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Causal effects of closing businesses in a pandemic ☆

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ABSTRACT

We study whether state-level mandatory business closures implemented in response to the outbreak of the Covid-19 causally affect economic and health outcomes. Using plausibly exogenous variations in exposure to these restrictions, we find that they impose substantial losses to firms and workers, the former bearing approximately two thirds of the cost, consistent with firms partially insuring their workers. We show that mandatory business closures have a significant negative causal effect on mortality rates, particularly in areas featuring contact-intensive occupations. We discuss the assumptions under which the health benefits of business closures exceed their associated economic costs.

1. Introduction

In response to the outbreak of Covid-19, virtually all U.S. states ordered businesses deemed as non-essential to close their physical operations in an attempt to curb the virus' propagation. Whether or not such decisions had a negative causal effect on economic outcomes is an empirical question. On the one hand, being forced to shut down physical locations might lead firms to experience severe losses and prevent them from insuring workers against the associated temporary disruption. On the other hand, if firms would have closed physical locations due to the drop in demand irrespective of mandatory business closures, or if firms adjust their operations to offset the impact of these policies by resorting to work-from-home, the causal effect of state-mandated business closures might be low. Estimating their incidence and distributional impact is important to understand how firms responded to these restrictions and inform the design of policies supporting businesses and workers.

Beyond their cost, another important question is whether or not mandatory business closures are effective at reducing virus propagation. The optimal business closure policy trades off the benefits of limiting infections and saving lives with the drop in economic activity. Estimating the impact of state-mandated business closures on Covid-19 infections and death requires to isolate their impact from that of all other policy

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interventions or behavioral patterns, and to properly take propagation dynamics into account.

In this paper, we address these questions and provide an empirical evaluation of the incidence of U.S. states' business closure decisions on economic and health outcomes during the first wave of the Covid-19 pandemic. Our setting allows us to tightly identify the causal effects of these restrictions on both profits and labor income, and on Covid-19 infections and deaths as well as a variety of other health outcomes.

Since the timing and list of sectors affected by mandatory business closures vary across states, we start by collecting state-level Executive Orders shutting down certain sectors deemed as "non-essential". We read each of them to identify the list of sectors whose physical operations were forced to shut down, and the dates at which those restrictions were implemented and eased. Building such a state-sector-week panel of restrictions is an important contribution of this paper. One legitimate concern may be that mandatory business closures are endogenous. For instance, they may be stricter or more strictly enforced in states where contagion is more severe, or in states where their impact on economic activity is expected to be weaker. To overcome endogeneity, our identification strategy relies on *within-state* and *within-sector* comparisons.

To estimate the causal impact of mandatory business closures on firms' profits, we exploit granular data on the industry and location of firms' establishment and compute, for each publicly listed firm in the U.S., the share of restricted labor, defined as the share of employees who cannot work due to the shut-down of physical operations mandated by each U.S. state, adjusted by the share of those who work from home. In panel regressions at the firm×quarter level with firm, state×quarter and sector×quarter fixed effects, we find that firms with a 10% share of restricted labor experience a decline in quarterly return on asset (ROA) of around 0.16 percentage points, a significant drop relative to the sample average quarterly ROA of 1.5%. We fail to find any of these effects prior to the initiation of business closures. This suggests that the parallel trends assumption is satisfied and validates our identification strategy.

One may wonder if the drop in profit is a permanent loss, or whether there may be subsequent catching up. To address this question, we check whether the drop in firms' profits translates into market value losses. We compute daily stock returns around the issuance dates of state-level Executive Orders and compare them for firms in the same state or sector but a different exposure to labor restrictions. We find that the announcement of mandatory business closures leads to a significant drop in firms' market values. Importantly, the effect is concentrated in a short event window around the announcement of the Executive Orders.

To estimate the causal impact of mandatory business closures on labor income, we exploit within-state variations in commuting zone (CZ) level exposure to these restrictions. More precisely, for each CZ in the U.S., we construct the share of restricted labor, defined as the local employment share of sectors mandated to shut-down their physical operations, adjusted by the share of workers who can work from home. In panel regressions at the CZ×week level with CZ and state×week fixed effects, we show that a 10 percentage point increase in the share of restricted labor is associated with a 9% drop in total wages. The results are robust to the inclusion of a host of variables controlling for CZ-level socio-demographic characteristics as well as local public health situation prior to the initiation of business closures, and the implementation of mask mandates and stay-at-home orders. Consistent with a causal interpretation of this finding, wages start dropping in the week when restrictions are implemented, and we fail to find any prior trend in the effect prior to state-mandated shut-downs.

After having assessed the impact of mandatory business closures on profits and labor income, we turn to the analysis of their effects on health outcomes. For this, we follow prior work in the empirical literature estimating the effects of non-pharmaceutical restrictions on health outcomes (such as Adda (2016); Chernozhukov et al. (2021); Karaivanov et al. (2021)), and use an empirical model consistent with a susceptible-infectious-recovered-deceased (SIRD) epidemiological model for the spread of Covid-19, in which we control for the lagged number of Covid-19 infections. We find that a 10 percentage point increase in the share of restricted labor for one week is associated with a 0.002 percentage point decline in Covid-19 mortality rates. The coefficients are stable when we introduce socio-demographic and public health controls. We fail to find any trends in infection and death rates prior to the restriction, which supports a causal interpretation of the findings. Importantly, we find no evidence of spillovers in health outcomes across CZs, which confirms that our analysis is run at the appropriate level and allows us to capture the full effect of labor restrictions. We run a variety of robustness tests that corroborate our findings.

We then expand the analysis to the effects of business closures on Covid-19 hospitalizations and mental health outcomes in order to further assess the health-related benefits (such as avoided hospitalizations) and health-related costs (such as deteriorated mental health) of business shutdowns. We find that a 10 percentage point increase in the share of restricted labor is associated with a significant drop in the number of new Covid-19 hospitalizations per week, around 0.9 per 10,000. We also find a statistically significant and positive relationship between business closures and symptoms of bad mental health (anxiety, being worried, feeling down, low interest for things).

We next complement our baseline analysis of Covid-19 deaths with an estimation of the effect of business closures on total (Covid and non-Covid related) *years of life saved*. To do so, we collect data from the CDC County Mortality Statistics on total deaths for each county. The data is available at the monthly frequency, and includes information on the age and gender of the deceased individuals. Combined with actuarial life expectancy tables by gender and age, we construct estimates for the number of years of life lost at the CZ×month level. We find that a 10 percentage point increase in the share of restricted labor is associated with a drop in monthly years of life lost of around 16.8 years per 10,000 inhabitants, and that the average person whose life was saved because of business closures gained around 13 years of expected lifetime. While this calculation highlights the fact that business closures caused a significant increase in expected lifetime, it is consistent with the notion that persons that were saved were on average older.

Our framework allows us to go one step further and assess whether lives might have been saved at a lower cost. Mandatory business closures are likely to have the strongest effect on health outcomes where workplace interactions are more intense. To check whether this is the case, we split the cross-section of firms and CZs into those above and below median workplace interaction intensity. The effect of labor restrictions on firm profits and labor income are similar across subgroups, but their effect on health outcomes is concentrated in CZs with above median workplace interaction intensity. Hence, the same number of life-years might have been saved at a lower cost if restrictions had been focused on CZs with intensive workplace interactions.

Finally, we extrapolate our micro estimates in order to provide an analysis of the aggregate cost of business closures in terms of lost profits for firms and foregone wages for affected workers, and compare it to the health benefit from reduced mortality and hospitalizations, net of the mental health costs. Our findings translate into a contraction in aggregate profits of around \$359 billion (with a 95%-confidence interval between \$51 and \$667 billion) and a contraction in aggregate labor income of around \$173 billion (with a 95%-confidence interval between \$31 and \$314 billion). Importantly, the cost of business closures is disproportionately borne by firms: approximately two thirds of the incidence falls on profits whereas only one third falls on wages. Given the share of labor in valued added in the U.S. (around 60%),[1] these findings suggest that firms partially insured their workers by absorbing a large share of the cost through lower profits.

[1] See e.g. Grossman and Oberfield (2022) for a recent review on the measurement of the labor share. While the gross labor share is around 60%, labor income as a fraction of net value added (which deducts depreciation from the gross measure) reaches 75%.

When extrapolating the micro estimates obtained from the SIRD epidemiological model, we find that state-mandated business closures saved approximately 8.1 million quality adjusted years of life (with a 95%-confidence interval between 2 million and 14 million) during the first wave of the Covid-19. Estimates from a linear model in which we do not control for the lagged number of Covid-19 infections are lower than that from the SIRD model. Under a linear model, we find that business closures saved approximately 3.2 million quality adjusted years of life (with a 95%-confidence interval between 0.5 million and 5.8 million).

Using confidence intervals around our point estimates and various assumptions about the value of a life-year, we are able to determine under which assumptions the benefits of business closures exceed their associated costs. From the SIRD model, we find that the point estimates of the health benefit, in monetary terms, are of the same order of magnitude as the economic cost of the state-mandated business closures and often exceed it. However, uncertainty around our estimates and results using linear specifications prevent us from statistically rejecting a net loss associated with business closures. With respect to other health outcomes, we find that the monetary equivalents of the aggregate health benefit in terms of reduced hospitalization and the aggregate cost in terms of mental health disorders are in the same ballpark, and of a much smaller magnitude than the social benefits in terms of reduced mortality.

Such an *ex post* analysis should not be interpreted as the cost-benefit faced *ex ante* by decision makers at the time business closures were implemented, when there was enormous uncertainty on both the epidemiological features of the Covid-19 and the potential economic consequences of business closures. The benefit of analyzing the *ex post* cost-benefit of mandated business closures is that it makes it possible to seize the cost of these policies that were implemented for the first time in response to a catastrophic event, and to examine how much of this cost was borne by profits and wages respectively.

Our work is related to studies of the economic consequences of policies undertaken in response to viral diseases and pandemics. Adda (2016) studies the effect of closing down schools and shutting down public transportation on the transmission of various viruses in France, and computes the associated economic costs. In the context of the 1918 Flu Pandemic, Barro et al. (2020) find a death rate of 2%, and a cumulative loss in GDP per capita of 6% over 3 years. Correia et al. (2022) show that the 1918 Flu Pandemic led to a 18% drop in state manufacturing output up to four years after the outbreak of the disease, and that social distancing policies had a positive effect on future economic outcomes. In contrast, we find significant *negative* effects of social distancing on economic outcomes. This difference can be attributed to the fact that policies implemented in response to the 1918 Flu did not include mandatory business closures.

Recent papers have analyzed social distancing restrictions undertaken during the Covid-19 crisis and found mixed results. In particular, Baek et al. (2021) and Goolsbee and Syverson (2021) provide evidence that stay-at-home orders had a limited causal impact on unemployment insurance claims and consumer behavior. In contrast, we document significant economic costs associated with stated-mandated business closures. We jointly study economic and health outcomes in a single framework and compare them to provide a cost-benefit analysis of these restrictions. While Bongaerts et al. (2021), Borri et al. (2021) and Porto et al. (2021) study the effects of mandated business closures on health outcomes in Italy, we assess both their impact on health and economic outcomes in the U.S., and estimate the incidence of business closures on both firms and workers.[2]

We also contribute to a recent stream of research on the macroeconomic implications of the Covid-19 crisis. Berger et al. (2022), Atkeson (2020), Eichenbaum et al. (2021), Acemoglu et al. (2021), Jones et al. (2021), Piguillem and Shi (2022), Glover et al. (2023) and Alvarez et al. (2021) incorporate epidemiological models of contagion in macroeconomic models to shed light on optimal mitigation policies. Barrot et al. (2021) explore the effects of social distancing in a production network model. Hall et al. (2020) and Greenstone and Nigam (2020) make assumptions about the value of a statistical life to compute the socially desirable amount of social distancing or consumption loss. Our *ex post* cost-benefit analysis offers a useful benchmark for their assumptions.

While our paper is the first to estimate the impact on firms' profits of a non-pharmaceutical intervention, other work have used firm and stock market data to assess the economic impact of the Covid-19 crisis. Gormsen and Koijen (2020) infer expected annual GDP growth from dividend futures. Landier and Thesmar (2020) infer the evolution of the discount rate from the difference between forecast-implied and actual returns. Hassan et al. (2020) document firms' concerns related to the collapse of demand, increased uncertainty, and disruption in supply chains, from the analysis of their earning calls. Gourinchas et al. (2020) and Carletti et al. (2020) provide ex-ante forecasts of the impact of Covid-19 on financial distress and business failures for small and medium sized enterprises. Alfaro et al. (2020) show that unanticipated changes in predicted infections forecast aggregate equity market returns. Ramelli and Wagner (2020), Ding et al. (2021), Albuquerque et al. (2020), and Martin and Nagler (2020) relate stock returns during the first quarter of 2020 to a variety of firm characteristics. Ru et al. (2021) and Croce et al. (2020) study the diffusion of Covid-19 related information and risk in financial markets.

The rest of the paper is structured as follows. Section 2 presents the data and Section 3 details the construction of the firm-level and CZ-level exposure to mandatory business closures. Section 4 discusses the empirical strategy. We present the effects on firms' profits in Section 5, labor income in Section 6, and health outcomes in Section 7. Section 8 discusses the economic significance of the results and Section 9 concludes.

2. Data

We first present manually compiled granular data on state-mandated business closures. In the rest of the section, we list other firm and CZ-level datasets used for the purpose of our empirical analysis.

2.1. Mandatory business closures in the United States

Covid-19 first spread to the U.S. in January 2020. A Public Health Emergency was declared on January 31 by the federal government, and a National Emergency was declared on March 13. On March 19, the Department of State advised U.S. citizens to avoid all international travels. In turn, U.S. state governors started issuing Executive Orders restricting social activities. Those vary across states and include stay-at-home orders, bans on public gatherings, out-of-state travel restrictions, and the closures of schools, daycares, bars, sit-down restaurants, and certain business activities.

In this paper, we focus on Executive Orders closing businesses deemed as non-essential, aside from restaurants that are closed for dine-in in virtually all states. We read the Executive Orders of each state to obtain the list of sectors forced to shut-down physical operations, the issuance date, the effective date, the initial expiry date and subsequent amendments. As evidenced in Table 1, 45 states issued such orders between March 19 (California) and April 6 (Missouri). 35 five of them had an explicit end date. All but three where then extended.

[2] Other recent papers on this topic include Davis et al. (2022), Kim et al. (2020), Crucini and O'Flaherty (2020), Bretscher et al. (2020), Fairlie (2020), Gupta et al. (2020), Spiegel and Tookes (2021), McLaren and Wang (2020), Li and Strahan (2020), Bloom et al. (2021), Levine et al. (2020), Fairlie and Fossen (2022), Song et al. (2021), Erel and Liebersohn (2022), Favilukis et al. (2021), Bognanni et al. (2020), Coibion et al. (2020), Baker et al. (2020), among others.

Table 1
Timing of initial Executive Orders restricting business activities. This table presents the issue date, effective date, and end date of the initial Executive Order of each U.S. state restricting business activities aside from restaurants and recreational facilities.

