all March 2020 closure or class size limit policies were implemented after March 12th, we code March 2020 responses in the CPS as untreated months. We also note that there is no staggered timing in closure policy adoption based on this CPS sampling; all states that implemented childcare closure policies did so between March 12 and April 12 and so parents of young children in those states are "treated" starting in our April 2020 CPS data.

Our microdata correspond to September 2019 to May 2022. We limit the sample to people aged 20-45, inclusive, to focus the analysis on the working-age population of childbearing age. We drop anyone living in group quarters or working in the armed forces. We drop New York from our sample because New York City had a childcare center closure policy while the rest of the state did not, so it is impossible to assign either treatment or control status to the state. We also drop any individuals whose reporting of age, sex, and race is inconsistent across the months where they report data to the CPS. Our primary analysis uses the subset of data corresponding to women with children aged 0 to 5 and women without any children.

5 Effect of Closure and Class Size Limit Policies on Child Care Center Closure Rates

To investigate the impact that official closure and class size limit policies had on states' childcare center closure rates, we estimate a state-level differences-in-differences regression:

$$\begin{aligned} ClosureRate_{st} = \alpha &+ \beta_1 ClosureInEffect + \beta_2 PostClosure \\ &+ \beta_3 LimitInEffect + \beta_4 PostLimit \\ &+ \gamma_s + \theta_t + \varepsilon_{st} \end{aligned} \tag{1}$$

where *ClosureRate* is the share of centers in the state who had at least a 50% decline in total monthly visits relative to the same month in 2019, our proxy for the share of centers that were closed. $ClosureInEffect$ is an indicator that equals 1 for state-months that had forced closure policies in effect; *PostClosure* is an indicator that equals 1 for states that previously had closure policies in months after these policies were lifted and no longer in effect; $LimitInEffect$ is an indicator that equals 1 for state-months with class size limit policies in effect; and *PostLimit* is an indicator that equals 1 for states which previously had class size limit policies in the months when these were lifted and no longer in effect. We include state fixed effects (γ_s), month-year fixed effects (θ_t), and report errors clustered at the state level. Under the assumption of parallel trends and policy exogeneity, β_1 represents the causal effect of an official closure policy on the child care center closure rate.

de Chaisemartin and D'Haultfoeuille (2020) show that two-way fixed effects models may deliver misleading estimates if policy impacts are heterogeneous across states or across months. We follow their advice and use the `twowayfeweights` Stata command to estimate weights attached to two-way fixed effects regressions throughout the paper (de Chaisemartin, D'Haultfoeuille and Deeb, 2019). Unfortunately, even with this information, it is difficult to determine the robustness of the regression estimates to heterogeneous treatment effects based on these results as the summary measures developed in de Chaisemartin and D'Haultfoeuille

(2020) do not apply to regressions with several treatment variables. de Chaisemartin and D'Haultfoeuille (2022) do propose an alternative estimator for two-way fixed effects models with multiple treatments; however, the authors point out that, "Our estimator's robustness may come at a high price in terms of external validity and statistical prevision," because it relies on matching a small number of switchers to control groups for whom all treatments remain the same and that had the same treatments as the switching group in period $t-1$. Even if we could implement their new estimator in our setting[3], the bias-variance tradeoff may not make it ex ante preferable to our standard two-way fixed effects specification. Therefore, throughout this paper, we have opted to present the results from standard two-way fixed effects specifications while noting whether there are average treatment effects on the treated (ATTs) that receive negative weight.

Two-way fixed effect results shown in Table 1 indicate that although a significant share of centers closed in non-mandated closure states due to direct effects of the pandemic, the implementation of an official closure policy increased closure rates.[4] The average closure rate 8 percentage points higher for states that implemented closure policies when these policies were in effect compared to states without them (based on our preferred cutoff of at least a 50% decline in total visits relative to the same month in 2019). Surprisingly, elevated closure rates persisted after closure policies had been discontinued; column 1 indicates that post-closure policy, these states had closure rates 7 percentage points above those who had never implemented them. Alternative proxies for center closure rates are shown in columns 2-4 and also reveal a statistically significant increase in closure from forced closure policies both in the short-term (when they were in effect) and in the long-term (once they were discontinued). By contrast, we do not detect a statistically significant effect of class size limit policies on closure rates, though some of the confidence intervals are somewhat wide, and we cannot rule out increases in actual closure rates of as large as 8 percentage points.

One threat to policy exogeneity is if state childcare center closure or limit policies were

[3]The authors have not yet released a Stata or R package to implement the proposed estimator.

[4]Using the Stata command `twowayfeweights`, we find there are only positive weights for the weighted sum of the first term ($\hat{\beta}_1$). There are some negative weights in the computation of the other three treatment indicators where the sum of negative weights is -0.174, -0.186, and -0.109 for $\hat{\beta}_2$, $\hat{\beta}_3$, and $\hat{\beta}_4$ respectively.

enacted in direct response to temporal changes in COVID infections or risk within the state. In this case, we would expect the $\hat{\beta}$s to be upward biased. In Appendix A, we augment equation 1 to include controls at the month-state level for a lag and current data on confirmed COVID cases per 1000 people in the state or deaths from COVID per 1000 people (Dong, Du and Gardner, 2020, 2022). Although these measures of COVID risk are correlated with state-month closure rates, our $\hat{\beta}$ estimates are robust to their inclusion. This suggests that the decision to implement an official childcare center closure or limit policy was determined by factors largely orthogonal to actual COVID risk in the state, which provides some support for the policy exogeneity assumption in this setting. Prior research on the diffusion of stay-at-home orders has shown, for example, that the governor's political party affiliation was a key factor in adopting stay-at-home orders (Patterson, 2022).

The event study plot in Figure 2, which uses childcare center closure rate data from January 2020 to February 2022, shows how the effect of these policies has evolved over time.[5] Figure 2 shows that there are both short-term and long-term effects of these policies. By one month after an official closure policy, the actual childcare center closure rate is about 5 percentage points higher than it otherwise would have been. This effect increases to between 5 and 10 percentage points in the months that follow. Actual childcare center closure rates remain elevated up to 20 months later. Since closure policies lasted at most three months, this means that more than a year and a half after closure orders were rescinded, states that implemented these policies have had elevated closure rates relative to states that did not implement these policies.

Consistent with these data, a survey by the National Association for the Education of Young Children found that nationally, 18% of child care centers were closed in July 2020 as a result of the pandemic, even though all states had officially allowed child care centers to reopen by that time, which is consistent with this type of permanent supply side response (National Association for the Education of Young Children, 2020*b*). The survey also pre-

[5]The Stata command `xtevent` is used to construct this plot (Freyaldenhoven et al., 2021). Since this command can only accommodate one treatment, we create two samples: a sample of control and closure states and another sample of control and class size limit states. The results from closure effects appear in the top figure while results from class size limit effects appear in the bottom figure.

dicted that closures would become more widespread in the months that followed. Forty percent of respondents said they were certain that they would close permanently within the year without additional public assistance (National Association for the Education of Young Children, 2020*b*). Corroborating these predictions, Bureau of Labor Statistics data indicate that there were 166,800 fewer childcare workers in December 2020 compared to December 2019 (Mongeau, 2021).

Early financial pressures directly caused by mandated closure probably caused some centers to close their doors permanently. Even in normal times, daycare centers operate on razor-thin profit margins, typically less than 1% (Grunewald and Davies, 2011), and labor costs constitute 60-70% of expenses at most centers (Grunewald and Davies, 2011). When some centers continued to pay staff, even when centers were closed, most could not survive long-term. A survey of 6,000 childcare workers by the National Association for the Education of Young Children in November 2020 found that 56% of childcare centers were losing money, and 42% of workers surveyed reported taking on debt for their programs on their own personal credit cards (National Association for the Education of Young Children, 2020*a*). Even if programs could meet budget shortfalls for a month or two, perhaps by drawing on reserves, taking on debt, or requesting that some parents continue to pay tuition when their children were not enrolled, it is unlikely they could do so in the long-term, leading to permanent closures and a contraction in the supply of child care.

6 Effects of Child Care Closures on Labor Market Outcomes

Next, we turn our attention to the downstream effects of childcare closures on parental labor market outcomes. To investigate the downstream impact of these policy-induced childcare center closures, we start by estimating a difference-in-differences regression using women with children aged 5 and younger. We omit women with only older children from the analysis because these mothers also experienced changing family obligations as many schools and universities were closed or switched to remote learning formats. Our difference-in-differences model takes the form:

$$
\begin{aligned}
y_{ist} = {} & \beta_1 ClosureInEffect_{st} + \beta_2 PostClosure_{st} \\
& + \beta_3 LimitInEffect_{st} + \beta_4 PostLimit_{st} \\
& + \gamma_s + \theta_t + \mathbf{X}_{ist}\delta + \omega_i + \varepsilon_{ist}
\end{aligned}
\tag{2}
$$

where y_{ist} is a labor market outcome for woman i in state s and month t. Our four treatment indicators capture both periods when policies were in effect ($ClosureInEffect$ and $LimitInEffect$) as well as time periods when these policies were no longer in effect ($PostClosure$ and $PostLimit$). The matrix $\mathbf{X}_{ipst}$ includes controls for age, marital status, and whether there is another adult in the household. Standard errors are clustered at the state level.

We can also augment 2 to include include person fixed effects (ω_i) so that comparisons are "within-worker." However, if we do so, the 4/8/4 rotational structure of the CPS implies we can only estimate impacts up to 14 months post-policy because the cohort entering the CPS in March 2020 (the period immediately prior to first closure or class size limit policy adoption) appears only through May 2021. Thus, we are able to estimate longer term impacts only in specifications that do not include these worker fixed effects.

The identifying assumption for this model is that outcomes for women with young children would have evolved similarly in states that did and did not implement these policies if not for these policies. We can relax this assumption with a triple-differences specification that adds data for women without children and uses not only cross-state variation in which states implemented policies and cross-time variation in when policies were in place but also adds cross-worker variation in whether a woman had young children who could potentially need child care:[6]

$$
\begin{aligned}
y_{iopst} = {} & \delta_1 ClosureInEffect \times Parent_{pst} \\
& + \delta_2 PostClosure \times Parent_{pst} \\
& + \delta_3 LimitInEffect \times Parent_{pst} \\
& + \delta_4 PostLimit \times Parent_{pst} \\
& + \gamma_{ost} + \theta_{pt} + \mu_{ps} + X_{ipst}\delta + \omega_i + \varepsilon_{iopst}
\end{aligned}
\tag{3}
$$

where y_{iopst} is a labor market outcome for woman i in occupation category o in state s and month t who either is or is not a parent (p) of a child aged 0 to 5. Because we omit parents of older children from the analysis sample, any observation that is not a parent of a child aged 0 to 5 is a non-parent. We control for state-occupation category specific shocks that vary over time γ_{ost} and include interactions for parent and time effects θ_{pt} and parent and state effects μ_{ps}. As before, the matrix X_{ipst} includes controls for age, marital status, and whether there is another adult in the household.

Our set of four treatment indicators are defined as follows: $ClosureInEffect \times Parent$ equals 1 if person i was a parent of a young child in state s where child care center closures were mandated in month t. $PostClosure \times Parent$ equals 1 for parents in post-closure months once centers were allowed to reopen. Similarly, $LimitInEffect \times Parent$ equals 1 if person i was a parent of a young child in state s where child care centers were subject to class size limits (and never closures) in month t. $PostLimit \times Parent$ equals 1 in months after class size limits were discontinued. We cluster standard errors at the state level.

The identifying assumption for our triple-differences estimator is that there is no contemporaneous shock that differentially affects the outcomes of the treatment group (parents with young children) compared to the control group (people without children) in the same occupation category-state-months. As was the case with the differences-in-differences specification, the panel structure of the CPS also allows us to include person fixed effects (ω_i)

[6]For a derivation of the triple-differences estimator and a complete discussion of its identifying assumptions, we refer readers to Olden and Møen (2020).

to obtain "within-worker" estimates, though their inclusion again limits the time window through which effects of closure policies can be estimated to May 2021.

6.1 Effects for Mothers

We start by estimating equation 2 and 3 with and without individual worker fixed effects on the full sample of women where mothers are defined as those who have a child aged 0 to 5. Table 2 reports the results from these four specifications on our two main outcomes of interest: an indicator for not being in the labor force and an indicator for a woman being unemployed, conditional on being in the labor force.[7] We estimate that when closures were in effect, they increased the probability that a mother was not in the labor force by 1-2 percentage points and increased the probability that she was unemployed, conditional on being in the labor force, by 1-4 percentage points. The evidence on whether adverse labor market impacts persist in the period when closures are discontinued is less robust across specifications, but specifications with worker fixed effects indicate that even in the post-policy period, unemployment rates are about 3 percentage points higher for mothers with young children.

The time path of treatment effects could explain why we estimate larger and statistically significant impacts in the DD and DDD models with worker fixed effects as opposed to those without. Figures 3 to 5 display event study plots for the impact of closure and class size limits using the DDD model without worker fixed effects for the full sample of women and various subgroups. Estimates in these plots are very imprecise but suggest that increases in unemployment for women tend occur in the first year or so after policies were implemented and fade out afterwards. This result coincides with other work showing that although female unemployment rates were significantly greater than male unemployment rates at the beginning of the pandemic, by early 2021 female unemployment had recovered and reached parity (Lee, Park and Shin, 2021). The post closure policy effect in models without worker fixed

[7] In our estimation of specification 1, no ATTs for $ClosureInEffect$ receive negative weight. For the other three treatments, we do have ATTs that receive negative weight, leaving open the possibility of bias. For specification 2, some ATTs for all four treatments receive negative weight. The Stata command `twowayfeweights` is not adapted to a triple-differences setting, so we are unable to estimate weights for treatment effects in those specifications.

effects implicitly averages a larger initial effect and the fade out. The models without worker fixed effects do not use participants from later CPS cohorts to identify the post closure policy effect, so those models are picking up only the larger initial impact.[8]

There is mixed evidence of adverse labor market impacts of class size limits for mothers. For the period when class size limits were in effect, estimated impacts on "not in the labor force" are close to zero and not statistically significant in the two DDD specifications, but they are positive and statistically significant in the two DD specifications. Estimated impacts on unemployment are positive and statistically significant in the two specifications with worker fixed effects. There is little evidence for any impacts after class size limit policies are discontinued.

6.2 Heterogeneity

Lim and Zabek (2022) find that COVID-19 induced labor force exists were much larger for women with children under 6 and lower-income women. Although differences in education and job characteristics can account for some of the differential effect, childcare interruptions could have played a key role as well. In this section, we estimate the effects of child care center closure policies on labor market outcomes for demographic groups that may be especially vulnerable to childcare losses.

Single (non-married) mothers and low-income mothers may be particularly responsive to changes in childcare availability (Berger and Black, 1992; Bateman and Ross, 2020; Beauregard et al., 2021). Single mothers have less flexibility to adjust to lack of care because there is often no other adult who can share childcare responsibilities. For Table 3, we re-estimate our models with the treatment group consisting only of non-married women. Compared to the full sample of all mothers of young children, our results are much more imprecise which makes drawing definitive conclusions difficult, but point estimates are generally larger in

[8] Theoretically, it's also possible that shifts in labor force composition could explain the difference in estimated effect. We think this is unlikely because event study plots for models with and without worker fixed effects show evidence of parallel trends. Moreover, if we estimate another model where we replace the post closure indicator with two post closure indicators: one for before May 2021 and one for June 2021 and onwards, estimated impacts for the pre May 2021 period are very similar across the specification with and without worker fixed effects. The June 2021 and later effect is generally smaller, consistent with our assertion that the adverse labor market effect may be fading over time.

magnitude.