State	Issue date	Effective date	End date	Extended?	Eased date	End date
ALABAMA	April 3, 2020	April 4, 2020	April 30, 2020			
ALASKA	March 27, 2020	March 28, 2020	April 11, 2020	1	April 24, 2020	
ARIZONA	March 30, 2020	March 31, 2020	April 30, 2020	1	May 16, 2020	
ARKANSAS						
CALIFORNIA	March 19, 2020	March 19, 2020			May 8, 2020	
COLORADO	March 25, 2020	March 26, 2020	April 11, 2020	1	April 27, 2020	
CONNECTICUT	March 20, 2020	March 23, 2020	April 22, 2020	1	May 20, 2020	
DELAWARE	March 22, 2020	March 24, 2020	May 15, 2020		May 8, 2020	
DC	March 24, 2020	March 25, 2020	April 24, 2020	1		
FLORIDA	April 1, 2020	April 3, 2020	April 30, 2020	1	May 4, 2020	
GEORGIA	April 2, 2020	April 3, 2020	April 13, 2020	1	April 24, 2020	
HAWAII	March 24, 2020	March 25, 2020	April 30, 2020	1	May 7, 2020	
IDAHO	March 25, 2020	March 25, 2020	April 15, 2020	1	May 1, 2020	
ILLINOIS	March 20, 2020	March 21, 2020	April 7, 2020	1		
INDIANA	March 23, 2020	March 25, 2020	April 6, 2020	1	May 2, 2020	
IOWA						
KANSAS	March 28, 2020	March 30, 2020	April 19, 2020	1	May 4, 2020	
KENTUCKY	March 25, 2020	March 26, 2020			May 11, 2020	
LOUISIANA	March 22, 2020	March 23, 2020	April 13, 2020	1	May 15, 2020	
MAINE	March 24, 2020	March 25, 2020	April 8, 2020	1	May 1, 2020	
MARYLAND	March 23, 2020	March 23, 2020			May 7, 2020	
MASSACHUSETTS	March 23, 2020	March 24, 2020	April 7, 2020	1	May 18, 2020	
MICHIGAN	March 23, 2020	March 24, 2020	April 13, 2020	1	May 7, 2020	
MINNESOTA	March 25, 2020	March 28, 2020	April 10, 2020	1	April 27, 2020	
MISSISSIPPI	April 1, 2020	April 3, 2020	April 20, 2020	1	April 27, 2020	
MISSOURI	April 3, 2020	April 6, 2020	April 24, 2020	1	May 4, 2020	
MONTANA	March 26, 2020	March 28, 2020	April 10, 2020	1	April 27, 2020	
NEBRASKA						
NEVADA	March 20, 2020	March 21, 2020	April 16, 2020	1	May 9, 2020	
NEW HAMPSHIRE	March 26, 2020	March 28, 2020	May 4, 2020	1	May 4, 2020	
NEW JERSEY	March 21, 2020	March 21, 2020				
NEW MEXICO	March 23, 2020	March 24, 2020			May 15, 2020	
NEW YORK	March 20, 2020	March 22, 2020	April 17, 2020	1	May 15, 2020	
NORTH CAROLINA	March 27, 2020	March 30, 2020	April 29, 2020	1	May 8, 2020	
NORTH DAKOTA						
OHIO	March 22, 2020	March 24, 2020	April 6, 2020	1	May 4, 2020	
OKLAHOMA	March 24, 2020	March 26, 2020	April 30, 2020	1	April 24, 2020	
OREGON	March 23, 2020	March 24, 2020			May 15, 2020	
PENNSYLVANIA	March 19, 2020	March 23, 2020			May 8, 2020	
RHODE ISLAND	March 28, 2020	March 30, 2020	April 13, 2020	1	May 9, 2020	
SOUTH CAROLINA	March 31, 2020	April 1, 2020	April 15, 2020	1	May 18, 2020	
SOUTH DAKOTA						
TENNESSEE	March 30, 2020	April 1, 2020	April 14, 2020	1	May 8, 2020	
TEXAS	March 31, 2020	April 2, 2020	April 30, 2020		May 1, 2020	
UTAH						
VERMONT	March 24, 2020	March 25, 2020	April 15, 2020	1	May 4, 2020	
VIRGINIA	March 23, 2020	March 25, 2020			May 15, 2020	
WASHINGTON	March 23, 2020	March 25, 2020	April 8, 2020	1	May 15, 2020	
WEST VIRGINIA	March 23, 2020	March 24, 2020			May 4, 2020	
WISCONSIN	March 24, 2020	March 25, 2020	April 24, 2020	1	April 29, 2020	May 13, 2020
WYOMING						

To the best of our knowledge, this is the first study to compile a granular database at the 4-digit sector × state level providing information on whether and when a given industry in a given state has been administratively shut down.[3]

States vary significantly in the type of businesses they decide to close. Some of them defined essential and non-essential businesses following and adapting the guidelines initially issued on March 19 by the Cybersecurity, Infrastructure and Security Agency (CISA).[4] Some states published the list of sectors that may or may not continue physical operations according to the North American Industry Classification System (NAICS). See, for instance, Online Appendix Figure A.1 for an extract of the state of Pennsylvania's list of "life sustaining businesses" attached to the Executive Order signed by Governor Tom Wolf on March 19. In such cases, we directly map these sectors to the data using NAICS codes. Other states listed sectors to be closed without an explicit reference to an industry classification. In such cases, we manually map listed sectors to NAICS codes.

For each state, the raw data that we compiled is a dummy $Closed_{ind,state}$ that equals 1 if the 4-digit NAICS industry ind has been

[3] We use an aggregated version of the same data in a companion paper on the network effects of social distancing measures, see Barrot et al. (2021). Other work compiled data on business closures at the industry level in other countries, see for instance Bongaerts et al. (2021), Borri et al. (2021) and Porto et al. (2021) for Italy. Our database provides us with both variation across states in the set of industries that have been closed, and across time as business closures have been implemented and eased at different point in time for each state.

[4] https://www.cisa.gov/publication/guidance-essential-critical-infrastructure-workforce.

classified as non-essential (and is therefore mandated to close) in the state's Executive Order, together with the associated beginning and end dates.[5] For the analysis of the effect of business closures on stock returns, we also exploit the initial announcement date of these business closures.

Importantly, we focus our analysis on the first Covid-19 wave, and do not consider the effect of business closures that might have been in place starting in fall of 2020. We make this choice for two reasons. First, whereas the first wave of business closures have been eased or ended in April and May of 2020, some states have reintroduced some business closures in the fall, that are hard to track in an exhaustive way. Second, we need to control for other within-state social distancing measures decided during the first wave. We do so by controlling for mask mandates and stay-at-home orders at the county-level.[6] However, social distancing measures implemented during the second wave applied to a larger set of domains and got more fine-tuned over time, so that they are practically impossible to track and measure in a proper way. For instance, California introduced a color-coded county tier system in the fall of 2020, where restrictions varied continuously according to county-level new cases per 100,000 population and test positivity rate.

2.2. Firm outcomes

To estimate the effect of business closures on firms' outcomes, we gather financial data for publicly listed companies from the Compustat North America Fundamentals Quarterly database over the sample period 2019Q1-2020Q3. We do not include data beyond 2020Q3 in order to focus on the first wave of Covid-19. We exclude financial firms (those with NAICS codes starting with 2-digit "52"). All continuous variables are winsorized at the 1st and 99th percentiles of their distributions. Our final sample includes 1,561 distinct firms.

Importantly, we exploit information gathered by Infogroup for the year 2018 to identify firms' headquarter state location as well as the employment, industry and state location of each of their establishments. Infogroup is a crucial source of information for our analysis in that it allows us to measure precisely the share of employment of each firm subject to labor restrictions.[7]

2.3. Labor and health outcomes

To estimate the effect of business closures on labor and health outcomes, we consider commuting zones (CZs) as the relevant unit of analysis. Developed by Tolbert and Sizer (1996) using county-level commuting data from the 1990 Census data, CZs are clusters of counties that are characterized by strong within-cluster and weak between-cluster commuting ties and therefore represent local labor markets. They cover the entire land area of the U.S.[8]

Wages, employment and hours worked. We exploit high-frequency information on wages, employment and hours worked at small businesses using data from Homebase. Homebase provides corporate clients with virtual scheduling and time-tracking tools. Its client base loads on small businesses in sectors such as retail, restaurant, and accommodation.

We use anonymized data on wages, employees and hours worked at Homebase clients at the establishment-worker-day level, that we then aggregate to the CZ-week level. We restrict our attention to employees of firms using Homebase at the end of year 2019. We use employment data from Homebase in year 2020 from week 1 (beginning of January) to week 44 (end of October). The benefit of using Homebase data is that it is available at the weekly frequency, which is ideal to study the impact of business closures on Covid-19 infection and death rates. The drawback is that Homebase clients are not representative of the employment distribution. As a first step to correct for the over-representation of certain industries in Homebase, we reweight the Homebase data to match industry shares in the general population of local firms in the County Business Patterns (CBP) 2019 File for each county. For this, we first aggregate industry categories in the CBP to match the Homebase 10 industry categories.[9] We then compute CZ-level wages, employment, and hours in Homebase data by taking the weighted average across sectors, using weights from the CBP.

Another limitation of the Homebase data is that it covers small businesses only, and it is therefore not representative of the firm-size distribution. To get around this limitation, we run robustness tests using employment data from the U.S. Bureau of Labor Statistics (BLS). BLS data includes employers covering more than 95 percent of U.S. jobs available, and is therefore representative of the U.S. economy. Yet it is only available at the monthly frequency – unlike weekly-level employment data from Homebase. The BLS reports monthly employment defined as the number of workers who worked during the period that included the 12th day of the month. We use monthly employment data available at the county level for employees in the private-sector, that we then aggregate to the CZ-month level (from January to October 2020).

Covid-19 infections, hospitalizations and deaths. We retrieve daily county-level counts of Covid-19 cases and fatalities in the United States from Johns Hopkins University Center for Systems Science and Engineering (CSSE).[10] Our data on Covid-19 hospitalizations are drawn from Harris et al. (2021).[11] We then aggregate the data on Covid-19 infections, hospitalizations and deaths to the CZ-week level. As for data on employment and hours described above, we focus on the period from week 1 to week 44 of 2020.

Mental health. We gather data on symptoms of bad mental health (anxiety, being worried, feeling down, having low interest for things) using information from the Household Pulse Survey. As described in more details in Buffington et al. (2021), the Household Pulse Survey was developed by the Census Bureau in collaboration with other federal agencies to provide high-frequency data on a range of ways in which

[5] There are 310 4-digit NAICS industries.

[6] To the extent that these county-level variations are uncorrelated with our local measures of exposure to business closures, they should simply introduce noise in the estimation. Reassuringly, our estimates are similar when we control for both mask mandates and stay-at-home orders in our regressions.

[7] In our sample, 45% of firms' employees are located in different states than the headquarters. Infogroup makes phone calls to establishments to gather, among other data items, the number of full-time equivalent employees. Note that Compustat only records the last available location of the headquarters of each firm, and does not provide information on the location of their establishments.

[8] CZs are the natural geographical units for estimating the causal impact of labor restrictions on Covid-19 infections and mortality rates. First, workers commute mainly within commuting zones. Because we estimate the impact of business closures on health outcomes at the CZ level, we are able to estimate the overall effect of business closures even if there are local externalities across counties of the same CZ. One concern is that individuals can still move across CZs, and therefore business closures in one CZ could still generate positive externalities on neighboring areas. We directly identify the size of local spillovers in Section 7.

[9] We match industry categories in the CBP to the Homebase category "Other" when we do not find a suitable match in the 9 other Homebase industry categories: Beauty & Personal Care, Charities, Education & Membership, Food & Drink, Health Care and Fitness, Home and Repair, Leisure and Entertainment, Professional Services, Retail, Transportation.

[10] Available at https://github.com/CSSEGISandData/COVID-19.

[11] We thank the authors for kindly agreeing to share the data with us. Information on Covid-19 hospitalizations are from "Change Healthcare", the nation's largest claims clearinghouse with a network of 900,000 providers and 5,500 hospitals across the country, processing nearly 55 percent of all commercial claims (including Medicaid Managed Care and Medicare Advantage, but not Medicare FFS) in the U.S for nearly 170,000,000 unique individuals. See Harris et al. (2021) for more details on the data. We received aggregated export approved data from the authors, without ever accessing the patient records.

people's lives were impacted by the Covid-19 pandemic, including mental health. The data is available from week 17, the last week of April 2020.

Mortality data. We retrieve overall (Covid-19 and non-Covid-19 related) mortality data for the year 2020 compiled by the National Center for Health Statistics at the Centers for Disease Control and Prevention (CDC) at the county level.[12] Unlike data on Covid-19 infections, hospitalizations and deaths that we observe at the weekly frequency, CDC mortality data are publicly available at the monthly frequency only. One strength of the data is that it includes information on total deaths by gender, age group, and county of residence. Combined with life expectancy tables by gender and age,[13] this allows us to construct estimates for the effect of business closures on the potential number of years of life lost at the CZ-monthly level. Similarly, we use data on residual age- and sex-specific quality-adjusted life-years for the U.S. from Palmer et al. (2022) in order to construct estimates for the effect of business closures on the number of quality-adjusted life-years lost at the CZ-monthly level.[14]

Other CZ-level data. We compute the Trump vote share in 2016 at the CZ level using MIT Election Data. We retrieve the share of urban population in 2010 from the 2010 Census Urban and Rural Classification, population density in 2019 and net migrations in 2019 from the County Population Estimates File, the share of the population with less than a high school diploma, the share of the population above 65 years old, and the logarithm of median household income from the Census Bureau. We obtain the number of intensive care unit (ICU) beds per inhabitants and the logarithm of one plus the number of hospitals from the HIFLD, and the 2019 employment ratio and unemployment rate from the Bureau of Labor Statistics.

We construct an index of contact-proximity at work for each 4-digit NAICS sectors following the same methodology used by Dingel and Neiman (2020) to compute the share of jobs that can be done at home. Specifically, we retrieve from O*NET surveys an index of contact-proximity for each occupation. Formally, we compute the share of respondents answering "Moderately close (at arm's length)" or "Very close (near touching)" to the question: "To what extent does this job require the worker to perform job tasks in close physical proximity to other people?" We then merge this classification of occupations with information on the prevalence of each occupation in each NAICS industry.

To control for the effects of school closures on the workforce with dependent children and therefore forced into inactivity, we use data from the American Community Survey (ACS). We compute for each state and sector the share of working people with children under 15.[15]

Finally, we construct CZ-level measures of the fraction of the population that is subject to mask mandates and stay-at-home orders.[16] For that purpose, we use the county-level dataset on mask mandates from Wright et al. (2020), as well as the county-level dataset on stay-at-home orders from Killeen et al. (2020). For each CZ and week, we compute the fraction of the population that is subject to mask mandate or stay-at-home orders by aggregating the county-level population that is affected by these policies according to their beginning and end dates.

3. Exposure to mandatory business closures

In the first part of this section, we describe how we compute the share of restricted labor at the state×sector level, combining information on business closures and the share of workers who work from home. We then use the data to obtain firm-level and CZ-level measures of exposure to mandatory business closures.

3.1. Restricted labor force

Formally, our measure of restricted labor in a given industry ind and state $state$ is:

$$Restricted\ Labor_{ind,state} = Closed_{ind,state} \cdot (1 - \textit{work-from-home}_{ind}) \tag{1}$$

where $Closed$ is a dummy that equals 1 if the industry has been classified as non-essential (and therefore closes) in the state's Executive Order, and *work-from-home* is the share of workers who work-from-home in this industry. We approximate *work-from-home* using the actual fraction of employees working from home in the year 2020 using data from the American Time Use Survey.[17] Equation (1) represents the share of workers who cannot work in a given industry and state due to the closure of this industry mandated by a given state's Executive Order.

3.2. Firm-level exposure

We use information from Infogroup on the employment counts, industry and state of location of firms' establishments, and compute the employment weight of firm f in industry ind and state $state$, defined as:

$$\omega^{f}_{ind,state} = \frac{Emp^{f}_{ind,state}}{\sum_{ind}\sum_{state} Emp^{f}_{ind,state}}$$

with $Emp^{f}_{ind,state}$ is the total number of employees of firm f working in industry ind and state $state$. By construction, $\sum_{ind}\sum_{state}\omega^{f}_{ind,state} = 1$. To compute the employment-weighted restricted labor share of each firm, we sum the employment weights $\omega^{f}_{ind,state}$ of 4-digit NAICS sectors that are closed in accordance with the Executive Orders of each state, adjusted from the share of workers who cannot work from home. Formally, we define the restricted labor share of firm f as:

12 Available at https://wonder.cdc.gov/ucd--icd10.html.

13 We use the 2019 life tables by gender and age from the National Vital Statistics Reports, available at https://www.cdc.gov/nchs/data/nvsr/nvsr70/nvsr70-19.pdf. Potential years of life lost are computed by taking the product of the monthly number of fatalities in a given county-age-gender bin and the residual life estimate for each gender and age group, which is evaluated in the middle of the age range and taken from the U.S. Social Security administration period life table, and then aggregated at the CZ-month level.