Low-income women may also be particularly susceptible to negative labor market impacts from childcare center closures. Pre-pandemic, low-income communities had less child care center capacity relative to estimated demand than higher income communities (Malik et al., 2020). The Center for American Progress predicted that child care businesses in lower-income areas would have a much harder time reopening after pandemic closures and that the discrepancy in child care access would only be exacerbated by the pandemic (Malik et al., 2020). Table 4 shows effects of childcare closures and class size limits using low-income mothers of young children and low-income non-mothers as the sample. We classify a woman as low-income if her household income is in the lowest tercile compared to other women of the same age and marital status in the same state in any pre-pandemic period where she appears in the CPS. Given the smaller sample sizes, it is not surprising that standard errors are considerably larger than for the full sample of women. However, estimated impacts are much larger and magnitude and statistically significant for some specifications. For instance, column 6 indicates that closure policies increased unemployment rates by 8 percentage points during the months when the policy was in effect and 4 percentage points in the months once the policies were discontinued. There is also fairly robust evidence that class size limits worsened unemployment rates for low-income mothers. The fact that pre-pandemic lower-income neighorhoods already had less child care capacity relative to higher income neighorhoods could explain the more severe impacts of these policies as class size limit constraints were more likely to bind for these areas. These results highlight yet one more mechanism through which the pandemic disproportionately impacted members of already disadvantaged populations.[9]

[9]Bacher-Hicks, Goodman and Mulhern (2021) show that residents of low-income areas were less able to adapt to the transition to online learning during school closures, and Alsan, Chandra and Simon (2021) find that racial minorities were disproportionately experienced excess mortality from COVID-19.

6.3 Effects for Fathers

We started by assessing impacts of closure and class size limit policies on mothers of young children because prior research indicates mothers bear a disproportionate share of childcare responsibilities (Alon et al., 2020*a*). However, childcare availability may also impact labor market outcomes of fathers. Table 5 reports estimated effects for fathers with a child aged 0 to 5. Estimated effects are generally smaller in magnitude than estimated effects for mothers in some specifications, but if we estimate models where we interact treatment with gender and our sample consists of both men and women, in most cases we cannot reject the null hypothesis that effects are the same across the two genders.

6.4 Robustness

In the analysis so far, we assessed impacts for parents whose youngest child was age 0 to 5. Appendix B tests robustness of the findings to including only mothers or fathers for whom all children are under age five. This estimation is less precise because we have smaller samples, but we find very similar impacts.

7 Conclusion

Using data on state-level childcare center closure policies and actual visits to child care centers based on monthly tracking of mobile phone users, we show that official childcare center closure policies have had important impacts on the childcare sector. Actual childcare center closure rates were 5-8 percentage points higher in the months when these policies were in effect, and even after the policies were discontinued, actual childcare center closure rates remain 2-7 percentage points higher in states that forced closures early on in the pandemic compared to states that did not.

Decreased childcare availability due to the short-term and long-lasting impacts of forced closures have decreased labor market participation rates and increased unemployment rates for parents of young children in these states, particularly non-married and low-income mothers. Taken as a whole, our results highlight the importance of a well-functioning childcare sector in promoting equitable labor market outcomes for parents with young children.

References

Ali, Umair, Chris M. Herbst, and Christos A. Makridis. 2021. "The impact of COVID-19 on the U.S. child care market: Evidence from stay-at-home orders." *Economics of Education Review*, 82: 1–13.

Alon, Titan, Matthias Doepke, Jane Olmstead-Rumsey, and Michele Tertilt. 2020*a*. "The Impact of COVID-19 on Gender Equality." *Covid Economics: Vetted and Real-Time Papers*, 4: 62–85.

Alon, Titan, Matthias Doepke, Jane Rumsey-Olmstead, and Michele Tertilt. 2020*b*. "This Time It's Different: The Role of Women's Employment in a Pandemic Recession." *NBER Working Paper 27660*.

Alsan, Marcella, Amitabh Chandra, and Kosali I. Simon. 2021. "The Great Unequalizer: Initial Health Effects of COVID-19 in the United States." *NBER Working Paper 28958*.

Bacher-Hicks, Andrew, Joshua Goodman, and Christine Mulhern. 2021. "Inequality in household adaptation to schooling shocks: Covid-induced online learning engagement in real time." *Journal of Public Economics*, 193: 1–17.

Baker, Michael, Jonathan Gruber, and Kevin Milligan. 2008. "Universal Child Care, Maternal Labor Supply, and Family Well-Being." *Journal of Political Economy*, 116(4): 709–745.

Bateman, Nicole, and Martha Ross. 2020. "Why has COVID-19 been especially harmful for working women?" *Brookings*.

Beauregard, Pierre-Loup, Marie Connolly, Catherine Haeck, and Timea Laura Molnar. 2021. "Primary School Reopenings and Parental Work." *Working Paper*.

Berger, Mark C., and Dan A. Black. 1992. "Child Care Subsidies, Quality of Care, and the Labor Supply of Low-Income, Single Mothers." *The Review of Economics and Statistics*, 74(4): 635–642.

Brown, Jessica H., and Chris M. Herbst. 2021. "Child Care over the Business Cycle." *IZA Discussion Paper No. 14048*.

Cascio, Elizabeth U. 2009. "Maternal Labor Supply and the Introduction of Kindergartens into American Public Schools." *Journal of Human Resources*, 44(1): 140–170.

Child Care Aware of America. 2020. "State by State Resources." *Accessed 27 August 2020*, `https://www.childcareaware.org/resources/map/`.

Collins, Caitlyn, Liana Christin Landivar, Leah Ruppanner, and William J. Scarborough. 2020. "COVID-19 and the gender gap in work hours." *Gender Work Organization*, 1–12.

Couch, Kenneth A., Robert W. Fairlie, and Huanan Xu. 2022. "The evolving impacts of the COVID-19 pandemic on gender inequality in the US labor market: The COVID motherhood penalty." *Economic Inquiry*, 1–23.

de Chaisemartin, Clement, and Xavier D'Haultfoeuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review*, 110(9): 2964–96.

de Chaisemartin, Clement, and Xavier D'Haultfoeuille. 2022. "Two-Way Fixed Effects and Differences-in-Differences Estimators with Several Treatments." *NBER Working Paper No. 30564*.

de Chaisemartin, Clément, Xavier D'Haultfoeuille, and Antoine Deeb. 2019. "TWOWAYFEWEIGhTS: Stata module to estimate the weights and robustness to treatment effect heterogeneity attached to two-way fixed effects regressions." *Statistical Software Components S458611*, Boston College Department of Economics, revised 17 Dec 2020.

Dingel, Jonathan I., Christina Patterson, and Joseph Vavra. 2020. "Childcare Obligations Will Constrain Many Workers When Reopening the US Economy." *Becker Friedman Institute Working Paper No. 2020-46*, `https://bfi.uchicago.edu/wp-content/uploads/BFI_WP_202046.pdf`.

Dong, Ensheng, Hongru Du, and Lauren Gardner. 2020. "An interactive web-based dashboard to track COVID-19 in real time." *The Lancet Infectious Diseases*, 20(5): 533–534.

Dong, Ensheng, Hongru Du, and Lauren Gardner. 2022. "COVID-19 Data Repository by the Center for Systems Science and Engineering (CSSE) at Johns Hopkins University." `https://github.com/CSSEGISandData/COVID-19`.

Felfe, Christina, and Rafael Lalive. 2018. "Does early child care affect children's development?" *Journal of Public Economics*, 159(1): 33–53.

Fitzpatrick, Maria Donovan. 2010. "Preschoolers Enrolled and Mothers at Work? The Effects of Universal Prekindergarten." *Journal of Labor Economics*, 28(1): 51–85.

Flood, Sarah and King, Miriam and Rodgers, Renae and Ruggles, Steven and Warren, J. Robert. 2020. "Integrated Public Use Microdata Series, Current Population Survey: Version 8.0 [dataset]." *Minneapolis, MN: IPUMS.*, `https://doi.org/10.18128/D030.V8.0`.

Food Industry Association. 2020. "Coronavirus Emergency State-by-State Child Care Landscape." *Accessed 27 August 2020*, `https://www.fmi.org/docs/default-source/state-affairs/covid19-state-by-state-child-care-landscape.pdf?sfvrsn=461eb9e2_8`.

Freyaldenhoven, Simon, Christian Hansen, Jorge Eduardo Perez, and Jesse Shapiro. 2021. "XTEVENT: Stata module to estimate and visualize linear panel event-study models." *Statistical Software Components S458987.*

Furman, Jason, Melissa Schettini Kearney, and Wilson Powell. 2021. "The Role of Childcare Challenges in the US Jobs Market Recovery During the COVID-19 Pandemic." *NBER Working Paper 28934.*

Garcia, Kairon Shayne D., and Benjamin W. Cowan. 2022. "The Impact of School and Childcare Closures on Labor Market Outcomes during the COVID-19 Pandemic." *NBER Working Paper No. 29641.*

Gelbach, Jonah B. 2002. "Public Schooling for Young Children and Maternal Labor Supply." *American Economic Review*, 92(1): 307–322.