14 Specifically, we use data by gender and age in Table 1 of Palmer et al. (2022). For instance, residual quality-adjusted life-years for men at age 50 is 24 whereas the residual life estimate in life expectancy tables is 30 years. As for years of life lost, we compute quality-adjusted life-years lost by taking the product of the monthly number of fatalities in a given county-age-gender bin and the estimates on age- and sex-specific quality-adjusted life-years from Palmer et al. (2022) for each gender and age group, and then aggregated at the CZ-month level.

15 More specifically, we consider that an active person has dependent children if there is not another inactive person in the household who could take care of them. If there are several active adults in the household, we consider that the lowest earning adult is in charge of childcare.

16 Goolsbee et al. (2020) and Spiegel and Tookes (2021) have shown that local policies at the county level are important to understand the economic impact of Covid-19.

17 Specifically, we follow the same procedure as the one described in Hensvik et al. (2020), who computed the actual fraction of employees working from home in the year 2019 (though not surprisingly, the fraction of employees declaring working from home is substantially larger in the year 2020 than in 2019). See also Papanikolaou and Schmidt (2022) for empirical evidence on the disruptive effect of Covid-19 depending on the fraction of workers who can work remotely in each industry using data from the American Time Use Survey. It turns out that the actual share of employees working from home according to the American Time Use Survey is similar to the share of workers who can work from home according to Dingel and Neiman (2020).

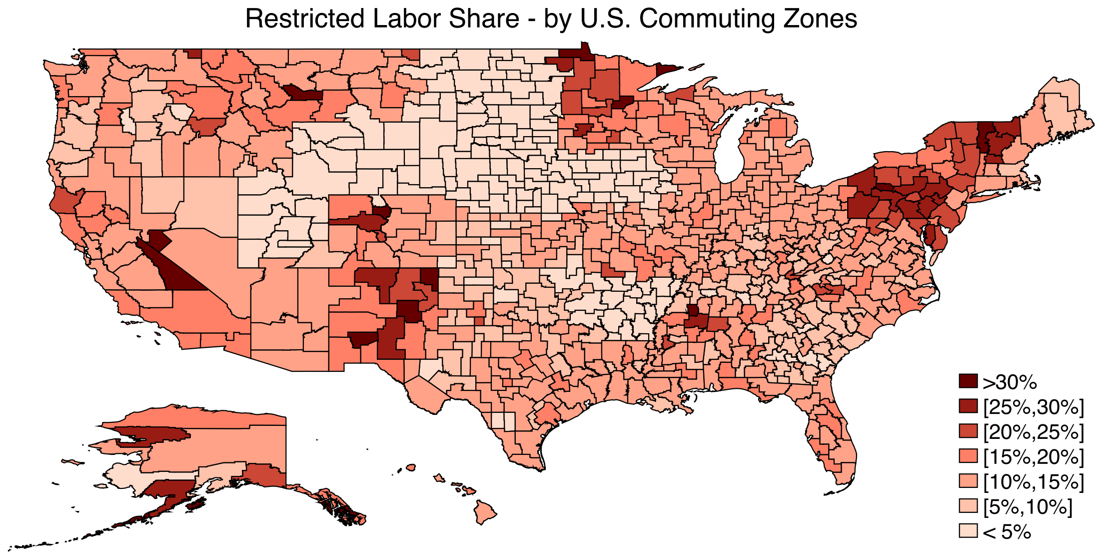

Fig. 1. Note: This map presents the restricted labor share in each U.S. commuting zone. (For interpretation of the colors in the figure(s), the reader is referred to the web version of this article.)

$$Restricted\ Labor_f = \sum_{ind}\sum_{state} \omega^f_{ind,state} \cdot Closed_{ind,state} \cdot (1 - \textit{work-from-home}_{ind}) \quad (2)$$

3.3. CZ-level exposure

In order to measure any given CZ's exposure to state-mandated business closures, we exploit pre-determined industry composition, using employment data from the Census' County Business Patterns in 2019. Let us denote the employment weight of commuting-zone CZ[18] in industry ind and county c:

$$\omega^{cz}_{ind,c} = \frac{Emp_{ind,c}}{\sum_{ind}\sum_{c\in cz} Emp_{ind,c}}$$

with $\sum_{ind}\sum_{c\in cz}\omega_{ind,c} = 1$. To compute the employment-weighted restricted labor share in each CZ, we sum the employment weights $\omega^{cz}_{ind,c}$ of 4-digit NAICS sectors that are closed in accordance with state-level Executive Orders across all counties of a given CZ, adjusted from the share of workers working from home. Formally, we define the restricted labor share of commuting zone cz as:

$$Restricted\ Labor_{cz} = \sum_{ind}\sum_{c\in cz} \omega_{ind,c} \cdot Closed_{ind,c,state} \cdot (1 - \textit{work-from-home}_{ind}) \quad (3)$$

3.4. Summary statistics

Panel A of Table 2 presents summary statistics of U.S. publicly listed firms' outcomes at the quarterly frequency between 2019Q1 and 2020Q3. To study the dynamics of firms' outcomes over this period, we scale firms' assets (Compustat quarterly item ATQ) and profits (Compustat quarterly item OIBDPQ) in a given quarter using total assets in 2018 in the same quarter, and scale sales (Compustat quarterly item SALEQ) by its value in 2018 in the same quarter.

Panel B of Table 2 presents summary statistics for our panel of 739 CZs over 44 weeks between January and October 2020.[19] We find substantial heterogeneity in the employment-weighted share of restricted labor across CZs. The average share of restricted labor across CZs is 9.2%, with a median of 10.2% and a standard deviation of 8.3%. Fig. 1 presents the distribution of the restricted labor share for each CZ across the U.S., which equals 0 for CZs in states without Executive Orders, and is positive otherwise. Within states with Executive Orders, differences in sectoral composition across CZs of the same state yield substantial heterogeneity in their restricted labor share.

There is also substantial heterogeneity across states in the duration of business closures, as evidenced in Fig. 2. 44 out of 51 states have issued an executive order over the sample period. Business closures are on average active for 43 days before they are either terminated or eased, see Table 1. The dummy $I_{ShutDown}$ captures periods in which executive orders are active.

4. Empirical strategy

We aim to estimate the causal effect of state-mandated business closures on profits, labor income and health outcomes, at the firm and CZ-level.

4.1. Firm-level analysis

To estimate the effect of business closures on firms' outcomes, we rely on the location and industry of their activities, and whether or not they are targeted by state-mandated business closures. Specifically, we run the following panel regressions at the firm×quarter level:

$$Y_{f,t} = \mu + \xi . Restricted\ Labor_f \times I_{ShutDown,t} + \sigma_f + \delta_{ind\times t} + \tau_{state\times t} + \epsilon_{f,t} \quad (4)$$

where $Y_{f,t}$ is the outcome variable at the firm f × quarter t level. $Restricted\ Labor_f$ is the share of firm f workers in closed sectors that cannot work from home, and $I_{ShutDown}$ is a dummy variable equal to

[18] Similarly, we define the payroll weight of commuting-zone CZ in industry ind and county c, and construct a payroll-weighted restricted labor share for each CZ that we then use in our specifications on wages at the CZ level.

[19] The data on employment at the 4-digit NAICS codes is not available in the County Business Pattern 2019 File for 2 commuting zones, that are therefore excluded from the analysis. These 2 commuting zones represent less than 0.001% of U.S. aggregate employment.

Table 2
Summary statistics. Panel A of this table presents summary statistics for our firm sample at the quarterly frequency over the sample period 2019Q1-2020Q3. All variables are winsorized at the first and ninety-ninth percentiles of their distributions. Panels B, C, and D present summary statistics for our CZ sample. Panel B presents the variables for each of the 739 CZs. Panel C (respectively Panel D) presents outcome variables and the restricted labor share at the weekly frequency (respectively monthly frequency) across the 739 CZs over the sample period 2020Jan-2020October. All continuous variables are winsorized at the first and ninety-ninth percentiles of their distributions.

	count	mean	sd	p1	p50	p99
	Panel A: Firm level					
Assets/Assets$_{2018}$	10,702	1.190	0.411	0.519	1.084	2.890
Sales/Sales$_{2018}$	10,702	1.072	0.440	0.084	1.019	3.305
Income/Assets$_{2018}$	10,702	0.015	0.060	-0.262	0.025	0.131
Restricted Labor × $I_{ShutDown}$	10,702	0.029	0.120	0.000	0.000	0.767
	Panel B: CZ level					
Restricted Labor Share	739	0.092	0.083	0.000	0.102	0.330
Pop Density	739	0.093	0.160	0.000	0.039	0.995
Share Urban Pop	739	0.506	0.254	0.000	0.529	0.967
Migration 2019	739	-0.001	0.009	-0.024	-0.001	0.021
Employment Ratio	739	0.458	0.062	0.299	0.458	0.621
Work-From-Home Share	739	0.269	0.045	0.111	0.268	0.382
Contact-Intensive Share	739	0.591	0.030	0.525	0.591	0.683
Kids Share	739	0.132	0.017	0.090	0.132	0.198
Share Less High School Diploma 2018	739	0.086	0.035	0.034	0.080	0.187
Share 65+ Years Old	739	0.187	0.042	0.107	0.183	0.292
Median HH Income 2018 ('000s)	739	52.377	10.674	32.135	51.197	86.604
ICU beds/Pop	739	0.172	0.135	0.000	0.166	0.619
Ln(1+NbHospitals)	739	1.840	0.875	0.000	1.792	4.466
Donald Trump Vote Share (2016)	739	0.611	0.143	0.249	0.624	0.862
Migration 2020 (March-September)/Pop 2019	739	-0.002	0.017	-0.045	-0.002	0.042
	Panel C: CZ-Week level					
New Covid-19 Infections per 10,000	27,994	7.387	12.774	0.000	2.435	60.104
New Covid-19 Deaths per 10,000	27,994	0.133	0.295	0.000	0.000	1.351
New Covid-19 Hospitalizations per 10,000	27,994	3.337	5.617	0.000	1.534	27.966
Log(Employment)	27,994	10.899	1.470	7.684	10.772	14.495
Log(Wages)	27,994	15.872	1.669	11.610	15.873	19.815
Log(Hours)	27,994	14.084	1.529	10.588	13.995	17.772
Restricted Labor × $I_{ShutDown}$	27,994	0.044	0.072	0.000	0.008	0.275
$I_{ShutDown}$	27,994	0.043	0.204	0.000	0.000	1.000
Stay At Home Share	27,994	0.155	0.360	0.000	0.000	1.000
Mask Share	27,994	0.246	0.401	0.000	0.000	1.000
	Panel D: CZ-Month level					
Total Deaths per 10,000	4,800	8.126	2.775	2.079	8.024	15.080
Years of Life Lost per 10,000	4,800	126.300	44.972	30.141	124.563	239.098
Quality-adjusted Years of Life Lost per 10,000	4,800	96.620	34.187	23.178	95.144	182.338

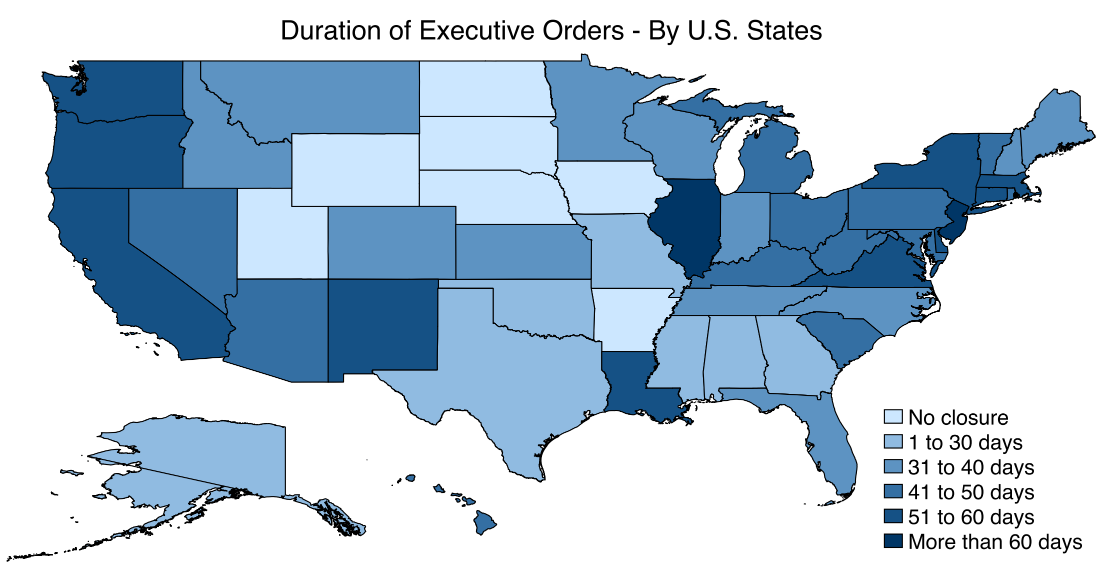

Fig. 2. Note: This map presents the duration of business closures of each U.S. state. The duration is computed as the difference between the Ease Date or End Date and the Effective Date of Executive Orders as described in Table 1.

one for the quarters 2020Q1-2020Q3 over which labor restrictions are active. σ_f is a vector of firm fixed effects, $\delta_{ind \times t}$ are sector×quarter fixed effects, $\tau_{state \times t}$ are state×quarter fixed effects, and $\epsilon_{f,t}$ is the error term. Standard errors are clustered both at the main industry of the firm, and at the state level of each firm headquarters. The coefficient of interest, ξ, measures the causal effect of labor restrictions on the firms' outcomes we consider. The sample period is 2019Q1-2020Q3.

The identifying assumption is that *in the absence of* business closures, affected firms would have behaved similarly than control firms. While this assumption cannot be formally tested, we check for parallel trends in the quarter prior to the implementation of business closures.

4.2. CZ-level analysis

For CZ-level analyses, our empirical strategy closely tracks the firm-level analysis and approximates the following example. Pennsylvania filed an Executive Order on March 19, effective on March 23, which closed firms in Textile, but left open firms in Food Manufacturing. We estimate the impact of these business closure decisions across CZs in Pennsylvania, depending on their local historical industry composition. Take two CZs in Pennsylvania denoted CZ_A and CZ_B, with CZ_A disproportionately populated by Textile firms, and CZ_B disproportionately populated by Food Manufacturing firms, in such a way that a larger fraction of the workforce in CZ_A cannot work after March 23 (and at least until May 8, date at which the closing measures are eased) as compared to CZ_B. When saturated with state × year fixed effects, our identification strategy boils down to comparing the differential effect of the larger exposure to business closures post-March 23 in CZ_A compared to CZ_B *within* the same state of Pennsylvania.

Formally, our identification is akin to a difference-in-differences framework with continuous treatment in which we estimate the differential impact of business closures on labor income and health outcomes over periods in which sectors have been *effectively* shut down.

Labor income. To estimate the effect on labor income, we run the following panel regressions at the CZ×week level:

$$\begin{aligned} Y_{cz,state,t} =& \mu + \xi.Restricted\ Labor_{cz,t} + \rho.X_{cz,t} \\ &+ \sigma_{cz} + \tau_{state \times t} + \epsilon_{cz,state,t} \end{aligned} \tag{5}$$

where $Y_{cz,state,t}$ is either wages, employment, or hours, in commuting zone cz in week t. $Restricted\ Labor_{cz,t}$ is commuting zone cz share of workers in closed sectors that cannot work from home in week t. $X_{cz,t}$ is a vector of CZ-level controls interacted with dummies for active restrictions in week t. Control variables include the share of population in urban areas, population density, initial infection rates, the share of work-from-home occupations, the share of contact-intensive occupations, the kids share, net migrations in 2019, the share of the population with less than a high school diploma, the share of the population above 65 years old, the logarithm of median household income, the number of ICU beds per inhabitants, the logarithm of one plus the number of hospitals, and Donald Trump Vote Share in 2016. $X_{cz,t}$ also includes the share of the CZ population subject to local mask mandates and stay-at-home orders. σ_{cz} is a vector of CZ fixed effects, and $\tau_{state \times t}$ are state×week fixed effects.[20] Standard errors are clustered at both state and week levels. The sample period is January to October 2020.

Health outcomes. To estimate the effects of business closures on health outcomes (such as Covid-19 infections, hospitalizations, and deaths), we build on prior work in the empirical literature estimating the effects of non-pharmaceutical restrictions on health outcomes (such as Adda (2016); Chernozhukov et al. (2021); Karaivanov et al. (2021)), and use an empirical specification consistent with a simple SIRD epidemiological model for the spread of Covid-19.

Formally, let us call I_t the number of new Covid-19 infections at period t, and S_t the number of susceptible at period t. The evolution of newly infected in a discrete time SIRD model is governed by the following equation:

$$I_{t+1} - I_t = \alpha I_t S_t - \beta I_t \tag{6}$$

where α is the contact rate parameter and β is the recovery rate. We assume that S_t is close to one, in order to approximate the early phase of the pandemic where almost everyone was susceptible. Rearranging the above equation leads to the following lag-dependent equation,

$$I_{t+1} = (1 + \alpha - \beta) I_t. \tag{7}$$

In a similar way as Chernozhukov et al. (2021), we incorporate this equation into a dynamic panel data model for Covid-19 infections at the CZ × week level, where we include our dynamically evolving measure of the share of restricted labor, $Restricted\ Labor_{cz,t}$, together with CZ-level and state-week fixed effects.

Formally, we run the following regression at the CZ × week level for new Covid-19 infections, I:

$$\begin{aligned} I_{cz,state,t+1} =& \mu + \xi.Restricted\ Labor_{cz,t} + \gamma.I_{cz,state,t} \\ &+ \rho.X_{cz,t} + \sigma_{cz} + \tau_{state \times t} + \epsilon_{cz,state,t} \end{aligned} \tag{8}$$

As in Equation (5), $Restricted\ Labor_{cz,t}$ is commuting zone cz share of workers in closed sectors that cannot work from home in week t, $X_{cz,t}$ is the same vector of CZ-level controls interacted with dummies for active restrictions in week t, σ_{cz} is a vector of CZ fixed effects, and $\tau_{state \times t}$ are state×week fixed effects. Standard errors are clustered at both state and week levels. The sample period is January to October 2020.

To estimate the effect of business closures on hospitalization $H_{i,t}$ and mortality rates $D_{i,t}$ at date t, we consider that infections materialize into hospitalizations at a rate ζ and that infections materialize into deaths with a one-period lag at a rate η, that is, $H_t = \zeta I_t$ and $D_{t+1} = \eta I_{i,t}$. These assumptions motivate the same empirical specifications for Covid-19 hospitalization and Covid-19 deaths at the CZ×week level as the one presented in Equation (8) in which the dependent variable $I_{cz,state,t+1}$ is replaced by respectively $H_{cz,state,t+1}$ and $D_{cz,state,t+2}$.

Finally, we estimate Equation (8) where the dependent variable refers respectively to the total number of deaths (Covid-19 related and non-related), and to the total number of years of life lost, but in specifications run at the monthly frequency. Accordingly, in order to keep the same 2-weeks lag between the measurement of business closures and their hypothesized effect on total deaths and total number of years of life lost in these specifications run at the monthly frequency, we compute the variable $Restricted\ Labor$ as the average of its weekly counterpart over 4 weeks, with a lag of 2 weeks compared to the measurement of total deaths and total years of life lost. In the same way, we control for the sum of new Covid-19 infections I over 4 weeks, with a lag of 2 weeks compared to the measurement of total deaths and total years of life lost. $X_{cz,m-1}$ is a vector of CZ-level controls interacted with dummies for active restrictions in month m, σ_{cz} is a vector of CZ fixed effects, and $\tau_{state \times m}$ are state×month fixed effects. Standard errors are clustered at both state and month levels, and the sample period is January to October 2020.[21]

Note that relative to a linear model, the SIRD model fits the dynamics of an epidemic in a concise but accurate way (Kermack and

[20] When a commuting zone overlaps more than one state, we attribute the commuting zone to the most important state in terms of employment shares.

[21] Formally, let us define $D_{cz,m}$ the monthly total number of deaths for commuting zone cz in month m, such that $D_{cz,m} \equiv \sum_{t=m_1}^{m_4} D_{cz,t} = \sum_{t=(m-1)_3}^{m_2} D_{cz,t+2}$ where m_i is the week $i \in \{1,2,3,4\}$ of month m. The analogue of Equation (8) at the monthly frequency then uses the monthly version of the right-hand side variables $Restricted\ Labor$ and I such that $Restricted\ Labor_{cz,m-1} \equiv \frac{1}{4}\sum_{t=(m-1)_3}^{m_2} Restricted\ Labor_{cz,t}$ and $I_{cz,m-1} \equiv \sum_{t=(m-1)_3}^{m_2} I_{cz,t}$.

Table 3

Restricted labor and firms' outcomes. This table presents estimates from panel regressions of individual firm sales (Panel A), assets (Panel B) and profits (Panel C) on the restricted labor share of the firm when business closures are active (2020-Q1 to 2020-Q3). In Panel A, sales are scaled by the firm's sales in 2018 in the same quarter of the year. In Panel B and C, assets and profits are scaled by the firm's assets in 2018 in the same quarter of the year. All regressions include firm fixed effects. We present results with quarter fixed effects (column 1), sector-quarter fixed effects (column 2), state-quarter fixed effects (column 3) and both sector-quarter and state-quarter fixed effects (column 4). In column 5, we control for the share of the firm employment that is subject to county-level mask mandates and stay-at-home orders. Standard errors presented in parentheses are clustered both at the firms' main industry and state levels. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Panel A: $Sales/Sales_{2018}$				
Restricted Labor $\times I_{ShutDown}$	-0.306***	-0.213**	-0.319***	-0.225**	-0.227**
	(0.057)	(0.091)	(0.058)	(0.087)	(0.088)
Quarter FE	Yes	No	No	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes
Sector × Quarter FE	No	Yes	No	Yes	Yes
State × Quarter FE	No	No	Yes	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	Yes
Obs.	10,702	10,702	10,702	10,702	10,702
R^2	0.597	0.651	0.615	0.660	0.660
	Panel B: $Assets/Assets_{2018}$				
Restricted Labor $\times I_{ShutDown}$	-0.087	-0.091	-0.126***	-0.119**	-0.119**
	(0.057)	(0.055)	(0.045)	(0.053)	(0.055)
Quarter FE	Yes	No	No	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes
Sector × Quarter FE	No	Yes	No	Yes	Yes
State × Quarter FE	No	No	Yes	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	Yes
Obs.	10,702	10,702	10,702	10,702	10,702
R^2	0.726	0.759	0.736	0.765	0.765
	Panel C: $Profits/Assets_{2018}$				
Restricted Labor $\times I_{ShutDown}$	-0.016***	-0.015**	-0.017***	-0.015**	-0.016**
	(0.006)	(0.007)	(0.005)	(0.007)	(0.007)
Quarter FE	Yes	No	No	No	No
Firm FE	Yes	Yes	Yes	Yes	Yes
Sector × Quarter FE	No	Yes	No	Yes	Yes
State × Quarter FE	No	No	Yes	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	Yes
Obs.	10,702	10,702	10,702	10,702	10,702
R^2	0.806	0.838	0.813	0.842	0.842

McKendrick, 1927; Anderson and May, 1991), and is therefore our preferred model. As discussed in Chernozhukov et al. (2021), one limitation of a standard linear specification is that the transmission of the disease is influenced by other containment policies and people's voluntary behavioral changes in response to information about infection levels. If other mitigation policies respond to past infection levels, the coefficient on business closures derived from the standard linear specification may not capture the "direct causal" effects of business closures, but rather the "total effect" for the exposed group. For instance, the observed aggregate effect using the estimates in the linear specification may be lower than the true causal impact because other mitigation policies might offset the effect of business closures. Therefore, in such cases, one would underestimate the health benefits of business closures compared to the computations obtained using a SIRD model (in which we control for lagged infections, and as a consequence for behavioral changes associated with social distancing). That being said, in robustness tests presented in subsection 7.4, we estimate alternative specifications without controlling for lagged infection rates. We discuss how the various estimates affect our aggregate analysis in Section 8.

In our CZ-level specifications, the inclusion of state×week (or state×month) fixed effects largely addresses the concern that unobserved characteristics across states could spuriously drive both their business closure decisions and infection rates. One may also argue that some states could have been more likely to shut down the most important sectors of CZs with initially high infection rates. To further alleviate this concern, we control in all specifications for the interaction of infection rates in the week of March 10 – that is, before the first restrictions were enacted – and a dummy taking the value of 1 when restrictions are active. Another concern may be that drivers of state decisions could be correlated with propensity to stay at home. For instance, states might close sectors in which labor supply would have dropped anyway, or sectors for which demand would have dropped anyway. We provide robustness tests that mitigate these concerns.

The identifying assumption is that *in the absence of* business closures, changes in labor incomes (as well as changes in health outcomes) are uncorrelated with the CZ-level employment share of non-essential businesses. While we cannot formally test this assumption, we check in dynamic specifications (without controls for lagged infections) whether we find any effect in the weeks prior to the effective dates of business closures.

5. Effect on profits

In this section, we analyze the causal effect of state-mandated business closures on firms' outcomes. We first examine their impact on firms' sales, assets, and profits by estimating Equation (4), and present

Table 4

Restricted labor, wages, employment and hours. This table presents estimates from panel regressions of log(wages) (Panel A), log(employment) (Panel B), and log(hours) (Panel C) on the restricted labor share interacted with a dummy that equals one when business closures are active. Restricted labor in Panel A is computed using weights based on the total payroll of a given industry in each CZ whereas it is computed using employment weights in Panel B and C. All regressions include CZ fixed effects, state × week fixed effects, and in the second to sixth columns additional controls interacted with a dummy that equals one if business closures have been active. In the second to sixth columns, we include the urban share, population density, initial infection rates, and the share of work-from-home occupations in a given CZ. In the third to sixth columns, we include the share of contact-intensive occupations, as well as the kids share. In the fourth to sixth columns, we include net migrations in 2019, the share of the population with less than a high school diploma, the share of the population above 65 years old, and the logarithm of median household income. In the fifth and sixth column, we add the log of the number of hospitals plus one, the number of ICU beds per inhabitants and Donald Trump Vote Share in 2016. In the sixth column, we include the share of the commuting zone population that is subject to local mask and stay-at-home orders. Standard errors presented in parentheses are clustered both at the state level and at the week level. Regressions are population-weighted. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Panel A: Log(Wages)					
Restricted Labor × $I_{Shut Down}$	-0.763*	-0.970**	-0.982**	-0.906**	-0.937**	-0.956**
	(0.441)	(0.385)	(0.392)	(0.408)	(0.398)	(0.398)
Initial Infection, Share Urban, Pop Density, Work-from-home $\times I_{Shut Down}$	No	Yes	Yes	Yes	Yes	Yes
Contact-Intensive Share, Kids Share $\times I_{Shut Down}$	No	No	Yes	Yes	Yes	Yes
Census Controls $\times I_{Shut Down}$	No	No	No	Yes	Yes	Yes
Hospitals, ICU Beds, Trump Share $\times I_{Shut Down}$	No	No	No	No	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	No	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	27,994	27,994	27,994	27,994	27,994	27,994
R^2	0.992	0.992	0.992	0.992	0.992	0.992
	Panel B: Log(Employment)					
Restricted Labor × $I_{Shut Down}$	-0.538*	-0.566**	-0.547**	-0.559**	-0.541**	-0.565**
	(0.269)	(0.246)	(0.246)	(0.254)	(0.246)	(0.240)
Initial Infection, Share Urban, Pop Density, Work-from-home $\times I_{Shut Down}$	No	Yes	Yes	Yes	Yes	Yes
Contact-Intensive Share, Kids Share $\times I_{Shut Down}$	No	No	Yes	Yes	Yes	Yes
Census Controls $\times I_{Shut Down}$	No	No	No	Yes	Yes	Yes
Hospitals, ICU Beds, Trump Share $\times I_{Shut Down}$	No	No	No	No	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	No	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	27,994	27,994	27,994	27,994	27,994	27,994
R^2	0.995	0.996	0.996	0.996	0.996	0.996
	Panel C: Log(Hours)					
Restricted Labor × $I_{Shut Down}$	-0.782**	-0.797***	-0.787***	-0.822***	-0.798***	-0.822***
	(0.295)	(0.273)	(0.272)	(0.288)	(0.278)	(0.275)
Initial Infection, Share Urban, Pop Density, Work-from-home $\times I_{Shut Down}$	No	Yes	Yes	Yes	Yes	Yes
Contact-Intensive Share, Kids Share $\times I_{Shut Down}$	No	No	Yes	Yes	Yes	Yes
Census Controls $\times I_{Shut Down}$	No	No	No	Yes	Yes	Yes
Hospitals, ICU Beds, Trump Share $\times I_{Shut Down}$	No	No	No	No	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	No	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	27,994	27,994	27,994	27,994	27,994	27,994
R^2	0.995	0.995	0.995	0.995	0.995	0.995

the results in Table 3. In the specification with firms, sector×quarter, and state×quarter fixed effects, we find that firms with 10% of their employees who cannot work due to labor restrictions experience a significant drop in sales of around 2.3% (0.1 × 0.225, see column 4 of Panel A), and a significant drop in assets of around 1.2% (0.1 × 0.119, see column 4 of Panel B). In Panel C, we find that firms with 10% of their employees who cannot work due to labor restrictions experience a significant drop in their return on assets (ROA) of around 0.15 percentage point, a sizeable impact compared to the sample average ROA of 1.5.

We then check the sensitivity of our results to the inclusion of controls for other social distancing measures implemented in the location of firms' operations. Specifically, in the last columns of each panels, we augment our baseline specifications with the share of the firm employment that is subject to county-level mask mandates and stay-at-home orders. Reassuringly, the coefficients of interest are virtually unchanged.

In Fig. 3, we check and confirm that the closure of non-essential businesses does not have any material effect on firms' sales, assets, or profits in the quarters prior to their implementation. In Online Appendix B, we present formal tests in support of the parallel trends assumption following recent work by Roth (2022), and sensitivity analysis to alternative assumptions about possible violations of parallel trends in the pre-treatment period using the approach proposed by Rambachan and Roth (2023). When we restrict the post-treatment violations of parallel trends to be no larger than half the maximal pre-treatment violation of parallel trends ($\overline{M} = 0.5$), a 10 percentage point increase in the share of restricted labor is associated with 95%-confidence intervals for its effect in the quarter following the implementation of business closures on firm sales, assets, and ROA that lie between -5.9% and -1.8%, -2.9% and -0.5%, and -0.48 and -0.04 percentage points, respectively.