Gordon, James A., Herbst Chris M., and Erdal Tekin. 2021. "Who's minding the kids? Experimental evidence on the demand for child care quality." *Economics of Education Review*, 80: 1–15.

Grunewald, Rob, and Phil Davies. 2011. "Hardly child's play." *Federal Reserve Bank of Minneapolis*, `https://www.minneapolisfed.org/article/2011/hardly-childs-play`.

Heggeness, Misty L. 2020. "Estimating the Immediate Impact of the COVID-19 Shock on Parental Attachment to the Labor Market and the Double Bind of Mothers." *Review of Economics of the Household*, 18(4): 1053–1078.

Herbst, Chris M. 2010. "The labor supply effects of child care costs and wages in the presence of subsidies and the earned income tax credit." *Review of Economics of the Household*, 8: 199–230.

Herbst, Chris M. 2013. "The impact of non-parental child care on child development: Evidence from the summer participation "dip"." *Journal of Public Economics*, 105: 86–105.

Herbst, Chris M. 2018. "The rising cost of child care in the United States: A reassessment of the evidence." *Economics of Education Review*, 64: 13–30.

Herbst, Chris M., and Erdal Tekin. 2016. "The Impact of Child-Care Subsidies on Child Development: Evidence from Geographic Variation in the Distance to Social Service Agencies." *Journal of Policy Analysis and Management*, 35(1): 94–116.

Hunt Institute. 2020. "COVID-19 Resources & Policy Considerations: Child Care State Actions." *Accessed 27 August 2020*, `http://www.hunt-institute.org/covid-19-resources/state-child-care-actions-covid-19/`.

Lee, Emma K., and Zachary Parolin. 2021. "The Care Burden during COVID-19: A National Database of Child Care Closures in the United States." *Sociological Research for a Dynamic World*, 7: 1–10.

Lee, San Yoon, Minsung Park, and Yongseok Shin. 2021. "Hit Harder, Recover Slower? Unequal Employment Effects of the COVID-19 Shock." *NBER Working Paper 28354*.

Lim, Katherine, and Mike Zabek. 2022. "Women's Labor Force Exits during COVID-19: Differences by Motherhood, Race, and Ethnicity." *Working Paper*, `https://www.mikezabek.com/pdf/COVIDLFP.pdf`.

Malik, Rasheed, Katie Hamm, Won F. Lee, Elizabeth E. Davis, and Aaron Sojouner. 2020. "The Coronavirus Will Make Child Care Deserts Worse and Exacerbate Inequality." *Center for American Progress*.

Meyers, Marcia K., Theresa Heintz, and Douglas A. Wolf. 2002. "Child Care Subsidies and the Employment of Welfare Recipients." *Demography*, 39(1): 165–179.

Mongeau, Lillian. 2021. "After mass closures, too little support, post-pandemic child care options will be scarce." *Hechinger Report*.

Moreland, Amanda, Christine Herlihy, Michael A. Tynan, Gregory Sunshine, Russell F. McCord, Charity Hilton, Jason Poovey, Angela K. Werner, Christopher D. Jones, Erika B. Fulmer, Adi V. Gundlapalli, Heather Strosnider, Aaron Potvien, Macarena C. García, Sally Honeycutt, Grant Baldwin, CDC Public Health Law Program, and Mitigation Policy Analysis Unit CDC COVID-19 Response Team. 2020. "Timing of State and Territorial COVID-19 Stay-at-Home Orders and Changes in Population Movement - United States, March 1 - May 31, 2020." *MMWR Morbitiy and Mortality Weekly Report*, 69(35): 1198–1203.

National Association for the Education of Young Children. 2020*a*. "Am I Next? Sacrificing to Stay Open, Child Care Providers Face a Bleak Future Without Relief." *Report*.

National Association for the Education of Young Children. 2020*b*. "Holding On Until Help Comes: A Survey Reveals Child Care's Fight to Survive." *Report*.

Olden, Andreas, and Jarle Møen. 2020. "The Triple Difference Estimator." *Discussion Papers 2020/1, Norwegian School of Economics, Department of Business and Management Science*.

Patterson, Shawn Jr. 2022. "The Politics of Pandemics: The Effect of Stay-At-Home Orders on COVID-19 Mitigation." *State Politics & Policy Quarterly*, 22(1): 1–23.

Prados, Maria J., and Gema Zamarro. 2021. "School Re-openings, Childcare Arrangements, and Labor Outcomes During COVID-19." *NBER Economics of Caregiving Conference Paper*, http://conference.nber.org/conf_papers/f158594.pdf.

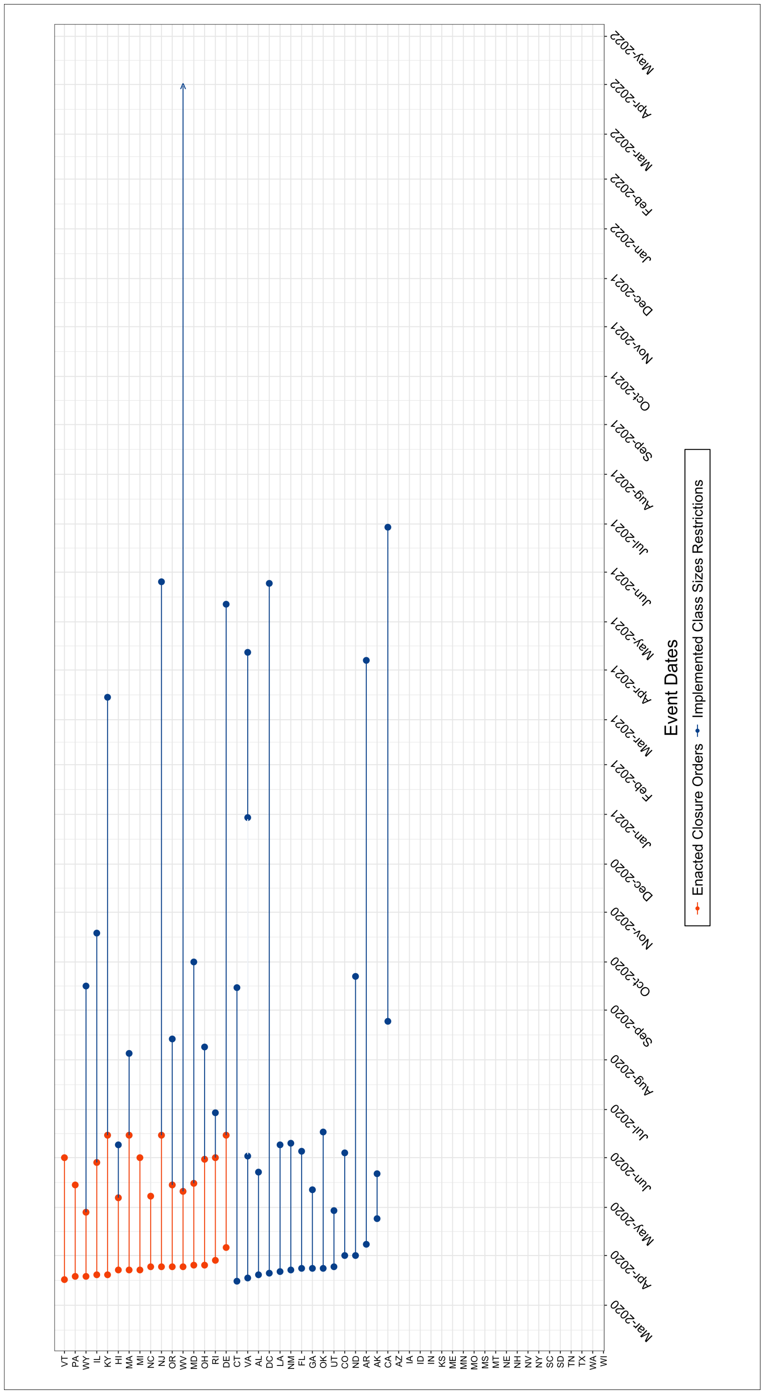
Quinton, Sophie. 2020. "Will Child Care Be There When States Reopen?" *PEW Trusts Stateline Article*, https://www.pewtrusts.org/en/research-and-analysis/blogs/stateline/2020/04/27/will-child-care-be-there-when-states-reopen.