One may worry that states are more likely to shut down sectors that experience a drop in demand that is specific to the state, i.e., not captured by sector fixed effects, in which case the relationship between

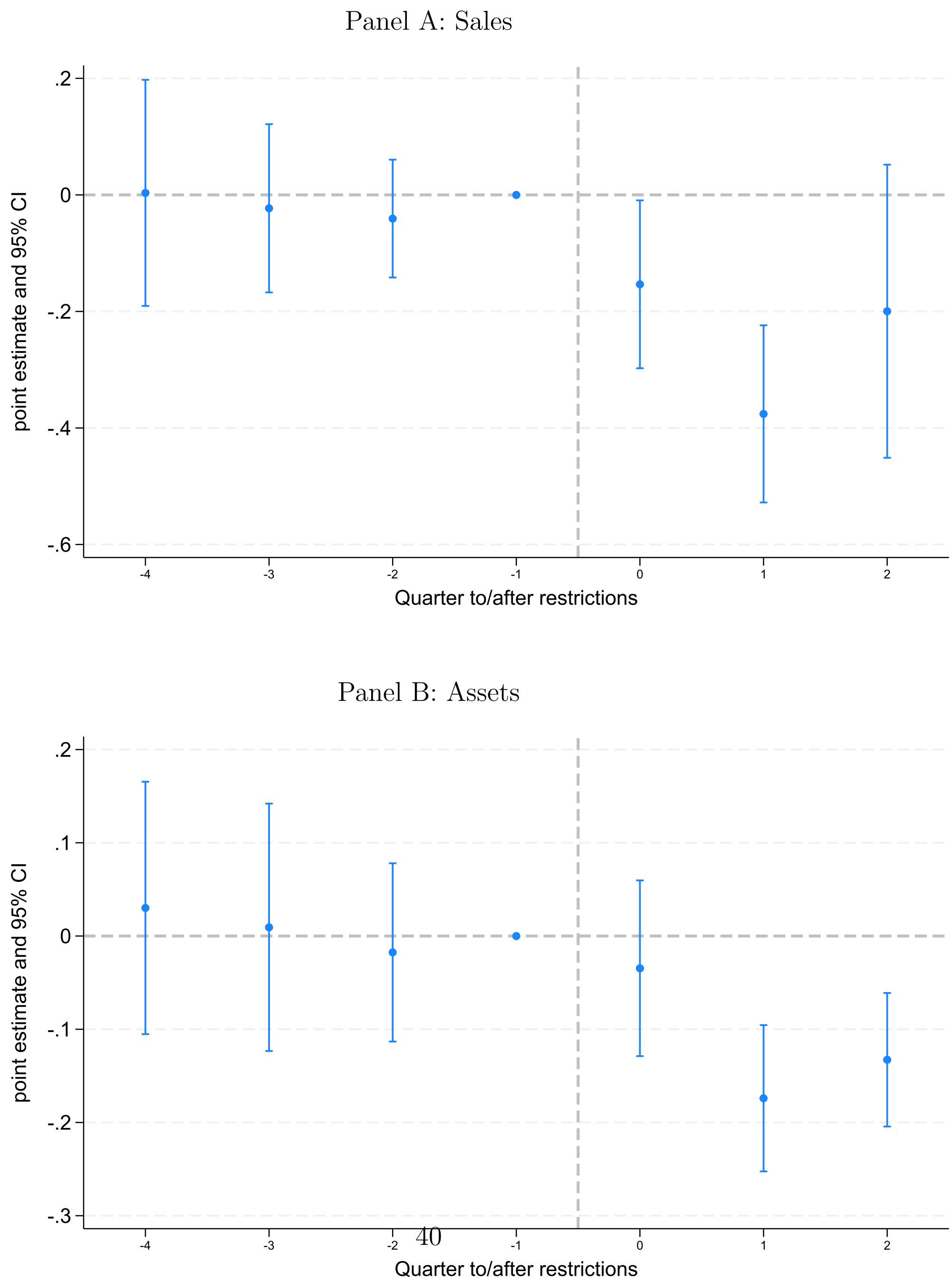

Fig. 3. Note: This figure presents coefficient estimates from panel regressions of individual firm sales (Panel A), assets (Panel B), and profits (Panel C) on the restricted labor share of each firm interacted with dummies around the implementation of business closures. In Panel A, sales are scaled by the firm's sales in 2018 in the same quarter of the year. In Panels B and C, assets and profits are scaled by the firm's total assets in 2018 in the same quarter of the year. The regressions include firm fixed effects, sector-quarter and state-quarter fixed effects, as well as the stay-at-home share and the mask share as control variables. Standard errors are clustered both at the firms' main industry and state levels.

Panel C: Profits

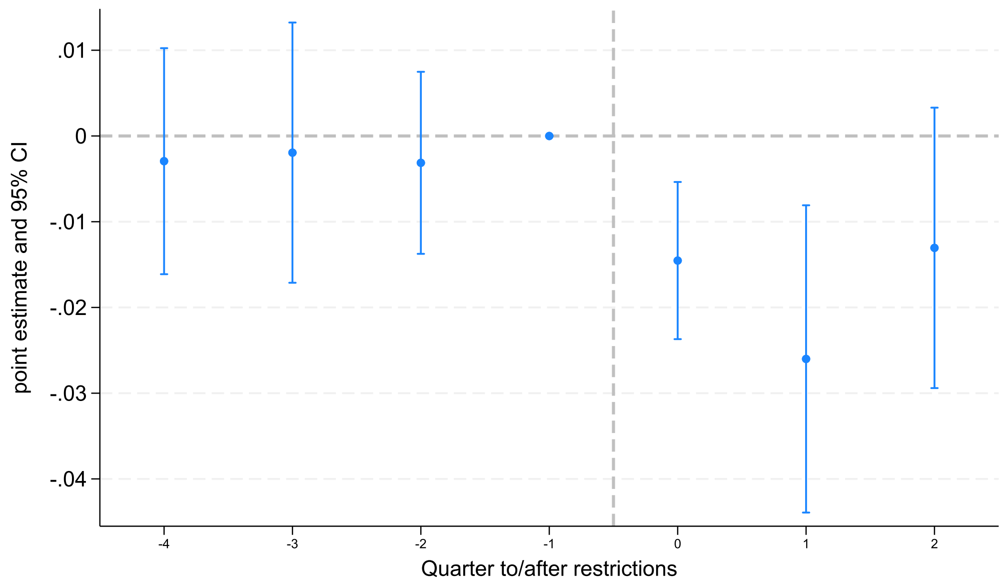

Fig. 3. (*continued*)

closures and firm outcomes that we document may be spurious. This is less likely to happen to manufacturing firms that typically sell outside of their state of headquarter. In Online Appendix Table A.1, we re-run the baseline specification on firm sales, assets, and profits after restricting the sample to manufacturing firms only, that are immune to the endogeneity concern. We find very similar effects, which greatly mitigates the concern that our findings may be endogenous.

One may argue that lost earnings resulting from business closures might simply be caught up after restrictions are lifted, such that they should not be considered as permanent losses to firms. To shed light on this issue, we go one step further and turn to the analysis of the impact of labor restrictions on firms' market value.[22] We follow standard event study methodology and consider the date when the state of each firm's headquarters *issues* an initial Executive Order restricting business activities. These event days are listed in Table 1, column 2. Online Appendix C provides more details on the event-study analysis and on the results. Overall, we find that the announcement of business closures have a robust and negative effect on firms' stock returns.

Taken together, these findings confirm that firms' profits are not simply postponed, and that labor restrictions have a negative effect on firm value.

6. Effect on labor income

We next study the effect of state-mandated business closures on wages and employment. As information on wages is not available in Compustat at the firm level, we estimate the impact of business closures on local wages and employment at the CZ level. We estimate Equation (5) for wages, employment, and hours and present the results in Table 4. We find that a 10 percentage point increase in the share of restricted labor in a given CZ is associated with a statistically significant and economically large drop in wages, employment, and hours ranging from respectively 9.5%, 5.6%, and 8.2%. Importantly, the coefficients are stable across specifications, including when we control for the share of the population subject to mask mandates and stay-at-home orders, suggesting that our estimates for the impact of business closures on labor outcomes are not confounded by the implementation of other social distancing measures over the sample period.

The effect on labor outcomes should show no prior trends for our identification assumption to be satisfied. To test whether this is indeed the case, we analyze the dynamics of the effects. We regress CZ-level wages, employment, and hours on the restricted labor share in each CZ interacted with dummies indicating different weeks around the implementation of business closures. The results are plotted in Fig. 4. No effect on wages, employment, or hours, is found in the weeks before the effective dates of the Executive Orders, and the coefficients start to decrease in the same week as the effective first date of the business closures, and remain large in absolute value over the following eight weeks. This confirms that the effect on labor outcomes is not driven by prior trends but is indeed caused by states' decisions to close non-essential businesses. In Online Appendix B, we also present formal tests in support of the parallel trends assumption following recent work by Roth (2022), and sensitivity analysis to alternative assumptions about possible violations of parallel trends in the pre-treatment period using the approach proposed by Rambachan and Roth (2023). When we restrict the post-treatment violations of parallel trends to be no larger than half the maximal pre-treatment violation of parallel trends ($\overline{M} = 0.5$), a 10 percentage point increase in the share of restricted labor is associated with 95%-confidence intervals for its effect in the week following the implementation of business closures on wages, employment, and hours that lie between -52% and 6.7%, -20% and -0.2%, and -23% and -3.1%, respectively.

To shed light on the representativeness of our results using Homebase, we also estimate the effect of business closures on employment using data from the BLS. One limitation of this data is that it is available at the monthly frequency only, unlike Homebase data. We run the same specifications as above at the monthly frequency, using CZ-month employment from the BLS as dependent variable. We present the results in Online Appendix Table A.6. We find an effect on employment which is quantitatively similar, though slightly smaller, compared to the one

[22] While estimating the impact of the announcements of business closures on stock returns is informative about whether they had a negative effect on firm profits in general, one cannot easily compare the magnitudes of the drop in market capitalization to the estimated drop in profits during the first wave in order to infer the amount of lost profits that are shifted across time (as opposed to permanently lost), partly because the announcement of business closures in a given State presumably also affects investors' beliefs about the likelihood and severity of business closures in future Covid-19 waves (and more generally future epidemics).

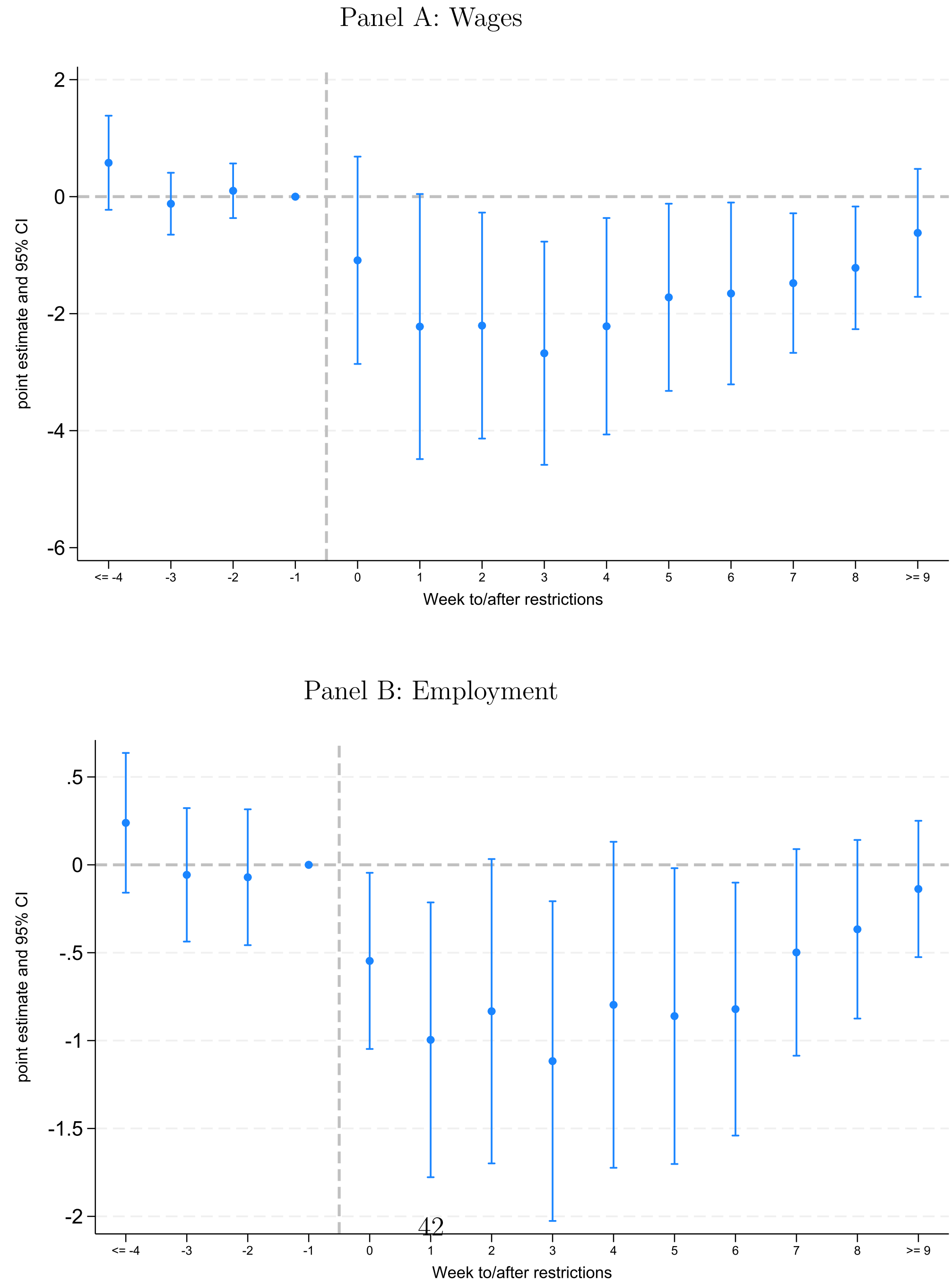

Fig. 4. Note: This figure presents estimates from panel regressions of log(wages) (Panel A), log(employment) (Panel B), and log(hours) (Panel C) on the restricted labor share in each CZ interacted with dummies around the implementation of business closures. Restricted labor in Panel A is computed using weights based on the total payroll of a given industry in each CZ whereas it is computed using employment weights in Panel B and C. All regressions include CZ fixed effects, state × week fixed effects, the share of the commuting zone population that is subject to local mask and stay-at-home orders, and the following control variables measured at the CZ level interacted with a dummy that equals one if business closures are active: the urban share, population density, initial infection rates, the share of work-from-home occupations, the share of contact-intensive occupations, the kids share, net migrations in 2019, the share of the population with less than a high school diploma, the share of the population above 65 years old, the logarithm of median household income, the log of the number of hospitals plus one, the number of ICU beds per inhabitants, Donald Trump Vote Share in 2016. Standard errors are clustered both at the state level and at the week level. Regressions are population-weighted.

using the reweighted Homebase data (-0.46 using BLS data versus -0.56 using Homebase).

7. Effect on health outcomes

In this section, we study the effect of state-mandated business closures on health outcomes. We consider their impact on Covid-19 infections and Covid-19 deaths, total deaths and years of life lost, Covid-19 hospitalizations and symptoms of bad mental health.

7.1. Covid-19 infections and Covid-19 deaths

Event-study plots. We first gauge the validity of the parallel trends assumption for the effect of business closures on Covid-19 infections

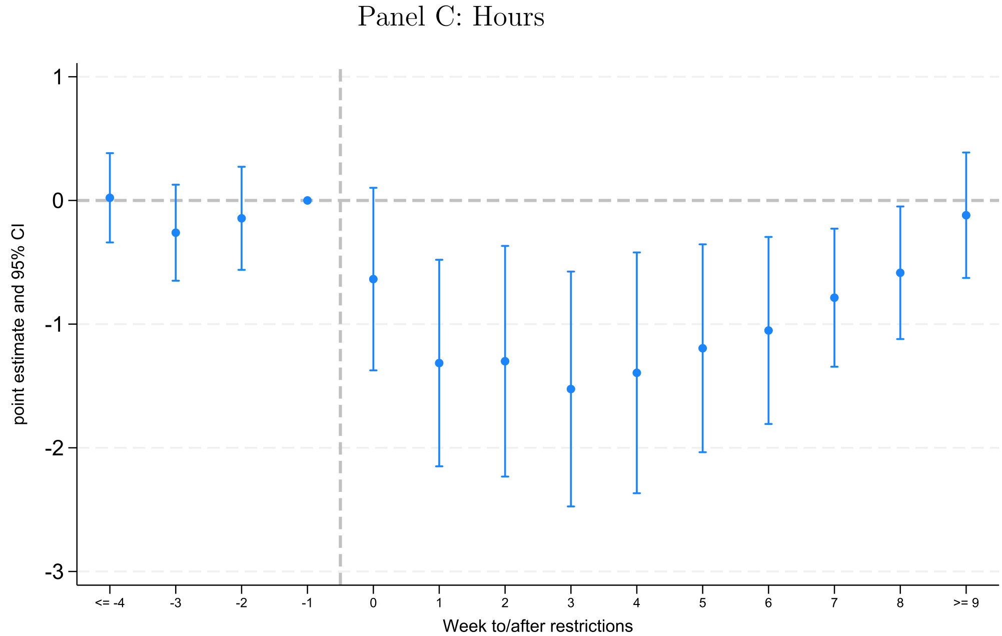

Fig. 4. (*continued*)

and Covid-19 deaths in event-study plots. Indeed, one may worry that activity levels, and therefore infection and death rates, would have decreased even in the absence of business closures, for instance if states were more likely to close sectors in which demand would have dropped anyway. To test whether this is indeed the case, we regress Covid-19 infection and death rates at the CZ level on the restricted labor share interacted with a full set of leads and lags around business closures. The reference point is one week before their implementation. We present the results in Fig. 5.