Russell, Lauren, and Chuxuan Sun. 2020. "The Effect of Mandatory Child Care Center Closures on Women's Labor Market Outcomes During the COVID-19 Pandemic." *COVID Economics: Vetted and Real-Time Papers*, 62: 124–154.

Tekin, Erdal. 2007*a*. "Child care subsidies, wages, and the employment of single mothers." *Journal of Human Resources*, 42: 453–487.

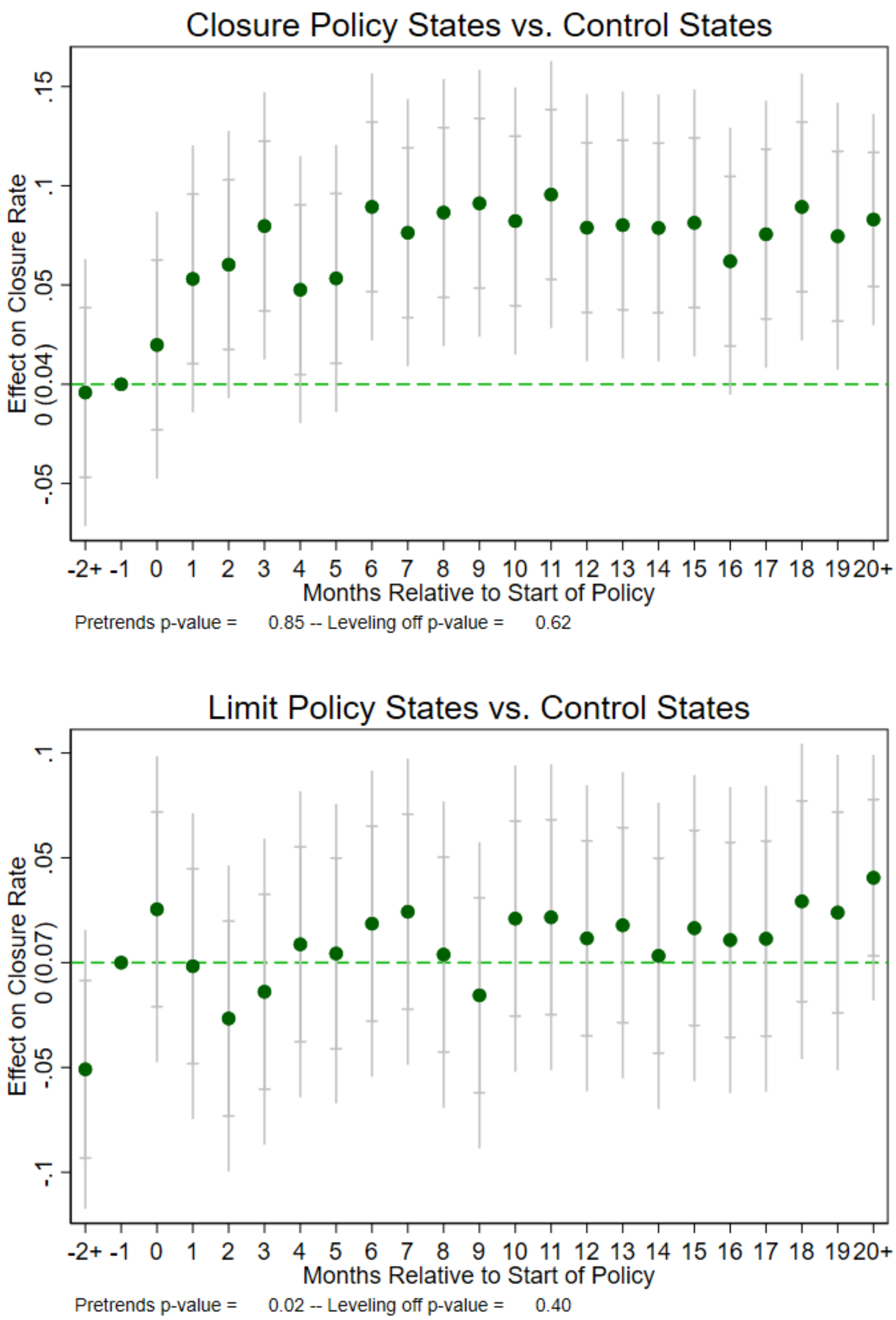
Tekin, Erdal. 2007*b*. "Single mothers working at night: Standard work, child care subsidies, and implications for welfare reform." *Economic Inquiry*, 45(2): 233–250.

Figure 1: State Policy Timeline



Notes: Figure shows time periods in which closure policies and class size limit policies were in effect by state. Information comes from government press releases, the Hunt Institute (2020), the Food Industry Association (2020), and Child Care Aware of America (2020). For more details, see full data appendix.

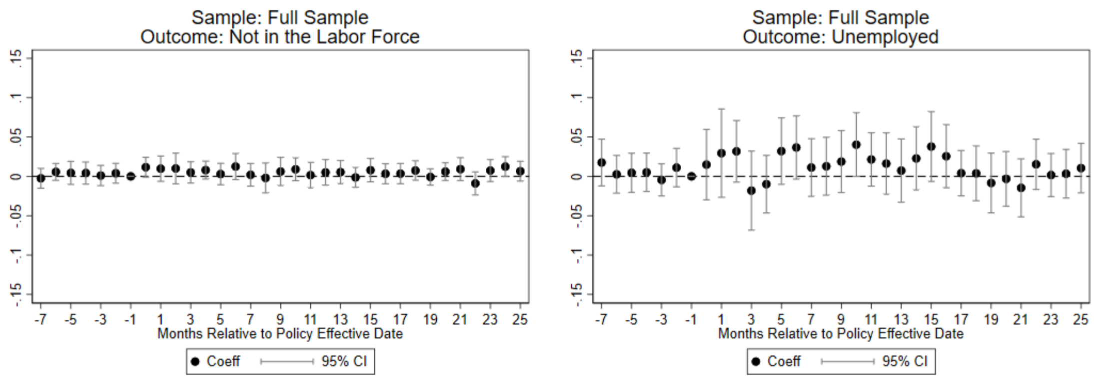
Figure 2: Event Studies for Effect of Forced Closure and Class Size Limit Policies



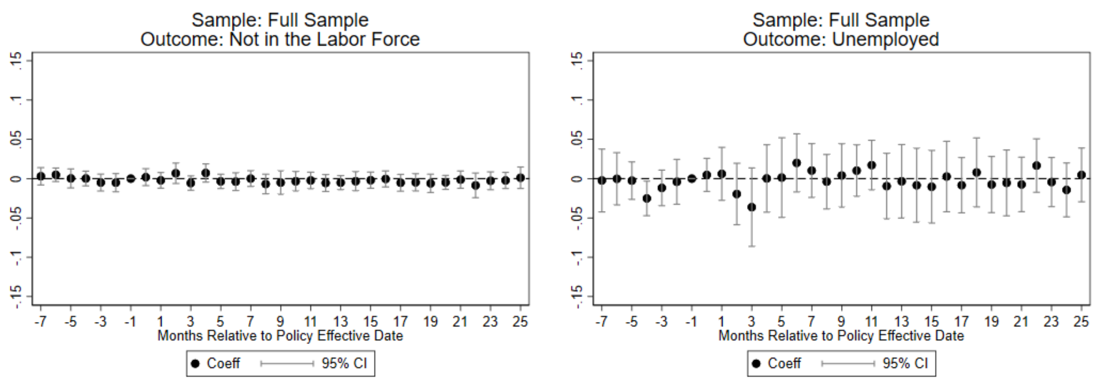
Notes: The dependent variable is the share of centers that experienced at least a 50% decline in total visits relative to the same month in 2019. Control states are those that have never had a forced child care center closure policy nor a forced class size limit policy. Month 0 is the first month the policy is in effect, even if the policy was in effect for only part of the month. Pointwise confidence intervals are illustrated by inner bars, and uniform sup-t confidence bands are illustrated by the outer lines (Freyaldenhoven et al., 2021).