No effect on Covid-19 infections or deaths is found prior to the week when business closures became effective. In Online Appendix B, we present formal tests in support of the parallel trends assumption following recent work by Roth (2022), and sensitivity analysis to alternative assumptions about possible violations of parallel trends in the pre-treatment period using the approach proposed by Rambachan and Roth (2023). When we restrict the post-treatment violations of parallel trends to be no larger than half the maximal pre-treatment violation of parallel trends ($\overline{M} = 0.5$), the effect of a 10 percentage point increase in the share of restricted labor is associated, in the third week following the implementation of business closures, with 95%-confidence intervals that lie between -8.3 and 1.1 Covid-19 infections per 10,000 inhabitants, and -0.62 and 0.03 Covid-19 deaths per 10,000 inhabitants.

SIRD-based econometric model. We now turn to the specification presented in Equation (8), motivated by the SIRD model, which allows us to approximate the causal effect of business closures in reducing Covid-19 infections and deaths, while controlling for the nonlinear nature of the spread of Covid-19.

Panel A of Table 5 presents the results of the estimation of Equation (8) for Covid-19 cases. We find that labor restrictions are associated with a significant drop in Covid-19 reported infections. The coefficients are stable across specifications. These point estimates hold after controlling for the interaction of a variety of CZ-level controls for demographic and public health infrastructure with week fixed effects, and for the share of the population subject to mask mandates and stay-at-home orders. They are obtained after including state×week fixed effects, so that they are identified off of within-state variations in commuting zone industry composition.

Panel B of Table 5 presents the results for Covid-19 deaths. We find that a 10 percentage point increase in the share of restricted labor is associated with a drop in weekly Covid-19 mortality rates, of around 0.2 Covid-19 deaths per 10,000. The coefficients are stable across specifications and the economic effect is large: one standard deviation in the restricted labor share explains around 50% of the standard deviation in weekly mortality rates.

Robustness. We present a series of robustness tests for our baseline result on Covid-19 mortality rates. In Panel A of Online Appendix Table A.3, we directly control for local negative demand shocks that might drive health outcomes irrespective of state-mandated business closures. For this, we augment our baseline panel regressions with a variable controlling for individuals' mobility to groceries in the same CZ. This variable is obtained from Google Community Mobility Reports,[23] and measures individuals' percentage change mobility to groceries relative to baseline. If the effects that we are picking up in Table 5 reflect contemporaneous negative shocks in shut down sectors, this variable should subsume the main variable of interest, Restricted Labor × $I_{ShutDown}$. Instead, the coefficient on Restricted Labor × $I_{ShutDown}$ remains stable and significant in all specifications.

Another related concern is that the estimates from Table 5 might reflect the fact that business closures were more likely to be implemented in sectors in which activity would have declined anyway, irrespective of state-level decisions. To handle this issue, we augment our panel regressions with the interaction of a placebo variable *Restricted Labor based on National Closure Average* with the dummy $I_{ShutDown}$. This variable is obtained after replacing the dummy $Closed_{ind,c,state}$ by the national share of restricted labor in industry ind in Equation (3). If the effects we are picking up in Table 5 reflect contemporaneous negative shocks in shut down sectors, this variable should subsume the main variable of interest, Restricted Labor × $I_{ShutDown}$. We present the results of this specification in Online Appendix Table A.3, Panel B. The coefficient on the additional variable is insignificant, whereas the coefficient on Restricted Labor × $I_{ShutDown}$ remains stable and significant in all specifications.

We then assess whether cross-CZ migrations over the sample period might bias our estimates. If people were more likely to leave areas with tighter business closures, this might lead to a mechanical decline in the number of deaths. Since our death rate is based on 2019 popula-

[23] https://www.google.com/covid19/mobility/. The specification is run on a smaller number of observations, because data from the Google Community Mobility Reports is not available for the first weeks of our sample period.

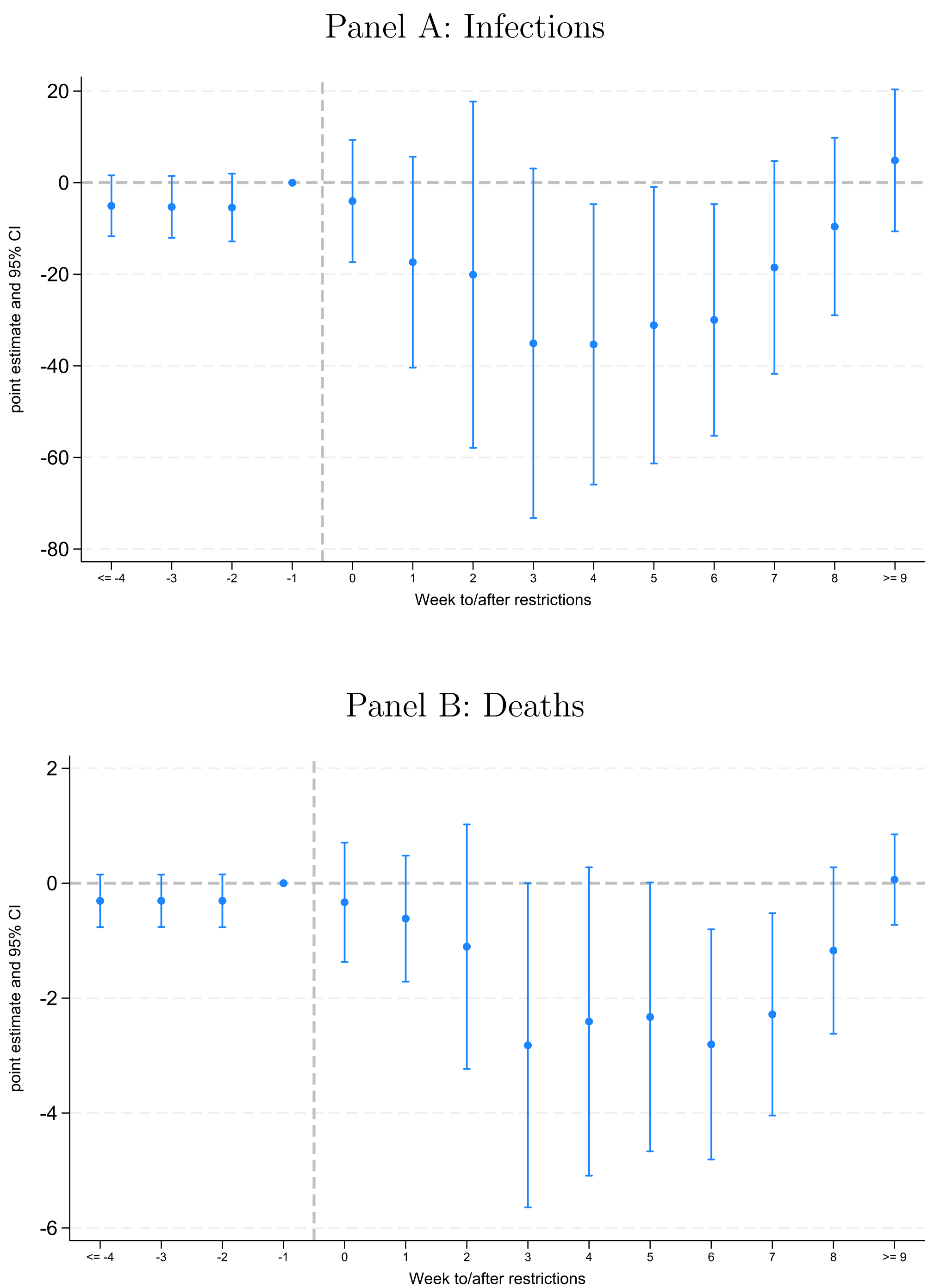

Fig. 5. Note: This figure presents estimates from panel regressions of new Covid-19 infections (per 10,000 inhabitants) (Panel A) and new Covid-19 deaths (per 10,000 inhabitants) (Panel B) on the restricted labor share in each CZ interacted with dummies around the implementation of business closures. All regressions include CZ fixed effects, state × week fixed effects, the share of the commuting zone population that is subject to local mask and stay-at-home orders, and the following control variables measured at the CZ level interacted with a dummy that equals one if business closures are active: the urban share, population density, initial infection rates, the share of work-from-home occupations, the share of contact-intensive occupations, the kids share, net migrations in 2019, the share of the population with less than a high school diploma, the share of the population above 65 years old, the logarithm of median household income, the log of the number of hospitals plus one, the number of ICU beds per inhabitants, Donald Trump Vote Share in 2016. Standard errors are clustered both at the state level and at the week level. Regressions are population-weighted.

Table 5
Restricted labor, Covid-19 infections and deaths. This table presents estimates from panel regressions of new Covid-19 infections (Panel A) and new Covid-19 deaths (Panel B), both per 10,000 inhabitants, on the restricted labor share interacted with a dummy that equals one when business closures are active. All regressions include CZ fixed effects, state × week fixed effects, lagged infection rates, and in the second to sixth columns additional controls interacted with a dummy that equals one if business closures have been active. In the second to sixth columns, we include the urban share, population density, initial infection rates, and the share of work-from-home occupations in a given CZ. In the third to sixth columns, we include the share of contact-intensive occupations, as well as the kids share. In the fourth to sixth columns, we include net migrations in 2019, the share of the population with less than a high school diploma, the share of the population above 65 years old, and the logarithm of median household income. In the fifth and sixth column, we add the log of the number of hospitals plus one, the number of ICU beds per inhabitants and Donald Trump Vote Share in 2016. In the sixth column, we include the share of the commuting zone population that is subject to local mask and stay-at-home orders. Standard errors presented in parentheses are clustered both at the state level and at the week level. Regressions are population-weighted. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Panel A: Infections					
	New Covid-19 Infections per 10,000 (T+1)					
Restricted Labor × $I_{ShutDown}$	-7.974**	-8.218**	-8.347**	-7.526*	-7.467*	-7.142*
	(3.726)	(3.826)	(3.935)	(3.907)	(3.883)	(4.029)
New Covid-19 Infections per 10,000	0.722***	0.722***	0.721***	0.714***	0.714***	0.714***
	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)	(0.046)
Initial Infection, Share Urban, Pop Density, Work-from-home $\times I_{ShutDown}$	No	Yes	Yes	Yes	Yes	Yes
Contact-Intensive Share, Kids Share $\times I_{ShutDown}$	No	No	Yes	Yes	Yes	Yes
Census Controls $\times I_{ShutDown}$	No	No	No	Yes	Yes	Yes
Hospitals, ICU Beds, Trump Share $\times I_{ShutDown}$	No	No	No	No	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	No	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	27,994	27,994	27,994	27,994	27,994	27,994
R^2	0.901	0.901	0.901	0.901	0.901	0.901
	Panel B: Deaths					
	New Covid-19 Deaths per 10,000 (T+2)					
Restricted Labor × $I_{ShutDown}$	-1.887***	-1.936***	-1.999***	-1.938***	-1.957***	-2.000***
	(0.634)	(0.613)	(0.617)	(0.610)	(0.613)	(0.610)
New Covid-19 Infections per 10,000	0.020***	0.020***	0.020***	0.019***	0.019***	0.019***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Initial Infection, Share Urban, Pop Density, Work-from-home $\times I_{ShutDown}$	No	Yes	Yes	Yes	Yes	Yes
Contact-Intensive Share, Kids Share $\times I_{ShutDown}$	No	No	Yes	Yes	Yes	Yes
Census Controls $\times I_{ShutDown}$	No	No	No	Yes	Yes	Yes
Hospitals, ICU Beds, Trump Share $\times I_{ShutDown}$	No	No	No	No	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	No	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	27,994	27,994	27,994	27,994	27,994	27,994
R^2	0.786	0.787	0.788	0.790	0.790	0.790

tion, this might in turn lead us to overestimate the effect of business closures on death rates. To address this concern, we use data collected by Ramani and Bloom (2021) who submitted a freedom of Information Act to request the United States Postal Service's National Change of Address (NCOA) dataset and obtain zip code-month level inflow and outflow data for the universe of U.S. zip codes. We aggregate their inflow and outflow data at the CZ-level for the period ranging from March to September 2020. We then adjust the 2019 CZ-level population count we use to scale Covid-19 deaths for net migrations (inflows minus outflows). In Online Appendix Table A.4, we present the results of specifications with this adjustment and find that the results are virtually identical to our baseline. Hence, we conclude that net migrations are unlikely to be driving or biasing our findings.

A recent literature in econometrics has raised concerns about the possibility of negative weights in two-period difference-in-differences estimators when treatment timing is staggered and there exists heterogeneity in treatment effects within-unit over time or between groups of units treated at different times (see e.g. Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020).[24] To address this point, we estimate separate regressions for each treated state and then take the average of the state-specific point estimates (in the spirit of Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). The average of these state-specific estimates, which do not use comparisons between treated CZs for identification, show a similar pattern as our main estimates, with negative post-closure estimates on death rates (see Online Appendix Table A.5).

Finally, our baseline estimates are potentially biased estimates of the true effects of mandated business closures on death rates in the presence of externalities. Formally, the validity of the identification strategy relies on the assumption that CZs are the relevant geographical unit in the sense that the health outcomes in one CZ are not affected by the policy in neighboring areas, the so-called "no-interference" (Rubin, 1978) or "stable unit treatment value" (Angrist et al., 1996) assumption. Our baseline regression already takes into account treatment externalities across areas within the same CZ, which is arguably where local externalities should be the strongest given that workers are more likely to commute within than outside CZs. Still, we now turn to examining cross-CZ externalities directly. The presence of positive cross-CZ externalities have two simple testable implications. First, death rates should drop in CZs where neighboring areas also have a high fraction of restricted labor. Second, the net impact of the treatment (the restricted labor share in the CZs of interest) should increase as the fraction of restricted labor in neighboring areas rises.

To gauge the importance of these externalities for our baseline estimates, we augment our baseline specification with the restricted labor share variable for neighboring counties, and present the re-

[24] This concern does not apply for our firm-level specifications as in that case virtually all treated firms have been affected in the same quarter.

sults in Online Appendix Table A.6. We find weak evidence for the presence of local spillovers. The coefficient on $Restricted\,Labor \times I_{Shut\,Down}(Neighboring\ Counties)$ is negative but statistically insignificant at conventional levels. While we find some weak evidence of cross-CZ externalities, note however that the coefficient on $Restricted\,Labor \times I_{Shut\,Down}$ remains stable and significant in all specifications, which suggests that the "stable unit treatment value assumption" holds in our data, and that we can confidently use our micro treatment effect estimates for running a back-of-the-envelope calculation on the impact of business closures on *aggregate* death rates, as we do in Section 8.

7.2. Total deaths and years of life lost

While we show in the previous section that business closures led to a reduction in the number of Covid-19 deaths, these specifications might not fully capture the overall effect of business closures on mortality for at least two reasons. First, some deaths (for instance for respiratory diseases) might be misclassified as Covid-19 deaths. Second, business closures might have an indirect impact on other types of death, for instance if they lead to a severe deterioration of people's fitness or mental health.

To shed light on these issues, we estimate the effect of business closures on total deaths using data from the National Center for Health Statistics at the Centers for Disease Control and Prevention (CDC). One limitation of the data is that it is available at the monthly frequency only, unlike the data on Covid-19 infections and Covid-19 deaths used in the previous section. Importantly, one additional benefit of the CDC data is that it allows us to estimate the effect of business closures on total *years of life saved*, rather than the *numbers of life saved*. This matters for the aggregate analysis presented in Section 8, as those that died from Covid-19 are likely to be older. Accordingly, we run the same specifications as above at the monthly frequency, using three dependent variables: total number of deaths, total years of life lost, and total quality-adjusted years of life lost. We report the results in Table 6.

Total deaths. Panel A of Table 6 presents the results on the total number of deaths (both Covid-19 and non Covid-19 related). We find that a 10 percentage point increase in the share of restricted labor is associated with a significant drop in monthly mortality rates, of 1.2 deaths per 10,000. The magnitude of the effect on total deaths is around 40% larger than the effect on deaths reported as due to Covid-19.[25] Our findings are consistent with other work showing that the effect of Covid-19 on mortality was significantly higher than the officially reported number of Covid-19 deaths (see e.g. Wang et al., 2022; Msemburi et al., 2023).

Years of life lost. To obtain an economic estimate for the health benefits of business closures in reducing mortality, one could relate the coefficients in Panel A of Table 6 on total deaths to empirical estimates for the value of a statistical life. However, doing so would probably overstate the health benefits of business closures given that the lives that were saved were presumably lives of people who were older. Instead, we follow prior work and estimate the effect of business closures directly on years of life lost.[26] In Panels B and C of Table 6, we find that a 10 percentage point increase in the share of restricted labor is associated with a drop in monthly years of life lost of around 16.8 years per 10,000 inhabitants, and a drop in monthly quality-adjusted years of life lost of around 12.9 years per 10,000 inhabitants. Comparing the estimates in Table 6 for total number of lives and total years of life lost suggests that the average person whose life was saved because of business closures gained around 13 years (168/12.7) of potential life. While this calculation highlights the fact that business closures caused nontrivial increases in expected lifetime, it is consistent with the notion that people whose lives were saved were relatively older.[27]

7.3. Other health outcomes

In this section, we explore some of the other potential health-related benefits (such as avoided hospitalizations) and costs (such as deteriorated mental health) associated to business closures.

Covid-19 hospitalizations. Table 7 presents the results of the estimation of Equation (5) for Covid-19 hospitalizations. We find that a 10 percentage point increase in the share of restricted labor is associated with a significant drop in the number of new Covid-19 hospitalizations per week, of around 0.9 per 10,000.

Mental health. We turn to the effect of business closure on mental health. To do so, we exploit data from the Household Pulse Survey. For our purposes, we rely on a question asking survey respondents the reason for why they did not receive any labor income in a given week (if indeed the case), for which one of the potential answer was *"My employment closed temporarily because of the coronavirus pandemic"*. While the answer does not refer explicitly to state-mandated business closures, we label this answer "Business closure" and estimate its correlation with different proxies for mental health included in the survey in a specification in which we include state×week fixed effects as well as an extensive series of demographics (year of birth, gender, race) and socioeconomic variables (income brackets, marital status, home ownership, education, household size). We present the results in Online Appendix Table A.7.

We find a positive and statistically significant relationship between business closures and symptoms of bad mental health (anxiety, being worried, feeling down, low interest for things). The economic effect is large: "Business closure" is associated with an increase by respectively 6, 7.7, 7.5, and 7.6 percentage points in reporting mild or severe anxiety, being worried, feeling down, and low interest for things (an increase by respectively 10%, 16%, 17%, 17% compared to the sample means). Severe forms of anxiety, being worried, feeling down, and low interest for things go up by respectively 2.7, 2.6, 2.0, and 1.9 percentage points (an increase by respectively 19%, 26%, 26%, 26% compared to the sample means). We discuss the implications of these estimates in our cost-benefit analysis presented below.

7.4. Additional specifications

In addition to our results on health outcomes presented above, we also present the results of specifications that do not include Covid-19 infections as a control variable. Formally, we estimate a specification similar to Equation (8) without the independent variable $I_{cz,state,t}$. This allows us to test the robustness of our results to different specifications. Note however that the main caveat of this alternative specification is that it is not consistent with a SIRD epidemiological model.

Results are presented in Online Appendix Tables A.8, A.9, and A.10 for weekly Covid-19 infections and death, monthly total deaths and years of life lost, and hospitalizations, respectively. We find that an increase in the share of restricted labor is associated with a statistically significant drop in each of these outcomes. We discuss the implication of using these estimates instead of those in the SIRD model in our cost-benefit analysis in Section 8.

[25] We find that a 10 percentage point increase in the share of restricted labor is associated with a drop in weekly Covid-19 mortality rates, around 0.2 per 10,000, which is approximately 0.2*30/7 = 0.86 at a monthly frequency, against a coefficient of 1.2 for total deaths.

[26] See e.g. Deschênes and Moretti (2009); Deschênes and Greenstone (2011) for empirical studies of the effect of weather events on years of life lost, using data on total deaths by gender and age groups, and counterfactual life expectancy from population life tables.

[27] While we account for heterogeneity in life expectancy for both deceased individuals' age and gender, one caveat for this calculation is that we may overstate (or understate) the potential gain in life-years if affected individuals have shorter (or longer) life expectancies than the average person in their age-gender group.

Table 6
Restricted labor, total deaths, and years of life lost. This table presents estimates from panel regressions of new total deaths (Panel A), years of life lost (Panel B), and quality-adjusted years of life lost (Panel C), all per 10,000 inhabitants, on the restricted labor share interacted with a dummy that equals one when business closures are active, at the monthly frequency. All regressions include CZ fixed effects, state × month fixed effects, lagged infection rates, and in the second to sixth columns additional controls interacted with a dummy that equals one if business closures have been active. In the second to sixth columns, we include the urban share, population density, initial infection rates, and the share of work-from-home occupations in a given CZ. In the third to sixth columns, we include the share of contact-intensive occupations, as well as the kids share. In the fourth to sixth columns, we include net migrations in 2019, the share of the population with less than a high school diploma, the share of the population above 65 years old, and the logarithm of median household income. In the fifth and sixth column, we add the log of the number of hospitals plus one, the number of ICU beds per inhabitants and Donald Trump Vote Share in 2016. In the sixth column, we include the share of the commuting zone population that is subject to local mask and stay-at-home orders. Standard errors presented in parentheses are clustered at the state level and at the month level. Regressions are population-weighted. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Panel A: Total Deaths (Monthly)					
	New Deaths per 10,000 (M+1)					
Restricted Labor $\times I_{ShutDown}$	-12.112**	-12.015**	-12.363**	-12.388**	-12.515**	-12.747**
	(4.639)	(4.701)	(4.687)	(4.843)	(4.962)	(4.959)
New Covid-19 Infections per 10,000	0.043**	0.043**	0.042*	0.043	0.043	0.043*
	(0.018)	(0.018)	(0.019)	(0.024)	(0.023)	(0.023)
Initial Infection, Share Urban, Pop Density, Work-from-home $\times I_{ShutDown}$	No	Yes	Yes	Yes	Yes	Yes
Contact-Intensive Share, Kids Share $\times I_{ShutDown}$	No	No	Yes	Yes	Yes	Yes
Census Controls $\times I_{ShutDown}$	No	No	No	Yes	Yes	Yes
Hospitals, ICU Beds, Trump Share $\times I_{ShutDown}$	No	No	No	No	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	No	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	4,800	4,800	4,800	4,800	4,800	4,800
R^2	0.897	0.897	0.897	0.897	0.898	0.898
	Panel B: Years of Life (Monthly)					
	New Years of Life Lost per 10,000 (M+1)					
Restricted Labor $\times I_{ShutDown}$	-160.999**	-159.132**	-165.200**	-163.692**	-165.893**	-168.439**
	(57.863)	(59.176)	(58.953)	(61.740)	(63.508)	(63.880)
New Covid-19 Infections per 10,000	0.627**	0.627**	0.625**	0.630	0.630*	0.630*
	(0.233)	(0.235)	(0.251)	(0.339)	(0.310)	(0.304)
Initial Infection, Share Urban, Pop Density, Work-from-home $\times I_{ShutDown}$	No	Yes	Yes	Yes	Yes	Yes
Contact-Intensive Share, Kids Share $\times I_{ShutDown}$	No	No	Yes	Yes	Yes	Yes
Census Controls $\times I_{ShutDown}$	No	No	No	Yes	Yes	Yes
Hospitals, ICU Beds, Trump Share $\times I_{ShutDown}$	No	No	No	No	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	No	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	4,800	4,800	4,800	4,800	4,800	4,800
R^2	0.899	0.899	0.900	0.900	0.900	0.900
	Panel C: Quality-adjusted Years of Life (Monthly)					
	New Quality-adjusted Years of Life Lost per 10,000 (M+1)					
Restricted Labor $\times I_{ShutDown}$	-123.978**	-122.435**	-127.093**	-125.853**	-127.493**	-129.473**
	(45.466)	(46.488)	(46.388)	(48.422)	(49.791)	(50.077)
New Covid-19 Infections per 10,000 (T)	0.481**	0.481**	0.479**	0.484	0.484*	0.484*
	(0.181)	(0.182)	(0.197)	(0.264)	(0.243)	(0.238)
Initial Infection, Share Urban, Pop Density, work-at-home $\times I_{ShutDown}$	No	Yes	Yes	Yes	Yes	Yes
Contact-Intensive Share, Kids Share $\times I_{ShutDown}$	No	No	Yes	Yes	Yes	Yes
Census Controls $\times I_{ShutDown}$	No	No	No	Yes	Yes	Yes
Hospitals, ICU Beds, Trump Share $\times I_{ShutDown}$	No	No	No	No	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	No	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	4,800	4,800	4,800	4,800	4,800	4,800
R^2	0.899	0.899	0.900	0.900	0.900	0.900

Table 7
Restricted labor and Covid-19 hospitalizations. This table presents estimates from panel regressions of new Covid-19 hospitalizations per 10,000 inhabitants on the restricted labor share interacted with a dummy that equals one when business closures are active. All regressions include CZ fixed effects, state × week fixed effects, lagged infection rates, and in the second to sixth columns additional controls interacted with a dummy that equals one if business closures have been active. In the second to sixth columns, we include the urban share, population density, initial infection rates, and the share of work-from-home occupations in a given CZ. In the third to sixth columns, we include the share of contact-intensive occupations, as well as the kids share. In the fourth to sixth columns, we include net migrations in 2019, the share of the population with less than a high school diploma, the share of the population above 65 years old, and the logarithm of median household income. In the fifth and sixth column, we add the log of the number of hospitals plus one, the number of ICU beds per inhabitants and Donald Trump Vote Share in 2016. In the sixth column, we include the share of the commuting zone population that is subject to local mask and stay-at-home orders. Standard errors presented in parentheses are clustered both at the state level and at the week level. Regressions are population-weighted. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	New Covid-19 Hospitalizations per 10,000 (T+1)					
Restricted Labor × $I_{ShutDown}$	-8.348**	-9.203***	-9.604***	-9.594***	-9.656***	-9.469***
	(3.505)	(3.317)	(3.236)	(3.294)	(3.378)	(3.434)
New Covid-19 Infections per 10,000	0.028***	0.029***	0.029***	0.028***	0.028***	0.028***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Initial Infection, Share Urban, Pop Density, Work-from-home ×$I_{ShutDown}$	No	Yes	Yes	Yes	Yes	Yes
Contact-Intensive Share, Kids Share ×$I_{ShutDown}$	No	No	Yes	Yes	Yes	Yes
Census Controls ×$I_{ShutDown}$	No	No	No	Yes	Yes	Yes
Hospitals, ICU Beds, Trump Share ×$I_{ShutDown}$	No	No	No	No	Yes	Yes
Stay at Home and Mask Share	No	No	No	No	No	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes	Yes
State × Week FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	27,994	27,994	27,994	27,994	27,994	27,994
R^2	0.748	0.755	0.755	0.756	0.756	0.756

Table 8
Restricted labor and death outcomes: Low versus high-contact CZ. This table presents estimates from panel regressions of new Covid-19 deaths per 10,000 inhabitants at the CZ× week level, and estimates from panel regressions of new total deaths, years of life lost, and quality-adjusted years of life lost, all per 10,000 inhabitants, at the CZ× month level, on the restricted labor share interacted with a dummy that equals one when business closures are active as well as with a dummy equal to one if the share of high contact employment in the CZ is above the median CZ share within each state. All regressions include CZ fixed effects, state × week fixed effects (respectively state × month fixed effects in the second to fourth column), lagged infection rates, and additional controls interacted with a dummy that equals one if business closures have been active, namely the urban share, population density, initial infection rates, the share of work-from-home occupations, the share of contact-intensive occupations, the kids share, net migrations in 2019, the share of the population with less than a high school diploma, the share of the population above 65 years old, the logarithm of median household income, the log of the number of hospitals plus one, the number of ICU beds per inhabitants, Donald Trump Vote Share in 2016, and the share of the commuting zone population that is subject to local mask and stay-at-home orders. Standard errors presented in parentheses are clustered both at the state level and at the week level in the first column, at the state level and at the month level in the second to fourth columns. Regressions are population-weighted. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	CZ-week level	CZ-month level		
	Covid-19 Deaths (T+2) per 10,000	Total Deaths (M+1) per 10,000	Years of Life Lost (M+1) per 10,000	Quality-adjusted Years of Life Lost (M+1) per 10,000
Restricted Labor ×$I_{ShutDown}$	-1.263**	-8.553*	-105.082	-77.973
	(0.575)	(4.172)	(56.662)	(42.733)
Restricted Labor × $I_{ShutDown}$ × High Contact CZ	-0.658***	-3.387**	-51.173*	-41.859*
	(0.125)	(1.353)	(26.648)	(20.720)
New Covid-19 Infections per 10,000	0.019***	0.043*	0.627*	0.481*
	(0.005)	(0.022)	(0.291)	(0.228)
Initial Infection, Share Urban, Pop Density, Work-from-home ×$I_{ShutDown}$	Yes	Yes	Yes	Yes
Contact-Intensive Share, Kids Share ×$I_{ShutDown}$	Yes	Yes	Yes	Yes
Census Controls ×$I_{ShutDown}$	Yes	Yes	Yes	Yes
Hospitals, ICU Beds, Trump Share ×$I_{ShutDown}$	Yes	Yes	Yes	Yes
Stay at Home and Mask Share	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes	Yes
State × Week FE	Yes	No	No	No
State × Month FE	No	Yes	Yes	Yes
Obs.	27,994	4,800	4,800	4,800
R^2	0.791	0.899	0.901	0.901

7.5. Low versus high contact intensity areas

One would expect the effect of state-mandated business closures on health outcomes to be larger in CZs where workplace interactions are more intense. To test whether this is the case, in Table 8, we present estimation results for death outcomes in augmented specifications in which we add an additional variable $Restricted\,Labor \times I_{ShutDown} \times HighContactCZ$, capturing the additional effects of labor restrictions in high-contact CZs, defined as those in which the employment-weighted contact intensity is above the median across all CZs within a given state. For Covid-19 deaths, total deaths, or total years of life lost, we find that the coefficients on the interaction term are large and statistically significant. Given that the effect of business closures on firm profits and labor income is similar across both subgroups (see Online Appendix Table A.11), this suggests that a differentiated business closure policy targeting high contact intensity CZs would have saved lives with lower impact on profits and wages.

8. Aggregate implications

This section provides an analysis of the aggregate cost of business closures in terms of lost profits for firms and foregone wages for affected workers, and compare it to the health benefits from reduced mortality and hospitalizations, net of the mental health costs. Table 9 presents different estimates, and their associated confidence intervals, for different specifications and different assumptions for the value of a statistical life, or life-year, for the cost of Covid-19 hospitalizations, and for the cost of mental health disorders.