Figure 3: Triple-Differences Event Studies for Effect of Forced Closure and Class Size Limit Policies on Mothers

(a) Forced Closure Policies



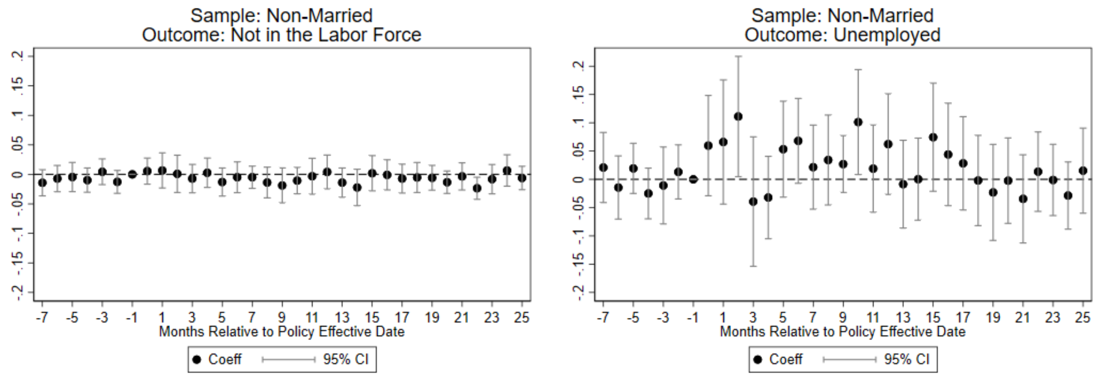
(b) Class Size Limit Policies



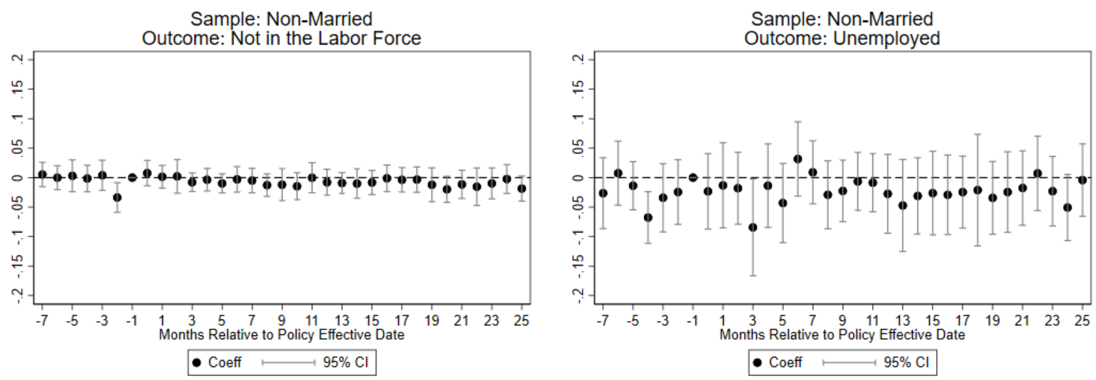
Notes: Plots report coefficients and 95% confidence intervals from an event study version of the triple differences specification (equation 3 as described in the text) using the female sample. For these plots, -1 is the month prior to when the policy first takes effect and is the omitted event study indicator. Panel A plots report coefficients on the closure policy event study time indicators while Panel B plots report coefficients on the class size limit policy event study time indicators.

Figure 4: Triple-Differences Event Studies for Effect of Forced Closure and Class Size Limit Policies on Non-Married Mothers

(a) Forced Closure Policies



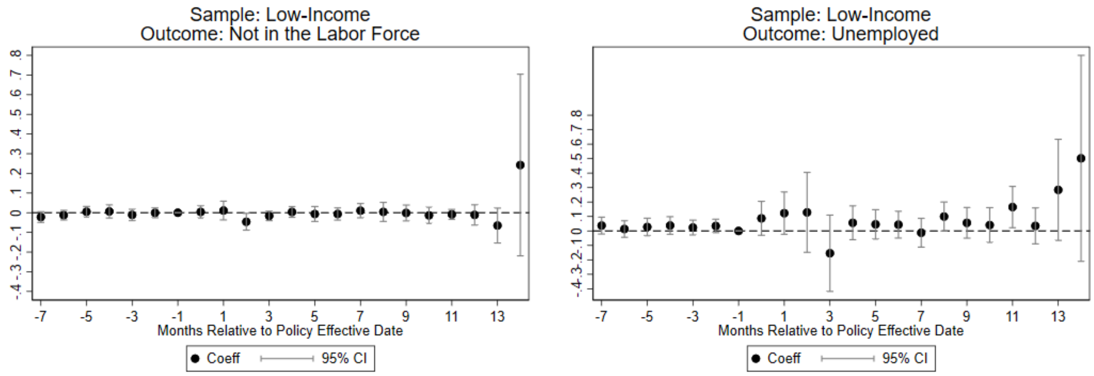
(b) Class Size Limit Policies



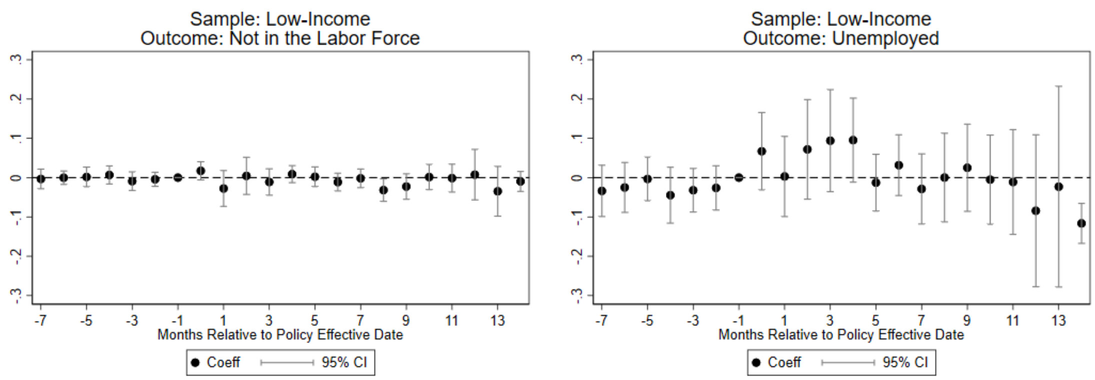
Notes: Plots report coefficients and 95% confidence intervals from an event study version of the triple differences specification (equation 3 as described in the text) using the non-married female sample. For these plots, -1 is the month prior to when the policy first takes effect and is the omitted event study indicator. Panel A plots report coefficients on the closure policy event study time indicators while Panel B plots report coefficients on the class size limit policy event study time indicators.

Figure 5: Triple-Differences Event Studies for Effect of Forced Closure and Class Size Limit Policies on Low-Income Mothers

(a) Forced Closure Policies



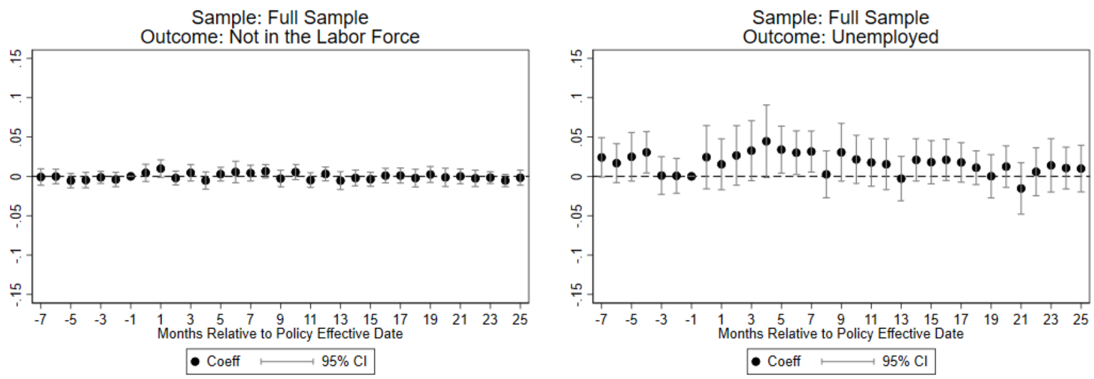
(b) Class Size Limit Policies



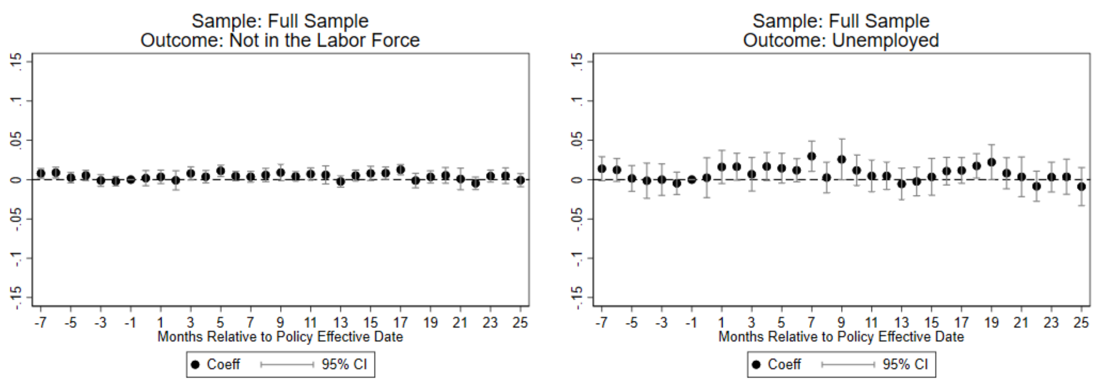
Notes: Plots report coefficients and 95% confidence intervals from an event study version of the triple differences specification (equation 3 as described in the text) using the low-income female sample. For these plots, -1 is the month prior to when the policy first takes effect and is the omitted event study indicator. Panel A plots report coefficients on the closure policy event study time indicators while Panel B plots report coefficients on the class size limit policy event study time indicators.

Figure 6: Triple-Differences Event Studies for Effect of Forced Closure and Class Size Limit Policies for Fathers

(a) Forced Closure Policies



(b) Class Size Limit Policies



Notes: Plots report coefficients and 95% confidence intervals from an event study version of the triple differences specification (equation 3 as described in the text) using the male sample. For these plots, -1 is the month prior to when the policy first takes effect and is the omitted event study indicator. Panel A plots report coefficients on the closure policy event study time indicators while Panel B plots report coefficients on the class size limit policy event study time indicators. These plots using the low-income subgroup has no effects beyond +14 because income status is defined based on pre-pandemic data and no cohorts reporting past +14 are observed pre-pandemic.

Table 1: Effect of Closure and Class Size Limit Policies on Child Care Center Closure Rates

	(1) Closure Rate (50% Decline)	(2) Closure Rate (25% Decline)	(3) Closure Rate (75% Decline)	(4) Average Change in Total Visits
Closure In Effect	0.0808***	0.0518**	0.0578***	-0.0525**
	(0.0276)	(0.0211)	(0.0191)	(0.0208)
Post Closure	0.0681***	0.0723***	0.0215**	-0.0638***
	(0.0240)	(0.0245)	(0.00890)	(0.0209)
Limit In Effect	0.0243	-0.00244	0.0220	0.0181
	(0.0263)	(0.0252)	(0.0170)	(0.0255)
Post Limit	0.0337	0.0205	0.0144	0.000703
	(0.0248)	(0.0231)	(0.0127)	(0.0212)
State FE	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	1479	1479	1479	1479

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the state level.

Table 2: Effect of Closure and Class Size Limit Policies on Womens' Labor Market Outcomes (Full Sample)

	(1) Not in Labor Force	(2) Not in Labor Force	(3) Not in Labor Force	(4) Not in Labor Force	(5) Unemployed	(6) Unemployed	(7) Unemployed	(8) Unemployed
Closure In Effect	0.0247**	0.0147*	0.0124***	0.0101*	0.0308*	0.0441***	0.0107	0.0343*
	(0.0112)	(0.00830)	(0.00409)	(0.00599)	(0.0183)	(0.0161)	(0.0200)	(0.0190)
Post Closure	0.0182	0.0158	0.00251	0.00526*	0.0100*	0.0265**	0.00782	0.0309**
	(0.0111)	(0.00976)	(0.00263)	(0.00307)	(0.00591)	(0.0100)	(0.00707)	(0.0129)
Limit In Effect	0.0240**	0.0139**	-0.000277	-0.000307	-0.00353	0.0172***	0.000849	0.0221**
	(0.0106)	(0.00598)	(0.00220)	(0.00272)	(0.00734)	(0.00578)	(0.00736)	(0.00936)
Post Limit	0.0102	-0.00123	-0.00221	-0.0000959	0.00253	0.0125*	0.00698	0.0188
	(0.00952)	(0.00770)	(0.00196)	(0.00275)	(0.00495)	(0.00658)	(0.00986)	(0.0133)
Worker FE	No	Yes	No	Yes	No	Yes	No	Yes
Model	DD	DD	DDD	DDD	DD	DD	DDD	DDD
Observations	140962	135501	380183	361563	95328	89980	285122	266758

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample sizes are smaller for models with worker fixed effects because singletons are dropped for the estimation. Standard errors are clustered at the state level.

Table 3: Effect of Closure and Class Size Limit Policies on Women's Labor Market Outcomes (Non-Married Mothers)

	(1) Not in Labor Force	(2) Not in Labor Force	(3) Not in Labor Force	(4) Not in Labor Force	(5) Unemployed	(6) Unemployed	(7) Unemployed	(8) Unemployed
Closure In Effect	0.0540	0.0371**	0.0126	0.00481	0.0518	0.0616	0.0444	0.0665
	(0.0338)	(0.0173)	(0.00843)	(0.0125)	(0.0451)	(0.0392)	(0.0488)	(0.0450)
Post Closure	0.0117	0.0512***	-0.000336	0.00853	0.0347**	0.0541*	0.0223	0.0335
	(0.0203)	(0.0184)	(0.00465)	(0.00606)	(0.0160)	(0.0277)	(0.0155)	(0.0337)
Limit In Effect	0.0442***	0.0324*	0.00412	0.00681	-0.00476	0.0420**	-0.0102	0.0335
	(0.0149)	(0.0174)	(0.00610)	(0.00496)	(0.0144)	(0.0159)	(0.0173)	(0.0210)
Post Limit	0.000601	-0.00841	-0.00487	-0.000546	0.00870	0.0167	0.00652	0.0127
	(0.0187)	(0.0195)	(0.00384)	(0.00632)	(0.0117)	(0.0217)	(0.0156)	(0.0309)
Worker FE	No	Yes	No	Yes	No	Yes	No	Yes
Model	DD	DD	DDD	DDD	DD	DD	DDD	DDD
Observations	38282	36043	212651	199253	27255	25008	162485	149123

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample sizes are smaller for models with worker fixed effects because singletons are dropped for the estimation. Standard errors are clustered at the state level.

Table 4: Effect of Closure and Class Size Limit Policies on Women's Labor Market Outcomes (Low-Income Mothers)

	(1) Not in Labor Force	(2) Not in Labor Force	(3) Not in Labor Force	(4) Not in Labor Force	(5) Unemployed	(6) Unemployed	(7) Unemployed	(8) Unemployed
Closure In Effect	0.0362	0.00367	0.00745	-0.00760	0.0834**	0.0764**	0.0600	0.0458
	(0.0270)	(0.0204)	(0.0120)	(0.0129)	(0.0388)	(0.0344)	(0.0469)	(0.0443)
Post Closure	-0.0176	0.00191	0.0000654	-0.00276	0.0145	0.0403**	0.0397	0.0404
	(0.0184)	(0.0189)	(0.00627)	(0.00918)	(0.0139)	(0.0186)	(0.0241)	(0.0251)
Limit In Effect	-0.0174	-0.00563	-0.00549	-0.00572	0.0321*	0.0522***	0.0553**	0.0902***
	(0.0192)	(0.0145)	(0.00701)	(0.00911)	(0.0170)	(0.0189)	(0.0253)	(0.0243)
Post Limit	0.00589	0.000351	0.00223	-0.00261	0.0207	0.0310*	0.0440*	0.0811***
	(0.0222)	(0.0181)	(0.00730)	(0.00772)	(0.0128)	(0.0164)	(0.0219)	(0.0298)
Worker FE	No	Yes	No	Yes	No	Yes	No	Yes
Model	DD	DD	DDD	DDD	DD	DD	DDD	DDD
Observations	25886	24951	52983	50166	15262	14356	33937	31065

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Low-income mothers are defined as those with incomes in the lowest tercile among other women in the state with the same age and marital status at any point in the pre-pandemic period. Sample sizes are smaller for models with worker fixed effects because singletons are dropped for the estimation. Standard errors are clustered at the state level.