Profits. In Panel A of Table 9, we compute the aggregate drop in firm profits associated to business closures using the following formula: $\sum_{cz} 3 \times \beta_{profits} \times Restricted\,Labor_{cz} \times AggregateAssets_{cz}$, where $Restricted\,Labor_{cz}$ is the level of the restricted labor share implemented in each CZ (see the distribution in Panel B of Table 2), and $AggregateAssets_{cz}$ is total assets of non-financial U.S. firms located in each CZ.[28] We report the estimates for the baseline specification presented in Table 3 ($\beta_{profits} = 0.16$ in Panel C, column 5), and for the dynamic specification presented in Fig. 3 (using the average of the three post-treatment estimates in Panel C). Finally, we also report the confidence sets for the average of the post-treatment estimates using the value $\overline{M} = 0.5$ in Rambachan and Roth (2023). We find that state-mandated business closures led to a drop in aggregate profits of \$359 billion (with a 95%-confidence interval between \$51 and \$667 billion) in the baseline specification, and of \$392 billion (with a 95%-confidence interval between \$112 to \$673 billion) in the dynamic specification. The confidence set using $\overline{M} = 0.5$ is between \$0 to \$785 billion.[29] We also report the same estimates assuming 10% of profit shifting over time.[30] The upper bound of all these estimates is \$785 billion, around 3.7% of U.S. GDP in 2019.

Labor income. We then compute the aggregate drop in labor income associated to business closures using the following formula: $\sum_{cz} \sum_{t} \beta_{wages} \times \frac{Restricted\,Labor_{cz,t}}{52} \times AggregateWages_{cz}$, where $Restricted\,Labor_{cz,t}$ is the payroll-weighted level of the restricted labor share implemented in each CZ and week t, and $AggregateWages_{cz}$ is total payroll in each CZ in December 2019 (obtained from the County Business Patterns 2019 File). We report the estimates for the baseline specification presented in Table 4 ($\beta_{wages} = -0.956$ in Panel A, column 6), and for the dynamic specification presented in Fig. 4 (using the average of the post-treatment estimates of Panel A over weeks 0 to +8). Finally, we also report the confidence sets for the average of the post-treatment estimates using the value $\overline{M} = 0.5$ in Rambachan and Roth (2023). We find that state-mandated business closures led to a drop in aggregate labor income of \$173 billion (with a 95%-confidence interval between \$31 and \$314 billion) in the baseline specification, and of \$186 billion (with a 95%-confidence interval between \$33 to \$339 billion) in the dynamic specification. The confidence set using $\overline{M} = 0.5$ is between \$-89 and \$588 billion. We also report the estimates based on the micro coefficients of the specifications using Homebase employment and BLS employment, and find similar magnitudes. The upper bound of all these estimates is \$588 billion, around 2.7% of U.S. GDP in 2019.

Finally, we compare the share of losses borne by profits and wages across these different specifications. We obtain point estimates for the aggregate cost of business closures (profits and labor income) between \$475-\$579 billion. Across these estimates, firms tend to bear on average around two thirds of the aggregate cost (with 95%-confidence intervals ranging from 20% to 95%). Given the share of labor in valued added in the U.S. (around 60%), these findings suggest that firms partially insured their workers by absorbing a large share of the cost through lower profits.

Health outcomes. In Panel B of Table 9, we report estimates for the aggregate effect of business closures on the number of lives saved, the number of quality-adjusted life-years saved (QALY), hospitalizations, and mental health disorders. We present estimates based on both the micro coefficients of the SIRD model and the linear model. For Covid-19 deaths, we also report estimates from the dynamic specification of the linear model, as well as the confidence sets for the average of the post-treatment estimates using the value $\overline{M} = 0.5$ in Rambachan and Roth (2023), where we restrict the post-treatment violations of parallel trends to be no larger than half the maximal pre-treatment violation of parallel trends.

In Online Appendix D, we show that the SIRD model presented in Section 4 implies that the aggregate effect of business closures can be obtained using the following formula (where t denotes weeks): $\beta^{SIRD}_{health} \sum_{t=0}^{T} \sum_{k=0}^{t-2} \gamma^k \sum_{cz} Restricted\,Labor_{cz,t-2-k} pop_{cz}$, where β^{SIRD}_{health} is respectively the most conservative estimate for Covid-19 deaths, total deaths, QALY, and hospitalizations presented in Tables 5, 6, and 7,[31] $\gamma = 0.7$ is the coefficient on lagged infections obtained from Panel A of Table 5, $Restricted\,Labor_{cz,t}$ is the level of the restricted labor share implemented in each CZ and week t, and pop_{cz} is total population in each CZ in December 2019. For the standard linear model, we compute the aggregate effect of business closures using the following formula: $\beta_{health} \sum_{t} \sum_{cz} pop_{cz} Restricted\,Labor_{cz,t}$, where β_{health} is the most conservative estimate for Covid-19 deaths, total deaths, QALY,

28 Unfortunately, there is no publicly available dataset with information on non-financial firms' total assets aggregated at the CZ level. We approximate it by multiplying each CZ GDP weights by total assets of non-financial U.S. firms in December 2018, \$42,140 billion. By doing so, we are implicitly assuming that the distribution of firm' assets across the U.S. is the same as the distribution of aggregate value-added. We find virtually identical numbers for the aggregate dollar drop in profits when using CZ employment weights, instead of GDP weights.

29 Note that our approximations for the aggregate drop in profits hinges on the assumption that the sensitivity of private firms' profits to business closures is similar to the one of publicly listed firms. To the extent that small firms' profits are likely to be more sensitive to business closures than publicly listed firms' profits, for instance because it is easier for publicly listed firms to adjust production across their different plants located across different states when some production facilities are disrupted by business closures, our estimate for the incidence of mandatory business closures on firms' profits is likely to be a lower bound.

30 The choice of 10% matches the share of durable goods' sectors in aggregate corporate profits, which is equal to around 11% in 2019, see Table 6.16D of the NIPA tables produced by the BEA.

31 As shown formally in Online Appendix D, one needs to divide the coefficients on total deaths and QALY by 4 as the regressions on total deaths and QALY are run at the monthly frequency (whereas hospitalizations and Covid-19 deaths are observed at the weekly frequency).

Table 9
Aggregate effects. This table presents estimates of the aggregate effect of business closures on firms' profits, labor income, Covid-19 deaths, total deaths, the number of quality-adjusted years of life saved, the number of hospitalizations saved, and the cost of mental health. Several estimates are presented based on different specifications or assumptions. 95% confidence intervals are presented in squared brackets, as well as bounds to alternative assumptions about possible violations of parallel trends in the pre-treatment period using the approach proposed by Rambachan and Roth (2023).

Panel A: Profits and labor income (losses in bn $)

	Baseline		Dynamic		Rambachan and Roth (2023) $\overline{M} = 0.5$
Firm profits					
Baseline	359	[51, 667]	392	[112, 673]	[0, 785]
Assuming 10% profit-shifting	323	[46, 600]	352	[101, 606]	[0, 707]
Labor income					
Homebase Wages	173	[31, 314]	186	[33, 339]	[-89, 588]
Homebase Emp	186	[31, 342]	147	[24, 271]	[-119, 449]
BLS Emp	152	[112, 191]			
Total losses					
Profits+labor income (Homebase Wages)	532	[194, 871]	579	[259, 899]	
Profits+labor income (Homebase Emp)	545	[201, 891]	540	[234, 847]	
Profits+labor income (BLS Emp)	511	[200, 821]			
Assuming 10% profit-shifting					
Profits+labor income (Homebase Wages)	496	[185, 807]	539	[244, 835]	
Profits+labor income (Homebase Emp)	510	[192, 828]	501	[220, 782]	
Profits+labor income (BLS Emp)	475	[195, 755]			
Share of profits in total losses (%)					
Profits/(Profits+labor income (Homebase Wages))	67%	[24%, 92%]	68%	[35%, 92%]	
Profits/(Profits+labor income (Homebase Emp))	66%	[22%, 92%]	73%	[40%, 95%]	
Profits/(Profits+labor income (BLS Emp))	70%	[25%, 83%]			
Assuming 10% profit-shifting					
Profits/(Profits+labor income (Homebase Wages))	65%	[22%, 92%]	65%	[33%, 92%]	
Profits/(Profits+labor income (Homebase Emp))	63%	[20%, 92%]	71%	[38%, 94%]	
Profits/(Profits+labor income (BLS Emp))	68%	[23%, 81%]			

Panel B: Health benefits (# in thousand)

	SIRD		Linear		Dynamic		Rambachan and Roth (2023) $\overline{M} = 0.5$
Deaths							
Covid-19 Deaths	502	[202, 801]	184	[49, 319]	82	[13, 151]	[-18, 205]
Total Deaths	800	[190, 1409]	303	[45, 560]			
QALY	8118	[1964, 14271]	3172	[504, 5841]			
Hospitalizations	2375	[687, 4063]	765	[268, 1263]			
Mental Health (-)			5953	[4202, 7703]			

Panel C: Health benefits (monetary value in bn $)

	SIRD		Linear		Dynamic		Rambachan and Roth (2023) $\overline{M} = 0.5$
Covid-19 Deaths							
VSL = $1.3 million	652	[262, 1042]	240	[64, 414]	107	[17, 197]	[-23, 266]
VSL = $3 million	1505	[605, 2404]	552	[147, 957]	246	[39, 453]	[-54, 615]
VSL = $6 million	3009	[1210, 4809]	1104	[294, 1914]	492	[78, 906]	[-108, 1230]
Total Deaths							
VSL = $1.3 million	1039	[247, 1831]	394	[58, 728]			
VSL = $3 million	2398	[569, 4226]	909	[135, 1680]			
VSL = $6 million	4795	[1139, 8451]	1818	[270, 3360]			
QALY							
Value_{QALY} = $50,000	406	[98, 714]	158	[25, 292]			
Value_{QALY} = $100,000	812	[196, 1427]	317	[50, 584]			
Value_{QALY} = $200,000	1624	[393, 2854]	634	[100, 1168]			
Hospitalizations							
Cost_{Hosp} = $19,900	47	[14, 81]	15	[5, 25]			
Cost_{Hosp} = $21,700	52	[15, 88]	17	[6, 27]			
Cost_{Hosp} = $24,500	58	[17, 99]	18	[6, 30]			
Mental Health (-)							
Cost_{MDD} = $1,900			11	[15, 8]			
Cost_{MDD} = $6,800			40	[52, 28]			
Cost_{MDD} = $24,000			142	[185, 101]			

and hospitalizations presented in Online Appendix Tables A.8, A.9, and A.10. We also report an estimate for the effect of business closures on the aggregate number of mental health disorders using the results in Online Appendix Table A.7.[32]

We find that business closures mandated by U.S. states led to approximately 502,000 fewer Covid-19 deaths (with 95%-confidence interval between 202,000 and 801,000), 800,000 fewer total deaths (with 95%-confidence interval between 190,000 and 1.4 million), 8.1 million fewer quality adjusted years of life (with 95%-confidence interval between 2 and 14.3 million), and 2.4 million fewer hospitalizations (with 95%-confidence interval between 0.7 and 4 million) according to the SIRD model, and around 184,000 fewer Covid-19 deaths (with 95%-confidence interval between 49,000 and 319,000), 303,000 fewer total deaths (with 95%-confidence interval between 45,000 and 560,000), 3.2 million fewer quality adjusted years of life (with 95%-confidence interval between 0.5 and 5.8 million), and 0.8 million fewer hospitalizations (with 95%-confidence interval between 0.2 and 1.3 million) in the linear model. Note that the aggregate effects for mortality (either measured with Covid-19 deaths, total deaths, or QALYs) and hospitalizations are larger according to the SIRD model than to the linear model. This is consistent with the fact that the transmission of the disease is influenced by other containment policies and people's voluntary behavioral changes in response to information about infection levels. If other mitigation policies respond to past infection levels, the coefficient on business closures derived from the standard linear specification may not capture the "direct causal" effects of business closures, but rather the "total effect" for the exposed group.

In Panel C, we first derive the monetary value of the health benefits of business closures for different assumptions for the value of a statistical life, and the value of a quality adjusted year of life. The empirical literature provides wide-ranging estimates for the value of a statistical life (VSL), depending on the method used. Ashenfelter and Greenstone (2004) found a low value of about $1.5 million using mandated speed limits, while Viscusi and Aldy (2003) presents values ranging between $5.5 and $7.5 million. We follow Adda (2016) and use values ranging between $1.3 and $6 million. To obtain a plausible range for the value of quality adjusted years of life, we follow Neumann et al. (2014) and report results for three different values: $50,000, $100,000, and $200,000.

Estimates vary across the model we consider (i.e. the SIRD or the linear model), the variables we use (i.e., Covid-19 deaths, total deaths, or quality adjusted years of life), and the assumptions we make for their monetary value. Yet, point estimates derived from the SIRD model range between $406 billion and $4,795 billion and therefore virtually always exceed estimates for total economic losses, which suggests that business closures might have been beneficial on average, even though we cannot statistical reject a net loss associated with business closures given the uncertainty surrounding the estimates. Estimates from the linear model are lower than that from the SIRD model: the point estimates for health benefits lie below total economic losses for the lowest assumed value of a statistical life ($1.3 million), or the lowest assumed value of a quality adjusted year of life ($50,000), but above total economic losses for the highest assumed value of a statistical life ($6 million), or the highest assumed value of a quality adjusted year of life ($200,000). In summary, under our preferred SIRD model and a reasonable range for the value or life, the health benefits of business closures might exceed their costs, but uncertainty around our estimates and results using linear specifications prevent us from statistically rejecting a net loss.

[32] For this, we replace β_{health} by the coefficient in column 6 of Online Appendix Table A.7 and use total employment instead of population in each cz to reflect the fact that the variable "Business closure" in the Household Pulse Survey applies to employees.

We then consider the monetary values of the health benefits and costs associated with hospitalizations and mental health disorders. From the literature, we draw three values for the cost of a Covid-19 related hospitalization: $19,900 from Ohsfeldt et al. (2021), $21,700 from Tsai et al. (2021), $24,500 from Di Fusco et al. (2021), and three values for the cost of a mental health disorder: $6,800 from Greenberg et al. (2021), as well as the lowest and highest estimates, respectively $1,900 and $24,000, in the meta-analysis of Coretti et al. (2019). Again, estimates vary across models and assumptions. Yet, we find that the benefits of reduced hospitalization, that lie between $15 and $58 billion on average, are of the same order of magnitude than the costs associated with increased mental health disorders, that lie between $11 and $142 on average. Furthermore, both are of a much smaller magnitude than the benefits of business closures in terms of reduced mortality. For both reasons, it is unlikely that hospitalizations and mental health disorders affect the aggregate comparisons between the total economic losses of business closures and the monetary value of reduced mortality.

9. Conclusion

Typical government responses to pandemics involve social distancing measures implemented to curb disease propagation. We explore the impact of state-mandated business closures in the context of the first wave of the Covid-19 in the U.S. We first show that these restrictions have a causal and negative impact on firms' profits. We then estimate the effects of business closure decisions on labor income, and on mortality rates, at the commuting-zone level. We find that a 10 percentage point increase in the share of restricted labor is associated with a drop by around 9% in wages, and a drop in monthly years of life lost, of around 16.8 years per 10,000 inhabitants.

An extrapolation of these findings suggests that state-mandated business closures might have cost around $475-$580 billion (with 95%-confidence intervals ranging from $185 billion to $900 billion), and that this cost was borne disproportionately by firms–through lower profits–, who partially insured their workers. We propose a cost-benefit analysis and discuss the assumptions under which the health benefits of business closures exceed their associated economic costs.

CRediT authorship contribution statement

Jean-Noël Barrot: Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis, Conceptualization. **Maxime Bonelli:** Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis, Conceptualization. **Basile Grassi:** Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis, Conceptualization. **Julien Sauvagnat:** Writing – review & editing, Writing – original draft, Methodology, Investigation, Formal analysis, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

The data that has been used is confidential.

Replication-BusinessClosures-BarrotBonelliGrassiSauvagnat (Original Data) (Mendeley Data)

Appendix A. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jfineco.2024.103794.

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