Table 5: Effect of Closure and Class Size Limit Policies on Fathers' Labor Market Outcomes (Full Sample)

	(1) Not in Labor Force	(2) Not in Labor Force	(3) Not in Labor Force	(4) Not in Labor Force	(5) Unemployed	(6) Unemployed	(7) Unemployed	(8) Unemployed
Closure In Effect	-0.000344	0.00187	0.00922**	0.0132***	0.0196**	0.0234**	0.00466	0.0255**
	(0.00860)	(0.00735)	(0.00424)	(0.00470)	(0.00962)	(0.0105)	(0.0116)	(0.0116)
Post Closure	-0.00450	0.00358	0.00299	0.0127***	0.0109*	0.0186**	0.00499	0.0348***
	(0.00592)	(0.00710)	(0.00240)	(0.00338)	(0.00543)	(0.00709)	(0.00786)	(0.00856)
Limit In Effect	0.00592	0.000358	0.00106	0.00263	0.00310	-0.00359	0.00632*	0.0124**
	(0.00631)	(0.00481)	(0.00249)	(0.00262)	(0.00374)	(0.00637)	(0.00370)	(0.00500)
Post Limit	0.00379	-0.0000140	0.000945	0.00318	0.00520	-0.00438	0.00754*	0.0132**
	(0.00419)	(0.00584)	(0.00218)	(0.00292)	(0.00488)	(0.00845)	(0.00381)	(0.00513)
Worker FE	No	Yes	No	Yes	No	Yes	No	Yes
Model	DD	DD	DDD	DDD	DD	DD	DDD	DDD
Observations	108317	104304	410485	391312	102739	98687	351146	331698

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample sizes are smaller for models with worker fixed effects because singletons are dropped for the estimation. Standard errors are clustered at the state level.

Appendix Materials

A Robustness of Policy DD Estimates to Inclusion of Controls for COVID Infection and Death Rates

Table A.1: Effect of Closure and Class Size Limit Policies on Child Care Center Closure Rates Controlling for Confirmed COVID Cases per 1000 People

	(1) Closure Rate (50% Decline)	(2) Closure Rate (25% Decline)	(3) Closure Rate (75% Decline)	(4) Average Change in Total Visits
Closure In Effect	0.0746***	0.0436**	0.0556***	-0.0473**
	(0.0266)	(0.0192)	(0.0189)	(0.0191)
Post Closure	0.0638***	0.0655***	0.0204**	-0.0606***
	(0.0231)	(0.0215)	(0.00909)	(0.0205)
Limit In Effect	0.0170	-0.00170	0.0182	0.00638
	(0.0232)	(0.0227)	(0.0151)	(0.0224)
Post Limit	0.0294	0.0215	0.0121	-0.0109
	(0.0222)	(0.0205)	(0.0126)	(0.0205)
Confirmed Cases per 1000	0.0000592	0.000110	0.0000166	-0.000227
	(0.000224)	(0.000262)	(0.000179)	(0.000271)
Lag of Confirmed Cases per 1000	0.000657**	0.000534**	0.000479*	-0.000859***
	(0.000267)	(0.000257)	(0.000257)	(0.000296)
State FE	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	1428	1428	1428	1428

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the state level.

Table A.2: Effect of Closure and Class Size Limit Policies on Child Care Center Closure Rates Controlling for Deaths from COVID per 1000 People

	(1) Closure Rate (50% Decline)	(2) Closure Rate (25% Decline)	(3) Closure Rate (75% Decline)	(4) Average Change in Total Visits
Closure In Effect	0.0730***	0.0456**	0.0514***	-0.0482**
	(0.0261)	(0.0195)	(0.0180)	(0.0189)
Post Closure	0.0638***	0.0652***	0.0207**	-0.0603***
	(0.0231)	(0.0214)	(0.00924)	(0.0204)
Limit In Effect	0.0168	-0.00132	0.0174	0.00619
	(0.0230)	(0.0227)	(0.0149)	(0.0224)
Post Limit	0.0295	0.0216	0.0123	-0.0111
	(0.0222)	(0.0203)	(0.0127)	(0.0202)
Deaths per 1000	0.0198	-0.0100	0.0339**	-0.00393
	(0.0120)	(0.0175)	(0.0149)	(0.0132)
Lag of Deaths per 1000	-0.00153	-0.00205	0.00662	0.00402
	(0.0128)	(0.0123)	(0.00693)	(0.0150)
State FE	Yes	Yes	Yes	Yes
Year Month FE	Yes	Yes	Yes	Yes
Observations	1428	1428	1428	1428

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors are clustered at the state level.

B Robustness Check: All Children Aged 0 to 5

Table B.1: Effect of Closure and Class Size Limit Policies on Mothers' Labor Market Outcomes: Have Any Child(ren) Aged 0-5)

	(1) Not in Labor Force	(2) Not in Labor Force	(3) Not in Labor Force	(4) Not in Labor Force	(5) Unemployed	(6) Unemployed	(7) Unemployed	(8) Unemployed
Closure In Effect	0.0164	0.00312	0.0119**	0.00947	0.0391	0.0429*	0.0194	0.0301
	(0.0208)	(0.0113)	(0.00523)	(0.00818)	(0.0254)	(0.0215)	(0.0274)	(0.0234)
Post Closure	-0.00442	0.00250	0.000680	0.00133	0.00798	0.0296*	0.00632	0.0254
	(0.0146)	(0.0121)	(0.00340)	(0.00506)	(0.00632)	(0.0149)	(0.00705)	(0.0162)
Limit In Effect	0.0197	0.0174*	-0.00138	0.000840	-0.00518	0.0129	-0.00610	0.0140
	(0.0118)	(0.00916)	(0.00299)	(0.00468)	(0.00610)	(0.00969)	(0.00725)	(0.0123)
Post Limit	0.00703	0.00623	-0.00380	0.00394	0.00334	0.00562	0.00137	0.00532
	(0.0155)	(0.00875)	(0.00291)	(0.00505)	(0.00817)	(0.00891)	(0.00851)	(0.0146)
Observations	71368	68128	310000	293330	50480	47373	239707	223389
Worker FE	No	Yes	No	Yes	No	Yes	No	Yes
Model	DD	DD	DDD	DDD	DD	DD	DDD	DDD

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample sizes are smaller for models with worker fixed effects because singletons are dropped for the estimation. Standard errors are clustered at the state level.

Table B.2: Effect of Closure and Class Size Limit Policies on Fathers' Labor Market Outcomes: Have Any Child(ren) Aged 0-5)

	(1) Not in Labor Force	(2) Not in Labor Force	(3) Not in Labor Force	(4) Not in Labor Force	(5) Unemployed	(6) Unemployed	(7) Unemployed	(8) Unemployed
Closure In Effect	0.00182	0.0000220	0.0124**	0.0134**	0.0120	0.0197	0.00509	0.0225
	(0.0123)	(0.0123)	(0.00507)	(0.00544)	(0.0105)	(0.0122)	(0.0116)	(0.0138)
Post Closure	-0.00993	0.00208	0.00432	0.0149***	0.00646	0.0126	0.00436	0.0290**
	(0.00715)	(0.0101)	(0.00277)	(0.00365)	(0.00630)	(0.00897)	(0.00894)	(0.0109)
Limit In Effect	0.00365	0.00523	0.00275	0.00690**	-0.000262	-0.00990*	0.00153	0.00993
	(0.00574)	(0.00780)	(0.00301)	(0.00326)	(0.00475)	(0.00575)	(0.00603)	(0.00633)
Post Limit	-0.00263	0.0111	0.00161	0.00744**	0.00650	-0.00301	0.00754	0.0196***
	(0.00538)	(0.00936)	(0.00288)	(0.00350)	(0.00585)	(0.00920)	(0.00550)	(0.00716)
Worker FE	No	Yes	No	Yes	No	Yes	No	Yes
Model	DD	DD	DDD	DDD	DD	DD	DDD	DDD
Observations	56287	53884	357869	340016	53512	51087	301344	283220

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Sample sizes are smaller for models with worker fixed effects because singletons are dropped for the estimation. Standard errors are clustered at the state level.